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OBSTETRIC
MEMOIRS AND CONTRIBUTIONS.

THE

OBSTETRIC MEMOIRS AND CONTRIBUTIONS

OF

JAMES Y. SIMPSON, M.D., F.R.S.E.

PROFESSOR OF MIDWIFERY IN THE UNIVERSITY OF EDINBURGH, ETC., ETC.

EDITED BY

W. O. PRIESTLEY, M.D., EDINBURGH,

FORMERLY VICE-PRESIDENT OF THE PARISIAN MEDICAL SOCIETY;

AND

HORATIO R. STORER, M.D., BOSTON, U. S.,

ONE OF THE PHYSICIANS TO THE BOSTON LYING-IN HOSPITAL.

MEMBERS OF THE MEDICO-CHIRURGICAL AND OBSTETRIC SOCIETIES OF EDINBURGH, ETC., ETC., ETC.

VOLUME I.

PHILADELPHIA:
J. B. LIPPINCOTT & CO.
1855.

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FRANCIS A. COUNTWAY
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DEDICATION.

TO DR. MAGNUS C. RETZIUS,

PROFESSOR OF MIDWIFERY IN THE UNIVERSITY OF STOCKHOLM, PHYSICIAN TO THE ROYAL FAMILY OF SWEDEN, ETC., ETC., ETC.

MY DEAR DR. RETZIUS:

As I have reserved the right of dedicating these volumes, I beg to have the pleasure of inscribing the following pages to you. And pray accept of the offering as a slight tribute, on my part, to your profound professional acquirements—as a mark of my sincere esteem for your personal friendship—and as a small memento of the delightful weeks which we spent together in the summer of 1852.

I feel deeply that I owe many apologies to you, and to my other professional brethren, for the publication of the Memoirs and Contributions of which these volumes consist, in so unusual and crude a form. Believe me, no one can be more sincerely impressed with their great and numerous imperfections, than I am myself. Most, if not all, of these Communications were, as you well know, written hurriedly and amid the incessant distractions of practice. If I had attempted to remodel, extend, and correct them, in the manner and to the amount which I have sometimes anxiously desired, they would, I fear, never have been published in a collected form. In this view, my friends and assistants, DR. PRIESTLEY and DR. STORER, were right, I believe, in urging the sheets through the press as rapidly as possible, after they had themselves arranged and annotated them. And for the great care and toil which they have expended upon the task, thus voluntarily assumed, I cannot possibly feel too grateful.

Allow me to add, that I sometimes venture to look forward with hope and pleasure to the possibility of the renewal of our personal intercourse. Nor, though far separated from you, are you ever allowed to be long absent from my thoughts, for one merry face around my hearth—that of your little namesake—is ever reminding me and mine of your happy sojourn among us.—Believe me, with heartfelt regards,

Ever faithfully yours,

J. Y. SIMPSON.

EDINBURGH, 16th April, 1855.

TO

DRS. JOHN WARE AND J. MASON WARREN, OF BOSTON,

THOMPSON, OF NORTHAMPTON,

AUSTIN FLINT, OF BUFFALO,

DUNGLISON, OF PHILADELPHIA,

KINLOCH AND JENKINS, OF SOUTH CAROLINA,
AND
MORISON, OF SAN FRANCISCO,

WHO AT VARIOUS TIMES DURING HIS RESIDENCE IN EDINBURGH, SHARED WITH
HIM THE ADVANTAGES OF DR. SIMPSON'S PRIVATE PRACTICE,

AND TO PRIESTLEY,

ASSOCIATED WITH EVERY PLEASANT REMINISCENCE,

THIS, THE AMERICAN EDITION,

IS INSCRIBED BY THEIR FRIEND,

THE JUNIOR EDITOR.

EDITORS' PREFACE.

THE greater portion of the following pages has appeared at various times in the British Journals of Medicine, as occasional contributions to Obstetric Pathology and Practice.

The object of the Editors has been to present these to the Profession in a more readily available form, believing that such compilations have at least the merit of greatly facilitating the study of any special inquiry,—an object of no slight importance to most Medical Men engaged in active practice.

In reprinting Dr. Simpson's Obstetric Memoirs and Essays, the Editors moreover believe that they will be the means of supplying many of his former pupils with papers they have vainly striven to possess themselves of, inasmuch as they were published originally in journals which are now out of print. Most of the Contributions thus scattered over many years and many volumes, while they do honor to the literature of the Profession, bear the impress of an almost unlimited practical knowledge and experience. The evidence of the latter appears to us especially to enhance their value, and it seems but justice to Dr. Simpson's acknowledged reputation as an Obstetrician, that some more tangible and permanent form should be given to his works.

Several of the Memoirs included in these volumes have been deemed of sufficient value, by our Continental brethren, to be translated into their own languages, and many of them have likewise been reprinted in American journals. Besides those already published, the present volumes contain a number of entirely new contributions, and Dr. Simpson has himself revised two or three of the older papers, which hence appear in an almost re-written form.

We believe that Dr. Simpson, after repeated requests from his professional friends and pupils, has only been prevented from, himself, republishing these Essays, and thus presenting them to the public in a more valuable form, by an impression that one or more of them must necessarily be finished, and many others elaborated and re-written; while the little leisure time at his disposal seemed entirely to preclude the accomplishment of so much extra labor.

We may, however, state that Dr. Simpson, after assenting to our request to allow these papers to be republished, has, whenever we needed it, given his cordial co-operation in our editorial labors; and that, while he is not responsible for the appearance of these volumes in their present form, he has most kindly placed at our disposal such of his papers as we desired, and has at all times been willing to assist us in any difficulty which presented itself during the preparation of the work.

The nature of the papers, from the mode of their original publication, was necessarily desultory, but we have endeavored to arrange them in different parts, according as they seemed to belong more especially to the Diseases of Women, Natural and Morbid Parturition, &c., &c. We have thought it better so to classify, than to arrange them chronologically, in order to give the book a more systematic form, and to facilitate reference; while it may thus be of more use to Students who are attending Dr. Simpson's Midwifery Class.

We have, at Dr. Simpson's special request, endeavored to avoid as much as possible entering into the controversies which have occurred on more than one occasion—always in self defence on his part—on the subject of some of the Memoirs here republished, and we have only extracted from such correspondence those portions which seemed to throw light on the subject under discussion. Our object has been to represent fairly the opinions of Dr. Simpson, written or spoken, on many important Obstetric matters; and we would refer our readers who may desire to see the controversial correspondences (more especially those with Drs. Lee and Radford on Placenta Prævia, and with Dr. Collins on Hospital Statistics, and the Effects of the Duration of Labor), to the Journals in which they were respectively published.

It will be found, we believe, that we have given *all* of Dr. Simpson's Obstetric writings, up to the present date (April, 1855), with but a single exception. We refer to a series of short articles on some of the most common diseases of women, written many years ago for the Library of Medicine. These are regarded by Dr. Simpson as of little value; and they would only have been a needless repetition of some of the pages in the first volume.

Many will ask, as has already been done, why we have not also published in the present work Dr. Simpson's more general Medical writings. Others will be surprised to learn, that a man not yet forty-four years of age, with such engrossing practice, and who has accomplished so much in his own peculiar field, should have yet found time for other subjects of thought.

We subjoin a list of the more important of Dr. Simpson's general Essays and Papers:

1. "The Contagiousness of Cholera."
Ed. Med. and Surg. Jour., April 1838.
Also a Paper containing further evidence. (Read to the Medico-Chir. Society, Dec. 13, 1848.)
Ed. Monthly Jour. of Med. Science, Feb. 1849.

2. "Antiquarian Notices of Leprosy and Leper Hospitals in Scotland and England." (Three papers read before Med. Chir. Society, March 3, 1841.)
Ed. Med. and Surg. Journal, Oct. 1841, and Jan. and April 1842.

3. "The Conduct and Duties of Young Physicians."
Edinburgh Graduation Address, 1842.

4. "Description of the Bones of the Distorted Foot of a Chinese Woman." (Read before Med. Chir. Soc., 4th June 1845.)
Northern Journal of Med., Aug. 1845.

5. "Solutions of Gun Cotton, Gutta Percha, and Caoutchouc, as dressings for wounds, &c." (Read before Med. Chir. Society, May, 1848.)
Ed. Monthly Journal of Med. Sc., July 1848.

6. "Ancient Roman Medicine Stamps." Three papers.
Ed. Monthly Journal of Med. Sc., Jan., March, and April 1851.

7. "Repudiation of Mesmerism."
Ed. Monthly Journ. of Med. Sc., May 1851, and Lancet, Sept. 1851.

8. "Was the Roman Army provided with Medical Officers?"
Printed and privately circulated, 1851.

9. "Notes on some Ancient Greek Medical Vases for containing Lykion; and on the modern use of the same drug in India."
Ed. Monthly Journ. of Med. Sc., Jan. 1853.

10. "Modern Advancement of Practical Medicine and Surgery." (Inaugural Address as President of Med. Chir. Society, 5th Jan. 1853.)
Ed. Monthly Journ. of Med. Sc., April 1853.

11. "Homœopathy; its Tenets and Tendencies."
Edinburgh, 1853. (3d Edition. Now out of print.)

These would of themselves have formed a large volume. We could not properly have embodied them with the papers we now present, to the collection and arrangement of which we have devoted several months.

For valuable aid in procuring papers out of print we are indebted to Drs. Malcolm and Wm. Ziegler, and to Messrs. Carmichael and Hunter Adam; and for opportunities of consulting the MSS. Records of the Obstetric Society, we are under obligations to its Secretary, Dr. Keiller.

52 Queen Street, Edinburgh,
10th April 1855.

PREFACE TO THE AMERICAN EDITION.

Without need of apology and without fear for its reception, the American edition of these papers is offered by the junior editor to his countrymen. Dr. Simpson is now probably better known throughout the United States than any other living obstetrician; the pages of our medical journals, for many years past, show with what avidity his opinions have ever been seized; the changes that have come over our practice, the extent and celerity of their adoption.

There are points of Dr. Simpson's practice, however, on which some are in doubt, and others, I believe, misinformed. The importance of these questions will warrant me in especially directing attention to several of them, and in claiming for them all a more extended and impartial investigation. No suggestion, I do believe, however impracticable or useless it might seem at first sight, ever came from this man's mind without being called forth by a felt want, being based on logical reasoning, and supported by experiment. In almost every instance thus far, the soundness and propriety of these suggestions have already been acknowledged; where such has not been done, it will be.

In 1844, Dr. Simpson proposed, in certain cases of placenta prævia, the extraction of the placenta before the child:

In 1845, the use of alkaline salts in placental phthisis:

In 1847, the operation of turning as an alternative for craniotomy:

And, in the same year, the use of anæsthetics in labor.

Three of these proposals, either of which were worth a lifetime to have made, were in turn received with ridicule and fierce attack. Within ten years these same proposals will have saved thousands of lives; and they now command the respect and assent of a large proportion of the profession. But yesterday, and the man who dared give ether or chloroform in labor was considered as breaking alike the laws of Nature and of God; the time is probably close at hand when such will be said of all who withhold them, even in natural labor.

In 1843, Dr. Simpson proposed the sound in uterine diagnosis:

And, in the same year, the treatment of displacements by intra-uterine pessaries.

In 1844, the incision of the cervix uteri in obstructive dysmenorrhœa:

And in 1849, the employment of sponge tents to expose intra-uterine polypi.

Regarding each of these latter proposals, many fears are still held, often openly expressed,—though upon two of them, the first and the last, the sense of the profession is doubtlessly now with Dr. Simpson. Without the sound, no man can satisfactorily diagnose some of the diseases of women; without the tent, though the sponge is very likely yet to give place to some safer agent, there are others that no man can cure.

Intra-uterine pessaries have, on the whole, been very unfairly dealt with. The very idea of manipulating an organ so important and apparently so sensitive as the uterus with such seeming heroism, was of itself enough to alarm very many honest and brave men, and to create an almost invincible prejudice. Such objections, though the subject was one somewhat more difficult of investigation, might yet have been answered by pointing to the successful and closely analogous use of the sound.

Other influences, however, were brought to bear upon this question,—influences which, in themselves, should never have been allowed for an instant to weigh, and never would have done so with an entirely unprejudiced mind. Of these I shall enumerate two,—the circumstances of the latter of which are in part already well known in this country. The other I shall allude to entirely without Dr. Simpson's knowledge, and, I may add, without his permission, which he utterly refused to grant when we asked it for the Scotch edition, so desirous was he that bygones should be past, and the wrongs he had received forgotten.

I should, however, be unmindful of my duty, as one of his editors, to Dr. Simpson's reputation, did I not now mention these facts. Abroad, where they are so well known, recapitulation would have been unnecessary; but to many here, the proposed practice is a new one.

Dr. Robert Lee of London, though himself a Scotchman, has for many years been Dr. Simpson's inveterate opponent, and has successively attacked every proposition of any importance he has made since 1840,—the date of their competing together for the chair Dr. Simpson now holds in the University of Edinburgh. To several of these attacks Dr. Simpson has replied at length; the remainder he has probably considered beneath his notice. Among others, Dr. Simpson's proposal of the intra-uterine pessary could not, of course, remain unnoticed, and on various occasions and in various connections, Dr. Lee has subjected it to unmeasured ridicule,* and at no time has he been at all temperate in his use of language. He has attacked Dr. Simpson at all times and seasons. Within even a year he has published in one of the London journals a most furious tirade upon anæsthetics in labor. He has written alike in his own name and anonymously;† and I think no one can systematically follow out the whole details of this warfare, as I have done, and still consider Dr. Lee an earnest physician laboring only for truth's sake. I regret to say this, and have done so with great reluctance. I know that it will give Dr. Simpson pain. But I must add that Dr. Lee's chief works on obstetrics, which formerly gained for him such reputation as a practitioner and as a writer, are now becoming considered entirely unreliable. His famous system of uterine nerves has been proved by Dr. Snow Beck, from the same identical specimens, not to exist; and the honors conferred upon him by one of the highest societies in Great Britain* have been formally and ignominiously cancelled.

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* London Med. Gazette, and Lancet, 1845–51.

† Vide papers by "Isaac Irons," loc. cit.

Objections to the intra-uterine pessary coming from such a source as this, and probably based on not a single truthful observation, should certainly gain no credence.

Again, a report has recently been submitted to the French Academy adverse to the employment of these pessaries, and has attracted much attention. To this report also I must warn the profession against lending too ready an ear. The opinions which led to it were many of them given by already and unduly biassed men; the cases experimented upon were many of them unfair and improper ones, and the instrument chiefly used, that of M. Valleix, was not at all that of Dr. Simpson, but an unwise, unjustifiable, and no doubt often dangerous modification. Judgment should be set aside, or at least withheld, and before long Dr. Simpson will no doubt himself answer all issues most satisfactorily.

In all cases these pessaries should perfectly *fit* the patient, or else there will of course be trouble: a man can no more make a single instrument answer for a series of patients than he could a single shoe; and I am satisfied that there are many cases which can no more be treated by any other form of pessary, than can the displacement itself be properly diagnosed by the speculum instead of by the finger or sound.

Incision of the cervix uteri by the hysterotome is an operation that, in experienced hands, and with proper care and precaution, is attended with little or no danger. In obstinate cases of dysmenorrhœa, that have undergone dilatation and all the other usual forms of treatment, it will usually be found productive of great relief,—often of complete cure.

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* The Royal Society of London.

In conclusion, we omitted these matters, as I have said, in the Edinburgh edition, and I have referred to them here, because in the one case they seem needed. In doing so, I have perhaps overstepped my bounds, and exchanged the coldness of an editor for the warmth—I trust still impartial—of a friend. If such is the case, I shall look for excuse to the peculiar relations in which I have stood to Dr. Simpson. For one year I was admitted to close intimacy with him—to his home. I learned to love him as my father; he treated me as a son. I am proud to call him master. In selecting an American as one of his editors, he testified to his regard for our country. I could only promise him a hearty welcome among us, and I am sure he will everywhere receive it.

H. R. S.

7 CHESTER STREET, BOSTON,
September, 1855.

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PART I.

SPECIAL PATHOLOGY
OF
THE UNIMPREGNATED FEMALE.

GENERAL REMARKS ON UTERINE DIAGNOSIS.

(From Edinburgh Monthly Journal of Medical Science, February 1851, p. 155.)

I SHALL begin these Lectures[1] with the consideration of the diagnosis of the diseases of the uterus and its appendages; and it is the more necessary in this respect, that, in studying these affections, you enter upon a field of practice in which the means of diagnosis are peculiar in various respects, in consequence of the anatomical and other peculiarities of the organs themselves, the diseases of which it is our object to investigate.

But you must permit me to make one or two preliminary remarks. And first of all, I would earnestly beg you to hold this important fact in view—namely, that the diseases of the uterus do not essentially differ, in their principles of pathology and treatment, from the diseases of other individual organs of the body. The uterus is liable, for example, to be the seat of congestion, inflammation, carcinoma, &c., like any other organ of the body which you can name. It may be, and certainly is, more liable than other organs to some morbid states and actions; but what I wish to impress upon you is, that its diseases are generically the same as the diseases of other organs; and the general principles of their treatment are also the same, the special modifications of management which they require not being greater than the special modifications that must be attended to in applying any general principles of cure to any other individual organ, or set of organs. You treat and cure, for example, an attack of acute inflammation of the uterus, or a chronic inflammatory ulcer of the cervix of the organ, as you would treat an acute inflammation or a chronic inflammatory ulcer of the cornea, or of the pharynx, or larynx, or of any other part of the body—with this difference, that you can apply local antiphlogistic and other curative measures much more readily and easily to the uterus than to many other more internal organs. In fact, you may, if you deem it necessary, apply lotions and dressings to an ulcer, for example, of the cervix uteri, with as much certainty and precision as if you applied them to an ulcer on the external surface of the body; you can thus almost reduce the organ, in relation to treatment, to the facilities presented in treatment by an external as compared with an internal organ.

---

[1] This lecture is the first of a series on Uterine Diagnosis, given annually by Dr. Simpson, when treating of the Diseases of Women. It is the only one of the course which he has published.—(ED.)

Modern pathology has made great advances in the management of uterine diseases. These advances, however, have not, let me repeat, consisted so much in finding out that the uterus is the seat of any new, or any peculiar affections, as by finding out means and methods by which we can detect and diagnosticate the affections that are actually present in it. Here, as elsewhere, after being enabled to discover what its diseases really are, the chief difficulty of the physician is removed; for, as I have stated to you, the same principles of treatment apply to the treatment of specific morbid actions in the uterus as apply to the treatment of the same specific morbid actions in other organs. Indeed, if you will attempt to analyze what the great progress in the medicine of modern times has actually consisted of, you will, I believe, find that it has consisted, not so much in the detection of any new principles of therapeutic treatment, as in the detection of appropriate means for enabling the practitioner to detect and discover in different individual organs what the actual diseased actions are, in those organs for which his aid may be required. The treatment, for instance, of inflammation, ulceration, and most other morbid states, was perfectly well known and established hundreds of years ago; but, to take our example from the uterus itself, it was not till of late years that we had certain means and measures of knowing when inflammation and when ulceration, &c., &c., were present in this organ.

The study and practice of medicine has confessedly made, within the last fifty years, enormous advances; but these advances have been advances in pathology and diagnosis, much more than advances in therapeutics. And yet, what we still wish and desiderate in actual practice are still greater advances in our diagnostic means; or, to state the same circumstances in other words, at the bedsides of our patients we find that it is in diagnostic means, and not in therapeutic resources, that our existing knowledge is most frequently deficient.

Every medical man is constantly meeting with difficulties in practice, and would often gladly avail himself of an opportunity of resolving these difficulties by appealing to the judgment of some older and more experienced practitioner. Ask the medical practitioner what the nature of the difficulties is which he most frequently encounters in practice. If he will properly analyze the subject, he will confess to you that his difficulties most usually consist in exactly determining what the special disease, or diseased action, in his patient may be; not in determining what he should employ for its cure, provided he were once perfectly certain of its precise nature, and precise seat and extent. The real difficulties of the practitioner do not commonly consist of doubts about the nature and details of the therapeutic measures which he ought to use; but they consist of primary doubts and uncertainties as to what the diseased action is, against which he is to turn his diversified therapeutic resources. They are doubts, not about therapeutics, but about diagnosis.

Since the diseases of the uterus and its appendages have of late years attracted so much more the attention of the profession than they formerly did, one grievous error has been committed in their study and investigation. It is an error which has been committed often in the study and investigation of the diseases of other organs, before we arrived at a sufficiently full and sufficiently comprehensive view of the morbid affections of these organs. The error I allude to is the error of exclusiveness. Formerly many practitioners seemed to look upon all diseases of the uterus as diseases indicative of debility; and they treated almost every one of them with muriate of iron and other chalybeate preparations; sometimes adding, where there was any discharge, the local employment of astringent or tonic injections. I fear that even yet you will find some old practitioners treating their uterine cases upon this sole principle. Then a second set of pathologists would, if we may judge from their writings, seem to suppose that all diseases of the uterus are marked by, if they do not consist of, congestion and engorgement of blood; and that they are to be remedied by the remedies applicable to this state. A third set, again, look upon the general run of uterine cases as almost invariably inflammatory in their nature, and imagine that we are sure, or almost sure, to find in every case inflammation, or some of its results; as ulceration, purulent discharges, &c. A fourth set would seem to fancy all uterine ailments to be produced by some mechanical displacement or dislocation of the uterus, and to consist of prolapsus, versions, and flexions of this organ upon itself, and upon the neighboring parts. Again, there are some practitioners—one, in particular, in immense practice in America—who would appear to believe that the affections of the uterus are fundamentally nervous or neuralgic disorders, and that they are always to be treated by the local inunction upon the cervix uteri, of morphia, aconite, and similar sedatives. Another section of pathologists imagine the so-called uterine diseases are, after all, not uterine, but ovarian; and that ovarian irritation and inflammation is actually the source and origin of much of the suffering that is imagined to be uterine in its seat. Lastly, for it is needless to extend this tedious enumeration, you will find another set enjoying the belief that these supposed uterine or ovarian diseases are not at all uterine or ovarian in their origin, but in reality diseases of the general system; they may not deny that local affections do occur in the uterus and ovaries, but these local affections are, in their opinion, results and effects of some more general constitutional disease of the nervous system, or of the economy at large.

All of these views are correct in one respect, that they are all founded partially, though only very partially, upon truth. For, no doubt, the uterus is in practice seen to be liable to one and to all of these morbid states. Thus it is liable to congestion, to inflammation and its consequences, to displacement, to neuralgia, &c.; and the ovary is liable also to ovaritis, &c. But all these opinions are excessively unsound and excessively wrong in another point of view, viz., from their having been in practice pushed by those who have adopted them, to such an extreme and exclusive length, as to amount in some instances to a practical fallacy and a practical error of the greatest possible magnitude. It shall be my earnest endeavor to prove to you that in relation to the uterine, as in relation to other organs, all such extreme and exclusive views are ever erroneous views. The diseases of the uterus are not always of one kind and of one character; they are of many different kinds and different characters, and require in consequence many different modes of treatment; and we should place ourselves under the most foolish and fatal fallacy, if we supposed that there was only one series of morbid actions liable to go on in the uterus and its appendages, and only one method of treatment applicable to all their diseased states.

No doubt all such errors in pathology and practice as I have just now adverted to, will gradually die away, as medical men become more intimately acquainted with uterine affections, with their pathology and treatment, and above all, as they become more intimate with the proper means of making a diagnosis of the affections of the internal reproductive organs in the human female. Certainly such errors will be best corrected in each of you, by each of you studying earnestly and carefully the means of diagnosticating the different morbid states and morbid actions to which these organs are subject. And, as I have already mentioned, it is my main object at present to point out to you by what different means the diagnosis of uterine and ovarian diseases may be effected, and to state to you the general principles of diagnosis which we can apply to their detection and discrimination. In doing so, I shall enter into no historical details; the knowledge I wish to impart to you is that kind of practical knowledge which we constantly require to use and apply at the bedside, and which we must be able to employ in the daily and hourly exercise of our profession.

In attempting to make a diagnosis of the diseases of the uterus and its appendages, as in attempting to make a diagnosis of the diseases of most internal organs, the physician calls in to his aid two sets of symptoms, viz.: first, the Rational, Dynamic, or Functional Symptoms, as they have been termed; and secondly, the Physical or Anatomical Signs and Symptoms. Let us first note the very marked and important difference which exists between these two sets of diagnostic phenomena—since we thus may have the

SYMPTOMS OF UTERINE DISEASE, EITHER 1. DYNAMIC, OR 2. PHYSICAL.

Under the Dynamic, Rational, or Functional Symptoms are included all those lesions, deviations, or derangements in the functions of the affected organ, in the functions of other organs that sympathize with it, and in the functions and state of the system generally, that can possibly be excited by the disease or diseases that we may happen to be investigating.

On the other hand, the Physical Signs or Symptoms consist of those means of diagnosis, and particularly of those exercises of the senses of touch, sight, hearing, &c., by which we can attain a knowledge of the anatomical structure, form, density, color, position, &c., &c., of the organ which is diseased. By this latter set of symptoms or signs—the Physical—we attempt, in fact, to read and discover within the living body the actual morbid anatomy of the organ or organs that are affected. We endeavor by them to study and to ascertain, as exactly as possible, upon the living patient, those anatomical lesions and changes which we should expect to detect on the post-mortem examination of that patient, provided the disease proved fatal.

In detecting and diagnosticating the diseases of the different organs and parts of the body, the practical medical man has recourse sometimes more to one of these sets of symptoms, sometimes more to the other. A diagnosis always becomes more and more accurate the more we can conjoin both means of investigation, and draw our deductions from both sources of evidence. Of the two sets, the rational or functional symptoms are the least to be depended upon; or, at all events, our diagnosis is always the less accurate, the less we can take advantage of the physical or anatomical signs.

Surgical diagnosis is, as a whole, more accurate than medical diagnosis. But why is the surgeon more accurate in the discrimination of the affections which he treats than the physician? Merely because he can, and does, found his diagnostic deductions far more upon physical symptoms; while the physician is of necessity driven to depend more upon the rational or dynamic symptoms. In fact, in making a diagnosis of the diseases submitted to his care, viz.: those placed upon the external surface of the body, the surgeon relies almost entirely upon physical diagnosis alone, because he can handle and examine the diseased parts by direct tactile examination, and by direct visual inspection. Deprive a surgeon of these means of physical diagnosis, and throw him back merely upon the functional or dynamic symptoms that may be present, without allowing him to exercise his sense of touch, or sight, or hearing, in the discrimination of the cases submitted to him, and you will find him as much or more at fault than the physician sometimes is. Take, for example, a case of any disease of the eye, leading on to imperfection and dimness of sight. No surgeon would venture to make a decided diagnosis of what that dimness or blindness was produced by, provided he were allowed to appeal to the dynamic or functional symptoms only. He might be told fully as to the mode and manner in which the blindness came on, as to its varieties under different circumstances, as to its being accompanied or not with pain or lachrymation, &c., &c.; but these and other circumstances would often be inadequate to enable him to decide whether the imperfection of vision arose from an affection of the cornea, or of the iris, or of the lens, or of the retina, or of some other parts; or by what particular affection of these parts it was produced; while an inspection of the diseased eye itself by the surgeon—that is to say, a glance at the physical symptoms or physical diagnosis—would probably enable him to resolve at once all these difficulties, and to ascertain determinately and unequivocally what the diseased state of the organ really was.

It is very different with the physician, in reference at least to some classes of affections. For in some he cannot avail himself of any of these measures of physical diagnosis which the surgeon finds to be so absolutely necessary to enable him to come to a correct decision in surgical cases. In affections for example, of the brain and spinal cord, and of their membranes and component parts, the physician can, with a few rare exceptions, make use only, in his diagnosis, of the dynamic or functional symptoms. It is only these symptoms that he can reach in this class of diseases. For we have no means as yet, and, in all probability, will never have any, in this special class of affections, of ascertaining, in the yet living subject, what the special anatomical lesions are, that may be present in the encephalon or in the spinal canal. We cannot reach and search these cavities and their contents by any modification of hearing, or touch, or sight. We can ascertain the morbid derangements that may take place in them only by studying the changes and derangement of function to which their presence gives rise in the nervous system generally, in the different parts of this system, and in the functions of those organs which are liable to be more directly or indirectly influenced by that system.

In other classes of disease, the physician, however, has fortunately the power of appealing to physical signs as well as to rational symptoms. He can derive his deductions of the diseased states of the organs themselves from both classes of proof; and you will always find that in medicine our practical knowledge of these diseases has progressed proportionally with the amount and extent to which physical diagnosis has been applied in their discrimination. There is no set of organs, for example, the pathology and practical knowledge of which has advanced more in modern times than that of the organs contained within the chest. If you look into any work on the affections of the heart or lungs, written thirty or forty years ago, you will find a strange contrast between its contents and those of any modern work written on the same subject. Formerly, physicians were often in error in regard to their cases, as to what this or that thoracic affection might be; and their true nature was often only accurately detected after death. The modern physician has almost always the power of discovering what the affections of the heart and lungs are, even during the life of the patient; and can trace, usually with great accuracy, their morbid anatomy while the patient is still alive. Mark what this difference between thoracic medicine, thirty or forty years ago, and that department of medicine now is to be attributed to. It is entirely attributable to the fact, that while in former times physicians were obliged and necessitated to attempt the diagnosis of these diseases by functional symptoms only, they have now the power of aiding and correcting that method of diagnosis by ascertaining a series of physical signs appertaining to almost every disease of the heart and lungs, through the instrumentality of auscultation and percussion. No careful or judicious physician depends now for the diagnosis of these chest affections upon functional symptoms only; but he appeals constantly, in aid of his diagnosis, to the more certain signs which may be derived from the pleximeter and the stethoscope.

I might safely repeat the same remark, and with nearly exactly the same degree of truth, with regard to the affections of the uterus and internal female organs. Formerly, and even in our own days still far too frequently, the physician has been led to suppose that he could make a diagnosis of diseases of the uterus by depending and relying upon functional symptoms only. A little practice will, however, I believe, readily convince you, that in most cases we are necessitated to have recourse to a physical diagnosis in order to obtain anything like an accurate knowledge of the diseases under which our patients are laboring. Do not suppose that, in making this remark, I mean to imply that the functional symptoms are of no value. Nothing could be farther from my belief and conviction. I by no means wish to state that they are of no value; all I mean to state is, that they are not of sufficient value to make a sufficient distinction between the different individual or special diseases of the uterus and uterine appendages.

And now having made these too diffuse introductory remarks, let me call your attention to what the *functional* and what the *physical* symptoms of the diseases of the uterus and ovaries really consist of. And first, let us consider

THE RATIONAL OR DYNAMIC SYMPTOMS OF UTERINE DISEASE.

The uterus performs at different times different functions. During menstruation its lining membrane forms and exudes the so-called menstrual fluid. In the intervals between menstruation we have merely a mucous secretion flowing from the same tissue. And again, it performs from time to time a variety of important functions connected with conception and pregnancy. Under different states of disease, one or more or all of these functions may become perverted. In other words, the first division of dynamical symptoms liable to occur in uterine disease consists of—

I. Derangements in the Functions and vital Condition of the Uterus itself.—Thus the function of menstruation may become irregular in regard to the time of its occurrence, or the duration of its appearance;—or the quantity of the menstrual fluid which is thrown off;—or the nature of that fluid may vary; and it may or it may not be accompanied by pain and suffering;—and pain, if present, may be constant, or it may be spasmodic or paroxysmal. The mucous secretion may be increased or diminished in its quantity or quality, or it may become occasionally or constantly mixed with blood or pus. The function of conception may be interfered with, so that sterility is the result; or the uterus may not have the power of carrying the fœtus beyond the second, fourth, or sixth month, the patient being subjected to a series of abortions or premature labors.

The natural states of sensation of the uterus and its appendages may also be altered and perverted. They may be decreased, but this is rare. Far more frequently they are increased to a degree amounting to actual pain; and the pain may appear under very different modifications. It may be continuous or intermittent; it may be dull and aching, or pulsative, or lancinating, and extremely severe. Often when it is present, it is accompanied with a feeling of heat and tension; and far more frequently still, with a feeling of weight and bearing down. Almost all these pains, resulting from diseases of the uterus, or its appendages, are diminished by the reclining position, and increased by two circumstances,—by a long continuance in the erect posture, and by the active and congested state of the organs, which accompanies the secretion and elimination of the catamenial discharge. Most of the so-called local pains of the uterus are not situated in the position of the uterus itself, or at least in the central part of the pelvis; but the patient more frequently complains of them as located in one or other of the sides of the pelvis; most generally in the left side, in consequence, no doubt, of the left side of the uterus and left broad ligament and ovary being more liable to be irritated than the right, by the varying condition of the rectum, which enters the pelvis towards the left; and the distensions and accumulations in this portion of the intestinal canal are certainly often the more immediate exciting causes of uterine and pelvic pains, if the uterus and its appendages are tender and predisposed to suffer pain, in consequence of any diseased condition existing in them.

At present it would be out of place for me to dwell upon the irregularities and deviations which we meet with in practice, in these functional symptoms of the uterus in uterine diseases generally, and in different cases of the same disease. But let me make this one remark as a caution to you, I shall insist upon it more afterwards, viz., that identically the same pathological affection does not always affect identically in the same way the functions of the uterus. You may have, for instance, fibrous tumors in the walls of the uterus, one of the most frequent organic diseases in this organ, producing, as local functional symptoms, increased menstruation, or menorrhagia;—increased mucous secretion, or leucorrhœa;—pain during menstruation, or dysmenorrhœa; and, in the intervals between the menstruations, constant uterine pains and bearing down, and irritation of the bladder and surrounding parts. But in other cases of exactly the same organic disease, more or fewer, or indeed all of these symptoms, may be entirely wanting.

Anatomists tell us that the cerebro-spinal nerves which come to the uterus are principally distributed to the cervix uteri;—and hence it has been considered by some pathologists, that the diseases of this part of the organ ought to be accompanied with pain, although the maladies of the body and fundus of the organ may not necessarily give rise to this symptom. But the results of practice by no means bear out this theory. The two most common affections of the cervix uteri are,—1. Chronic inflammation and its consequences —hypertrophy, ulceration, &c.; and, 2. Carcinoma of this part. In practice, cases of both of these diseases from time to time occur, in which, though the disease has progressed far, yet the patient has not been made aware of its presence by any increased tenderness or pain in the part, sufficient, at least, to attract her attention; and it is frequently very difficult to persuade the patient—nay, sometimes difficult to persuade the medical attendant—that such serious affections are present, because they incorrectly believe and argue that such affections could not exist without the accompaniments of local pain and suffering.

II. Dynamic Symptoms in other neighboring Pelvic Organs.— Dynamic symptoms often exist, in connection with uterine disease, not in the uterus or its appendages, but in the parts and organs in their more immediate vicinity. There may be pains, for example, about the bladder or rectum; about the coccyx or sacrum; in the groins; or along the crest of the ilium, and, what is exceedingly frequent, down the limbs, along the course of the crural or sciatic nerves. Pains running from the lower lumbar region, or from the groins, down along the limbs, are felt, and often felt severely, by females even during common menstruation; and intermittent pains in the lower part of the abdomen, or uterine colics, as they have been termed, are common under the same circumstances, as well as in different varieties of morbid uterine irritation. The functions of the rectum or bladder may be interfered with. Defecation may be difficult or painful. Often in uterine diseases there is constipation; and sometimes a want of power to expel the feculent matter, as if there was a kind of paralysis of the rectum; this last occurs not unfrequently in cases of retroversion of the unimpregnated uterus. In many cases the functions of the bladder are more or less affected. Micturition may be too frequent—a symptom that is common to many uterine diseases; or there may be great dysuria, as is often seen in cases of pelvic cellulitis, &c.; or there may be complete retention, as from the pressure of uterine tumors; or the result may be incontinence and impossibility of retaining the urine.

These various pelvic symptoms are, in some instances, mechanical in their pathology; in other instances they are sympathetic results. In other words, they are occasionally the direct result of the physical pressure of the enlarged or displaced uterus upon the rectum, or bladder, or trunks of the affected nerves. But, again, they may be present, and equally strongly marked, although the uterus is not unnatural in size, or unnatural in position; and, consequently, when the origin of these symptoms cannot be mechanical. In these last instances, and perhaps they are the more numerous class, we speak of the resulting disturbances of function and increased sensations, as sympathetic or reflex—that is, as originating in an irritation primarily seated in the uterus and uterine nerves, but terminating and felt, secondarily, in the sensitive nerves of those other neighboring parts which are affected.

This last remark leads me next to show you, that, in diseases of the uterus and its appendages, we have often the principal pains and sufferings of the patient not located in the uterus or in the pelvis itself, but located in other parts of the body; or we have a third series of dynamic symptoms, consisting of

III. Sympathetic Pains in different and distant parts of the Body.—This important class of reflex sympathetic pains or neuralgias is often so marked and severe as to draw away the attention of the patient, and even of the practitioner, from the real nature and seat of the original and primary malady.

Of late years, much important physiological and pathological information has been laid before the profession regarding what are termed reflex motions. You are all well acquainted with the nature and mechanism of the motions that are designated by that term. You all know that they are muscular movements created by excitation upon distant mucous and cutaneous surfaces, the impression made upon these surfaces being conveyed by a nervous arc, which arc always passes through the spinal cord, to those muscles that are ultimately set into contraction by it. The surface or part primarily irritated and excited is often, in this way, far removed from the muscles or sets of muscles secondarily called into action. I believe that often morbid sensations originate in the same way; and that you can speak as truly of reflex sensations as of reflex motions. In your surgical studies you have all studied one form of reflex sensations, viz., the severe morbid pain in the knee-joint, which is so frequently found to accompany the early stage of morbus coxarius. It is impossible to explain, upon any mechanical principle, why a patient, laboring under disease of the hip-joint, should have the principal pain in the knee-joint. It is not explicable by reference to any amount of swelling in or around the diseased hip-joint, compressing the trunks of those nerves that supply the knee. In fact, the sympathetic or reflex pain in the knee occurs most generally in the very commencement of the disease, when the lining membrane merely of the hip-joint is as yet affected, and when there is as yet little or no swelling or effusion. An impression, or excitation, is conveyed from this affected membrane by the nerves which are distributed to it, and this impression is probably conveyed by them upwards, as high as the spinal cord; and then another impression would seem to be reflected or conveyed downwards from the cord along another set of nerves, viz., along those the branches of which are ultimately distributed to the knee-joint, the immediate seat of the so-called sympathetic pain. Or, perhaps, we may state the matter more correctly, if we say, that the trunk of the nerve going to the knee, has a morbid sensitive impression made upon it somewhere during its course, as it passes through one of its higher plexuses, or at its origin in the cord itself, producing a change in it, as if its own branches in the knee were painfully affected. Irritation of the mucous membrane of the bladder, to take another example, often gives rise to sympathetic or reflex pains in the orifice of the urethra, and along the course of the lower extremities. I have seen several cases where the small, red, granular, and very painful sensitive tumor which sometimes grows at the orifice of the female urethra, was accompanied with distressing pain in the heel and sole of the foot. But it is needless to go to other organs in search of such examples; the uterus itself affords us many marked instances, even when it is not diseased. Thus, when the uterus is distended and irritated in pregnancy, the patient often complains of reflex or sympathetic pains in the lumbar region, in the mammæ, in the nerves of the teeth, &c. When the uterus is the seat of actual disease, such reflex pains are, as I have just now stated, often very marked; and, at the same time, they are very apt to deceive and mask the original and primary disease. Some years ago, I saw a case, with Dr. Johnston of Berwick, which made a strong impression upon my mind, in relation to this question. The lady had been under the care of an eminent London accoucheur, and complained to him principally of pain in the mammæ, for which he had ordered, at different times, leeches, and a sufficiently varied course of sedative local applications, and numerous internal remedies. But he had not once examined into the condition of the uterus, which was the source of the sympathetic pain in the mammæ. She was suffering, in fact, not from any diseased state of the mamma, the organ which the physician was so assiduously treating, but she was suffering under a slow corroding carcinoma of the cervix uteri. At the time I saw her, the disease had already destroyed almost the whole tissue of the cervix, leaving the remainder of its excavated walls hard, rough, and indurated like the interior of a dice-box. The dependence of the reflex or sympathetic pains seen in uterine cases upon the uterine irritation itself, as their primary source, is best seen in cases of displacement. By restoring a displaced uterus to its natural position, even though only temporarily, you generally at once moderate or entirely remove the attendant sympathetic effects. We cannot have the same precise proof in other uterine diseases, as ulcer, cancer, &c., because we cannot make the experiment of at once removing the primary irritation.

The reflex or sympathetic pains under which patients suffering from uterine diseases are liable to complain, may be situated in very different parts of the body. I shall enumerate the principal of them, and that, as far as possible, in the order of their frequency.

Pain in one or both mammæ, such as occurred in the case I have just now alluded to, is very common in many forms of uterine irritation or disease; but is seldom so very severe as it was in that instance. It is often accompanied by some degree of swelling or tension in the gland; and occasionally the areola becomes somewhat darker and changed, as in pregnancy. These mammary symptoms are particularly liable to be increased at the period of menstruation. If a patient complains to you of pain in this part of the body, without your being able to trace any local affection of the mamma itself capable of creating it, always make the necessary inquiries and investigations, in order to know whether some congestive, or inflammatory, or other diseased action, is not going on in the uterus.

Pain under the left mamma and upon the edges of the ribs on that side, is also very frequent, perhaps even more so than pain in the mamma itself. It has sometimes appeared to me to be more common, in cases of uterine or ovarian disease, occurring in the unmarried than in the married. It is probably as frequent in cases of uterine affections, as pain in the shoulder is in cases of hepatic affections. This pain under the left mamma is sometimes diffused along the side; but more usually it is limited, and the patient will tell you that she thinks she could cover the whole of the pained spot with a crown piece. When it is not severe, it is not affected by the act of respiration; but in more severe cases, it is increased by deep inspiration; and many a poor patient has been bled and leeched, &c., over and over again, under the idea that this pain was an indication of pleuritic inflammation, when in fact, it had nothing to do with pleurisy, or any disease of the chest; but was merely a sympathetic or reflex pain, and one entirely resulting from uterine irritation.

Pains in the right side, or right hypochondriac region, are much more rare, but are still sometimes also to be met with.

Pain in some of the vertebræ of the back, imitating spinal irritation, is often also an accompaniment of uterine irritation, and is not unfrequently combined with the existence of the pain under the left mamma. The patient principally complains of it when the part is pressed. Many a patient suffering from these sympathetic vertebral pains, in consequence of uterine derangement or disease, has been laid up and counter-irritated with the cautery, moxa, &c., under the idea that there was actual spinal disease present.

Pain in the lumbar region, or in the sacral and lumbar region combined, is especially common, as a sympathetic morbid sensation in uterine diseases, but particularly in those which consist of any displacement or enlargement of the organ. The great sacro-lumbar pain which the woman in labor suffers in the same part, is itself entirely a reflex or sympathetic pain of the character that we are speaking of.

Pains in the abdominal parietes also occasionally occur. Sometimes they are limited to one or other side, occupying the site of the ascending or of the descending colon. Occasionally they are confined to a limited spot. But in other instances we find the whole surface of the abdominal walls tender and neuralgic. And when this is the case, there is often coexisting a greater or less degree also of tympanitis—a kind of combination which is very apt to deceive the young practitioner into the idea, that the disease of his patient is really more formidable than it actually is. For the combination may lead him to suppose, that he has to do with a case of acute peritonitis, or, if it is of longer standing, with a case of serious enlargement and organic disease of the uterus, or ovary, or some abdominal organ. In some cases, the apparent tenderness is so great that the patient timidly shrinks under the least touch, and is even terrified at the approach and pressure of the hand of the practitioner.

Pains in the extremities and joints are sometimes secondary morbid results when the primary morbid irritation or disease is seated in the uterus. I have already stated to you that pain, stretching from the loins or pelvis downwards, along one or both of the lower extremities, is a very common symptom indeed in uterine diseases. In ovarian enlargement there is sometimes, particularly in the earlier stages of the disease, a morbid sensation of numbness and prickling in the corresponding limb; but this effect is probably mechanical, the consequence, that is to say, of pressure upon the nerves of the limb, and not true reflex or sympathetic results. I had occasion also to mention the occasional excitation of reflex morbid pain in the heel and foot, by the small sensitive vascular tumor that occasionally grows in and around the orifice of the female urethra. Other forms of irritation in the urinary canals may excite morbid pains in the same distant part; and I have met with them also, but more rarely, in uterine cases, in which I could not trace any primary pathological disease in the kidneys, bladder, or urethra. Under the name of hysterical knee-joint, surgical authors describe a painful affection of that joint, occasionally accompanied by swelling and apparent effusion, and which has often been mistaken for the common white swelling, or scrofulous disease, of the same part of the body. The pain and other symptoms are so chronic and so marked, as sometimes to have led the surgeon to amputate the limb for supposed incurable disease of the joint. I feel well assured that in these cases the pain in the knee is almost always, or indeed, always, a secondary or reflex pain, and that the primary source of irritation will be found in some other distant organ, and very often, indeed, in the uterus or ovary. Similar pains in the other joints of the extremities seem rarer; but are not altogether unknown. At present I have a patient suffering under chronic inflammatory ulceration of the cervix uteri, and profuse purulent discharge from that part, with much pain and tenderness in the canal of the vagina. But she is suffering still more from severe pain and tenderness of the skin and tissues lying over the head of the right radius. This pain in the arm has sometimes been so excessive as to deprive her of rest, and she herself maintains that it decreases and increases in correspondence with the changing state of the uterine irritation and discharge.

Pains in the face and head may also occur. I have already stated to you, that toothache, as a sympathetic pain, occurs occasionally in patients in the state of pregnancy. I have not had occasion to trace it distinctly, as a mere reflex and sympathetic pain excited by disease of the uterus. Local and limited pains, as over one or other eyebrow, in the forehead, in the occiput, in the temple (clavus hystericus), &c., have appeared to me sometimes to be the secondary consequences of uterine irritation. The headaches, however, under which uterine patients so often suffer, are not generally direct secondary sympathetic effects of uterine affection. The cephalalgia in these cases is almost always a kind of indirect result either of the anæmia, or of the deranged digestion and assimilation, which so often come to attend upon chronic uterine affections. For uterine diseases frequently produce other deviations in the functions of the body, than mere painful sensations. They often derange the functions performed by different organs. Let me state to you what phenomena, as rational or dynamic symptoms of uterine disease, we find most frequently referable to such.

IV. Derangements of Functions in distant Organs.—We have already considered the *urinary bladder* as liable to be irritated and deranged in its function in uterine disease—sometimes mechanically, but very frequently sympathetically. Let me merely add on this point, that occasionally the urinary secretion is altered and very various in its character. The irritation of pregnancy is well known to produce changes in the urinary secretion, which have been considered as quite characteristic of pregnancy; sometimes the morbid secretion of albuminous urine occurs; and one or two rare cases have been recorded, in which women have suffered from diabetes when pregnant, and have recovered their usual health completely after delivery. It may yet be found that uterine diseases are also liable occasionally to derange and alter the urinary secretion itself; but the subject, as a source of any rational symptoms of uterine disease, has hitherto attracted no degree of attention. Uterine patients, when questioned on the matter, tell you generally, in relation to it, that the urinary secretion is most variable, and especially that it alters, from time to time, from being very profuse and watery, to being small in quantity and loaded with sediment. This sediment is usually composed of the urates; but I have seen the triple phosphate, oxalates, and other morbid ingredients in the urine, in uterine cases. I am not acquainted, however, with a sufficient number of facts to be able to state to you whether these occurrences are mere coincidences, or whether, in any case, the derangement in the urinary secretion, and the derangement in the morbid state of the uterus, stand in relation to each other in any respect as cause and effect.

The *intestinal canal* is an organ more frequently deranged in its functions in uterine disease than perhaps any other organ of the body. Usually the functions of it are performed sluggishly and inertly. Constipation is very often present; and the lower portions of the canal especially appear to have lost their healthy tone and power. At the second or third month of pregnancy the abdomen of the human female is often larger than it is when the uterus is actually still more enlarged at the fourth or fifth month. This early enlargement is the result of the derangement of the functions of the bowels, and more particularly of the accumulation of gases within them. In the same way a partial tympanitis often accompanies other morbid irritations of the uterus, as well as the irritation of it from pregnancy. Pregnancy is, as you are all aware, not unfrequently accompanied with dyspepsia in the form of gastrodynia, pyrosis, and other morbid symptoms; and nausea and vomiting are very common sympathetic phenomena in the pregnant state. All these symptoms occur also in connection with diseases of the uterus; though more rarely than with pregnancy.

Further, in uterine disease we find another abdominal organ not unfrequently sympathetically affected—I mean the *liver*. In very many women the biliary secretion becomes disordered at the return of each menstruation—in some a state of constipation, in others a state of diarrhœa, recurring during each menstrual period. The biliary and catamenial secretions seem almost vicarious of each other; and, as in other cases in which such physiological relations exist, the two functions are not unfrequently simultaneously deranged in their pathological actions also—both being occasionally increased or decreased together; or, what oftener happens, one being increased in extent and activity when the other is diminished. In some cases the cure of a uterine disease seems also to rectify the coexistent, and perhaps resultant, hepatic derangement; while, no doubt, also, in other cases we find ourselves altogether unable to amend and arrest uterine diseases and discharges, till we have, in the first instance, used appropriate means to modify and correct the attendant hepatic disorder.

The *organs within the chest* are more rarely affected. Often, indeed, there is palpitation present, but it is principally in cases where there has been a loss of blood from uterine disease, or a constant and profuse leucorrhœal discharge, weakening and debilitating the patient; or when a deranged state of the digestive functions has led on to the same result. Besides in prolonged uterine disease, we very often find the mobility of the nervous system greatly increased; and palpitation in all such patients occurs readily under any excitement, and sometimes forms a sufficiently distressing, and, in their own opinion, an alarming symptom.

At first thought, one would scarcely expect to find the respiratory function ever affected sympathetically in uterine disease. I have, however, seen a sufficient number of cases, fully to convince me that there is in reality a true kind of *asthma uterinum*, as mentioned by the older nosologists. I saw, for example, within the last few days, a lady whose history is briefly this:—She never menstruated till she was twenty-eight years of age, but every month, or at the period of the menstrual molimen, there was, instead of menstruation, a distinct attack of asthma, lasting generally for two or three days, and sadly shattering and breaking down her constitution. Her state greatly alarmed her brother, who was himself a medical man.

I attempted, during several successive months, to induce menstruation, by applying at the expected periods, nitrate of silver freely to the interior of the uterus, by the usual instrument adapted to that purpose. Some sanguineous discharge was the result, with marked and evident alleviation of the asthmatic symptoms. At last, I introduced a small galvanic pessary into the interior of the uterine cavity, and left it there for several months. After wearing it for a time, the patient menstruated regularly, the asthma entirely disappeared, her health and strength became restored, and, for the first time for many long years, she was able again to join in general society. Menstruation continued regular for some months after the instrument was removed, and the asthma was kept in perfect abeyançe; but, on the recurrence of cold weather, she again failed to menstruate, and, instead of it, had one of her old attacks of asthma. At her own earnest suggestion and solicitation, I have, in consequence, again replaced the pessary, to which she herself correctly attributes the new life, as she describes it, which she has of late years gained. Perhaps such a result may appear not so paradoxical as at first sight it might look, when we remember that several German physiologists have insisted that menstruation is a kind of vicarious respiration, intended to rid the female system of a cumulative amount of carbon. And you know that Andral and Gavarret have proved experimentally, a few years ago, that in the human female there is a kind of direct relation and interchange between the amount of carbon discharged in expiration, and the amount of menstruation present; more carbon being always exhaled from the lungs when the patient is suffering under amenorrhœa, or when menstruation is arrested by pregnancy, than when she is not pregnant, and when she is menstruating regularly.[1]

The *nervous system*, as I have just now had occasion to state to you, becomes weakened or mobile and supersensitive in most patients suffering under any protracted, and especially under any weakening form of uterine disease. There is languor, and at the same time excitability of mind observable to a distressing degree in many such patients. There is not the usual capacity for mental exertion. Not a few are morbidly despondent of themselves and of their state. We see, sometimes, sad evidence of the effects of uterine irritation upon the mind and nervous system, in the production of puerperal insanity. In all large asylums you will find cases of insanity combined with, if not resulting from, uterine irritation and disease; and in such institutions the paroxysms of mental excitement are very often seen to be regulated by the monthly recurring irritation in the uterus which exists during the days of menstruation. Occasionally, in cases of uterine disease, there are lesions of the senses, more particularly of the sense of sight—sometimes the vision being impaired, and motes floating before the eyes. The whole muscular system, under such a condition, sometimes becomes debilitated and reduced in tone, the patient being unable to take anything like her usual amount of exercise. You have the same class of patients, in whom the nervous system is thus weakened and exhausted, occasionally complaining of prickling and numbness of the extremities, as if these parts were losing their power of motion and sensation. And local paralyses seem sometimes, though very rarely, to occur from the irritation of uterine disease, in the same way as they are seen to occur from the irritation of pregnancy. The power of standing and walking is sometimes weakened and abrogated from another cause in uterine ailments, viz., from the uterus being so much enlarged or displaced, as to produce compression upon the nerves, &c., in the interior of the pelvis. I have seen several cases where this power was restored to the lower extremities, by restoring to its position a uterus that was retroverted.

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[1] Dr. Simpson, on the 17th of January, 1855, further directed the attention of the Obstetric Society of Edinburgh to the periodical attacks of asthma occurring in cases of amenorrhœa He reported several instances which he had observed since writing the above, and reiterated his conviction, that were the subject properly investigated, the importance of the connection would become evident.—(Ed.)

In pregnancy, the *cutaneous surface* is sometimes affected sympathetically, ephelis and other eruptions appearing upon it. In uterine disease the skin is often dry and parched, and in an unhealthy state; and the cutaneous surface of the hands and feet is particularly liable to be easily chilled, perhaps from imperfect capillary circulation, or from imperfect innervation. I have under my care at present, in private practice, two cases of amenorrhœa in both of which the face is thickly covered with acne rosacea. In one of these the cutaneous eruption has become more than once greatly diminished when the menstrual discharge was restored, by the application of nitrate of silver and dry cupping to the interior of the uterus—so much diminished, as to impress my mind with the belief, that the eruption itself was a derangement in some way or other the direct or indirect result of the uterine disorder. Some women have, at each menstrual period, a slight erythematic appearance upon the skin of the face.

Some time ago I showed Dr. Christison an interesting case, in which there was amenorrhœa coexisting with such a profuse degree of *chronic salivation and ulceration of the lips and gums*, as at first to have impressed me, as it had impressed other practitioners, with the idea, that she was suffering under the effects of mercurial salivation. My inquiries, however, regarding her, quite satisfied me that she could not possibly have been taking any mercury. In what relation the amenorrhœa and salivation stood to each other, in this case, as in other cases, I cannot venture to determine. But you all know that in some women salivation is a very common dynamical symptom in pregnancy. The pregnant patient sometimes loses, in this way, large quantities of saliva daily, up to the very period of delivery.

The *thyroid gland*, in occasional and rare cases, enlarges in pregnancy. I have seen it enlarged also in uterine cases; but in this, as in others of these complications that I have named, we yet certainly want sufficient evidence to determine the actual relation between these functional derangements and the coexisting uterine disease and irritation. We have yet to study and learn, how far they stand related to each other as simple coincidences, or as regular sequences. And a more careful and complete investigation of them by pathologists would probably add considerably to the amount and value of the rational or dynamic symptoms of uterine disease.

In reference to all these morbid pains and morbid derangements of function, which may thus in different cases accompany, and be produced by, derangement or disease of the uterus or ovaries, let me make one more general remark, viz., that they are all liable to be more or less increased in character and in intensity at the menstrual period, when the uterus and ovaries become congested during the days of the usual menstrual molimen.

Lastly, among the dynamic symptoms produced by uterine disease, I have yet to state to you that these affections sometimes produce, and are accompanied by,

V. STATES OF GENERAL CONSTITUTIONAL DERANGEMENT.—The states of general constitutional derangement that may be found attendant upon uterine disease vary with the disease itself.

The inflammatory diseases of the uterus are seldom so acute, except in the puerperal state, as to lead to much secondary fever; but inflammation in the cellular tissue of the pelvis, or Pelvic Cellulitis, frequently leads to much constitutional disturbance; and if the disease is prolonged, and ends, as it often does, in suppuration, then you have very well-marked hectic symptoms usually supervening. I have more than once seen the hectic accompanying a pelvic abscess, mistaken for the hectic fever of phthisis.

Where there is much and continued loss of blood from polypus, or cancer, or other causes, you have the results and general effects of anæmia and chlorosis superinduced, with their usual accompaniments of headache, palpitation, nervousness, &c.

But you have often chlorosis supervening, apparently parodoxically, under the very opposite set of circumstances, viz., where there is amenorrhœa, and partial or total suppression of the catamenial discharge.

In carcinoma of the uterus, the patient, after the disease begins to break up, usually exhibits the sallow, yellow complexion of organic disease; but it is generally mixed up also with pallid lips, gums, and other symptoms of anæmia, in consequence of the frequent violence of the accompanying hemorrhages. In most such instances, and in the advanced stages of ovarian dropsy, the general atrophy and emaciation of the patient become at last extremely striking.

But in a large proportion of uterine ailments, especially in those of the slighter description, little, or, indeed, no appreciable amount of constitutional disturbance is produced. Let me merely further add, that when the disease is attended with exhaustion, or if the irritation of it has excited and weakened the nervous system, so as to render it supersensitive and impressible, hysteria in one or other of its multifarious forms is always apt to supervene.

We come now to another question,—one I have already slightly alluded to, viz.:

WHAT IS THE DIAGNOSTIC VALUE OF THESE VARIOUS FORMS OF DYNAMIC OR RATIONAL SYMPTOMS, IN THE DETECTION AND DISCRIMINATION OF UTERINE DISEASES?

This question is in itself a very important one, and I think we can answer it satisfactorily and decidedly. For I feel assured that no man, who is practically acquainted with the diseases of the uterus, can have any hesitation in declaring, that the dynamic symptoms of which I have spoken, taken either individually or conjointly, are in the general run of such cases, altogether incompetent and inadequate for the purpose of a perfectly correct and practical diagnosis of the different or specific affections, to which the uterus and uterine appendages are liable. But do not mistake me in what I wish to state to you upon this point.

There are two objects of diagnosis which the medical man is to consider, in relation to every case applying to him for relief, viz., first, What organ of the body is affected? and secondly, How is that organ affected? In other words, his first duty is, to trace out the seat of the disease, or the organ or organs which are diseased; and his second object is, to determine what special or particular affection of that organ or organs is present in his patient. Now, I do believe that the dynamic symptoms of uterine disease, such as I have described them to you, may enable us in most cases to determine that the uterus is affected. They are, in many cases at least, sufficient to decide us in relation to the first point that I have named; but they are perfectly insufficient, by their own unassisted evidence, to enable us to come to a decision in regard to the second point, viz., What is the special disease of the uterus, or uterine appendages, under which our patient is suffering? They generally afford us proof enough of the probability of the uterus being affected, without proving to us how it is affected. They may enable us to decide that it is the seat of some morbid state, or some morbid action; without enabling us to determine at all exactly what that morbid state, or that morbid action, may be; and, consequently, without enabling us to decide what should be the proper line of treatment requisite for the relief of our patient.

In treating of this important point—in endeavoring to show that the dynamic symptoms are not sufficient to enable a practitioner to make a differential diagnosis among the different diseases of the uterus—I have often found it difficult to impress deeply enough upon the mind of the student the fact, that there is no practical guiding relation between the kind or amount of uterine disease that may be present, and the character of the secondary dynamic symptoms to which it gives rise; and hence, that he cannot, in practice, depend for the discrimination of the different diseases of the uterus from each other upon the dynamical or functional symptoms. But I think I can adduce in evidence, a case, with the peculiarities of which you are all sufficiently familiar to enable you to understand the argument, in relation to this question, that may be derivable from it.

The rapidly dilating and developing condition of the uterus during pregnancy, and the state of irritation produced by the ovum enlarging within it, may be regarded as the same condition in all women. Pregnancy is the same disease, if I may so term it, in all. In all cases the vital changes and actions going on in the walls and in the cavity of the uterus are identically the same. Yet mark how very different are the dynamic symptoms accompanying this same condition in different patients. Some patients during pregnancy are long unaware of the condition in which they actually are; they suffer from no kind of complaint—from no form of distress—no functional derangement, or dynamic symptom. A second set of patients during pregnancy are afflicted with severe local suffering, from irritation, induced by the enlarged organ, in the bladder and the rectum; or they have a feeling of prolapsus and bearing down of the uterus, with pains in the loins, &c. You will find another and a third series, not complaining of any local feeling or distress, but suffering under sickness and nausea—under general derangement of the stomach and intestinal canal—under headache and nervous affections—under toothache or salivation, and other derangements of the sensations and secretions of the body. But, again, in another and in a fourth set of cases, we occasionally find all the most frequent dynamic symptoms, local and constitutional, of pregnancy present, and so well marked, as to impress both the patient, and sometimes even the practitioner, with the certainty of the existence of that state; and yet, after the lapse of a little time, it may be found that the patient is really not pregnant at all; the symptoms being sympathetic merely of some morbid state of the uterus or ovaries, but not sympathetic of the state of pregnancy, though very exactly similar to the symptoms which most commonly accompany it.

Observe, for a moment, what important evidence these well-known facts afford. They afford us evidence to this extent—first, that in pregnancy we have the same identical condition of the uterus, not by any means always accompanied by the same identical dynamical symptoms, these symptoms varying greatly in different patients; and, secondly, they show us that the most usual dynamic symptoms—local, sympathetic, and constitutional—which we see produced by pregnancy, may be present in other conditions of the uterus than in the pregnant condition. But exactly the same two conclusions are true in regard to almost every one of the diseases of the uterus, as well as to its state of pregnancy. In different diseased conditions of the uterus, as in the condition of it in pregnancy, we constantly find the same specific affection of the organ exciting very different phenomena in different women. And, further, we find that apparently, phenomena, or dynamic symptoms, which are nearly if not entirely identical, may be excited by two or more diseases of the uterus that are entirely different from each other in their pathology and in their treatment.

I have already alluded to the frequency of fibrous tumors in the walls of the uterus. Let me take them as an example of the remark—though, in fact, uterine pathology is full of similar instances. Fibrous tumors do not, in some patients, produce any very decided dynamic symptoms; and even when of large size, occasionally they may pass for years altogether undiscovered either by the patient or by the practitioner. Menstruation may be perfectly regular; and pregnancy even may take place. In other patients, however, these fibrous tumors, even when still small in size, sometimes, as I have already stated to you, produce distressing irritation among the pelvic viscera, and derange in various ways the physiological functions of the organ, leading on to menorrhagia, leucorrhœa, dysmenorrhœa, &c. Here, then, we have a disease identical in character in different patients, but producing in these different patients very different kinds of symptoms. And, if it were necessary, I might repeat the same kind of deduction, as derivable from the consideration of the different symptoms attending all other organic diseases of the uterus in different patients, as chronic inflammation, hypertrophy, polypus, cauliflower excrescence, carcinoma, &c. &c. In many cases carcinoma of the uterus produces, from a very early stage of its progress, distressing dynamic symptoms. But in others it marches onward in its fatal course for a long time, without occasioning almost a single dynamic symptom calculated to excite the attention or rouse the alarm of the patient; and when marked dynamic symptoms do at last supervene, how commonly are they mistaken for the symptoms of other and milder diseases of the uterus. I have often, on the other hand, seen cases which have been supposed to be cases of incurable carcinoma, when the disease was some more benign and perfectly curable form of uterine malady, such as inflammatory induration of the cervix, or polypus. In most, if not in all such cases, the error committed has consisted in the practitioner supposing he could make out the differential diagnosis of the disease by dynamic symptoms alone, and without calling in to his aid the evidence to be derived from physical diagnosis.

The most malignant organic diseases of the uterus may, as I have just remarked in relation to carcinoma, long remain occult and latent in their character; they may have marched far on to a fatal termination, without a single dynamic symptom being present calculated to warn the patient to any knowledge of her state of danger. But, on the contrary, we have often severe local and constitutional dynamic symptoms of uterine disease developed, and developed early, in instances of slight and remedial organic affections of the organs, as in simple chronic ulceration, and inflammatory eruptions upon the cervix. And again—mark this other perplexing fact—in other instances, all, or almost all, these dynamic symptoms may be present, in their most aggravated forms, for months, and even for years, in instances of the so-called irritable uterus, or in neuralgia of the organ—that is to say, in a set of cases where there is actually no organic disease at all, and where there may be much real distress, but not much real danger.

To sum up these remarks in one more general deduction, let me observe, that we sometimes have the same apparent train of local and secondary dynamic symptoms in neuralgic, in inflammatory, and in benign and malignant organic diseases of the uterus. And, in whatever way we may explain the fact, it is still an undoubted important pathological and practical fact, that, in uterine diseases, there is generally no very direct or fixed relation observable between the intensity and character of the local pathological disease which is present, and the intensity and character of the dynamic symptoms to which that disease gives rise. The accompanying dynamic symptoms may be severe, where the local disease of the uterus is slight and simple; and, on the other hand, it too frequently happens that the local disease of the uterus is severe and serious in its nature, when the dynamic symptoms that accompany it are apparently trivial and transient in their character. It is in consequence of this want of relation between uterine diseases and their dynamic symptoms, that it becomes so requisite in this class of affections to have recourse to physical diagnosis, in order to determine what ascertainable changes have taken place in the structure and organization of the uterus or its appendages. It is only by instituting, in this way, a physical diagnosis that you can hope, in most instances, to decide upon the specific nature of the uterine disease that may be present. In many cases we may make, as I have already repeatedly stated to you, the general diagnosis of the existence of uterine disease, by the consideration alone of the rational symptoms or functional derangements, to which such diseases give rise. We can only make a differential diagnosis of what the existing specific disease is by physical diagnosis; or, in other words, by the physical examination of the anatomical conditions of the organ itself.

The necessity of physical diagnosis will, I believe, be willingly confessed by all practitioners in relation to the diagnosis of one uterine state, that is not a state of disease. I specially allude to the illustration which I have already brought before you, viz.: the state of pregnancy. In deciding upon the existence or non-existence of pregnancy in any cases of doubt or importance, no medical man, who valued his professional reputation, would deem himself justified in offering a final and decided opinion upon the subject, by considering the dynamical symptoms alone—symptoms, which I have already described to you to be sufficiently varying and equivocal in different patients. No medical man would venture to form a definite deduction and judgment upon the matter until he was allowed to make some physical diagnosis of the condition of the uterus,—that is, until he had ascertained by sight or touch, or both, that the abdomen was really enlarged; until, probably, by a careful external examination of the abdomen by the hand and stethoscope, he had ascertained that the existing tumor was really uterine, and really contained a fœtus; or, in the earlier months, until, perhaps, he was permitted to make a vaginal examination, in order to make out the state of development, size, &c., of the uterus. Exactly in the same way, in deciding upon the pathological nature of any marked uterine disease, and consequently upon the line of treatment which it may require, we believe precisely the same caution to be necessary, and the same local examination to be requisite, where there exists any doubt, and where such a local examination is not otherwise counter-indicated. And in a practical point of view, the examination in a case of uterine disease is far more necessary, because far more important, than in the case of pregnancy. In pregnancy the local examination is had recourse to, in order to determine a point which time itself would soon alone determine. In uterine diseases the same physical examination is had recourse to, for an object of much more immediate and practical moment, viz.: to obtain information sufficient to enable us to form a correct and precise judgment as to the specific pathological nature of the case before us, and, consequently, to enable us to select the proper remedial measures for its treatment and cure.

WHAT ARE THE MEANS OF PHYSICAL DIAGNOSIS, THAT WE MAY USE IN DISEASES OF THE UTERUS, OVARIES, ETC.?

Now, in the diagnosis of these diseases, we require to use the sense of touch, and occasionally of sight, hearing, and smell. We require to use the sense of touch in the external and internal examination of the region of the uterus and of the surrounding parts. We require to use the sense of hearing in the employment of auscultation or percussion. We require to use sight in the use of the speculum. And the nature of the fœtor emitted by the vaginal discharge is sometimes almost enough in itself to decide for or against the existence of extensive carcinomatous ulceration and disintegration.

The os uteri is, as you are aware, an opening so narrow as not to allow the passage of the finger through it for the examination of the interior of the cavity or walls of the viscus. But it is most important, in some cases, to be able to make an examination of this cavity and of these walls, for the purpose of ascertaining various points with regard to their condition; and in order to do so, we may pass through the os a slender metallic finger, or, in other words, use the uterine sound. Or, if necessary, we may enlarge the size of the opening of the os, so as even to admit the finger itself, by introducing a series of dilating sponge-tents into the cavity of the os and cervix uteri. Occasionally matters are discharged from the cavity of the uterus, of which it is necessary to ascertain the character and source; and, in some instances, we are aided in coming to a decision upon this matter by the use of the microscope and by chemical tests. Sometimes there are fluid collections in the cellular tissue of the pelvis, or in the ovary; and it may become a matter of life or death to the patient that we should be able to trace what the character of these effusions really is, and, in particular, whether they are inflammatory and purulent or not. We cannot reach them with the finger, but we may reach them with the exploring needle, and in this way trace their true character. Lastly, in cases of neuralgic tenderness of the abdomen and passages, and more especially when these are combined, as they often are, with tympanitic distension and enlargement of the abdomen, giving an appearance of formidable enlargement and very serious disease, it is sometimes impossible to make an accurate examination of the abdominal and pelvic viscera till the patient is fully anæsthetized; and you have seen in the hospital more than one case where the employment of chloroform has, in this way, at once dispelled what previously seemed to be an undoubted and enormous ovarian or uterine tumor.

We have thus, you will observe, a variety of means of physical diagnosis, that we may resort to in the detection and discrimination of the diseases of the uterus, and ovaries, and neighboring parts; and, if we attempt to classify them, we may reduce them to the following order:

1. The external or abdominal examination of the patient by sight, touch, auscultation, and percussion.
2. The tactile examination of the uterus, ovaries, &c., by the vagina or by the rectum.
3. That most important mode of diagnosis, viz.: the simultaneous combination of the external and internal modes of tactile examination.
4. The use of the speculum.
5. The use of the uterine sound.
6. The use of sponge-tents, with the view of dilating the os uteri, so that the finger can be introduced into the cavity of the cervix or cavity of the body of the organ.
7. The microscopic and chemical examination of the discharges from the uterus and vagina.
8. The employment of the exploring needle in cases of fluid collections, in order to ascertain the contents of such collections; and
9. The adoption of anæsthetic agents, to relax the abdominal parietes, and enable us to practise the different modes of examination, in cases of excessive or neuralgic tenderness of the abdominal surface or vagina, &c.

Most of the above means of physical diagnosis are discussed in the following chapters of this division of the volume. For the introduction of four of these means of uterine diagnosis, viz., the sound, sponge tents, the exploring needle, and the production of anæsthesia to facilitate examination, modern pathology is, we believe, indebted to Dr. Simpson.—(ED.)

ON THE POSITION OF THE PATIENT FOR THE USE OF THE SPECULUM.[1]

(From Edinburgh Medical and Surgical Journal, January, 1841, p. 155.)

It is almost unnecessary to insist at the present day, upon the importance of the early and accurate local examination of the uterus in all cases of suspicious vaginal discharges. In some instances, examination by the finger may be sufficient; but in every doubtful case the speculum should likewise be resorted to, if there is any affection of the vagina or cervix. It will be often found confirming, and not unfrequently also changing and rectifying the opinion which the mere tactile examination had led the practitioner to adopt. In this country great difficulties have been raised against the more general introduction of the speculum into practice, in consequence of the very disagreeable exposure of the person of the patient, which is usually considered necessary in its employment. In my own practice I have latterly endeavored to avoid this very natural objection, by teaching myself to introduce and use the instrument when the patient was placed on her left side in the position usually assumed in making a tactile examination, and with the nates near the edge of the bed. I strongly recommend my professional brethren to follow this plan, as by it, and with attention to the management of the bed-clothes, I have found that the instrument can be perfectly employed with little, or indeed without any exposure of the body of the patient. The speculum is introduced easily without the assistance of sight, and the mouth of it only requires to be afterwards uncovered, in order to enable us to examine the cervix uteri and top of the vagina. I have made trials of many different forms of specula, and find, for almost all purposes, that of Charrière by far the most manageable. In exposing the cervix uteri for the purpose of drawing blood from it by scarification, in cases of chronic congestion and metritis, I have occasionally employed a tubular speculum with advantage, but even in this case the three-bladed instrument is equally useful, and in some instances preferable. In a case of ulcer of the os uteri which I am at present attending with Dr. John Gairdner, and where the passages are much relaxed and the uterus very low in the vagina, I have, on Dr. Gairdner's suggestion, employed with much advantage a short tubular speculum of only an inch and a half in length, and with a deficiency or opening along the course of one side of it, of sufficient size to enable me to pass my finger for the purpose of placing the diseased part in the proper centre of the instrument. We have thus been enabled to touch easily the ulcerated surface with different applications; while with the usual instruments it was found a very difficult task to fix in this instance the very mobile cervix uteri.

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[1] See further on this subject, p. 65.

The speculum generally employed by Dr. Simpson, is the three-bladed instrument of Charrière, with the handles so arranged as to fold backwards when not used. This modification of the handles renders the instrument much more convenient for the pocket. Sometimes we have seen him employ a tubular glass speculum; and occasionally Coxeter's two-bladed instrument.—(Ed.)

MEMOIR ON THE UTERINE SOUND.

(From London and Edinburgh Monthly Journal of Medical Science, June, 1843, p. 547.)[1]

SECTION I.

The symptoms by which the diseased states of the various individual organs of the body are detected and discriminated from each other, are divisible into two great classes. The first class, forming the Functional, Dynamical, or Rational symptoms of various pathological writers, includes all the ascertainable derangements of functions, local and sympathetic, that may be present; the second class, constituting the Physical symptoms of the same authors, comprehends all the ascertainable circumstances connected with the structure, density, form, and other anatomical conditions of the organ which is the primary seat of disease.

Our diagnosis is always the more exact and perfect the more we can combine the information that may be gained from both classes or sets of symptoms—and especially the more that we can manage to confirm or correct the knowledge derivable from our study of the functional symptoms, by ascertaining, by the cognizance of our own senses, the exact physical state of the affected organ with which these symptoms happen to coexist.

[1] Read before the Medico-Chirurgical Society of Edinburgh, April 19, 1843.

We have preferred, rather than disjoint this memoir, to retain it in its original form; although the introductory portion may appear in some points a repetition of the previous lecture.—(Ed.)

In morbid affections of the exterior parts of the body, the Physical Diagnosis of the disease can be in general easily accomplished, by the direct tactile examination and visual inspection of the affected organ. It is this facility of examination and inspection which renders the diagnosis of these more external diseases, which belong to the province of the surgeon, so much more precise and accurate than the discrimination of those maladies of the more internal parts of the body, that fall under the investigation of the physician.

Within the last half century, however, the diagnosis of the diseases of the deeper-seated organs has been very greatly advanced by the application of various measures to improve our knowledge of their existing structural states. In fact, the medical science of the present day owes its superiority over that of an earlier date to no circumstance more, than to the increased degree of attention that has, for a considerable time past, been devoted to the study and improvement of Physical Diagnosis. As the knowledge of the structural lesions which the various organs may undergo from disease, has of later years extended in the hands of the pathological anatomist from his examination of the body after death, the practical physician has, for the purpose of his diagnosis and the guidance of his treatment, exerted himself in discovering means of detecting these same morbid alterations during the lifetime of his patient, and thus of studying, if I may so speak, necroscopic anatomy upon the living body. It is true that, in the discrimination of the diseases of some organs, as of the brain or spinal cord, we are, with some slight exceptions, obliged to trust entirely to the functional symptoms, because we have no means of detecting the morbid states of these organs except in the derangements produced in their functions. Hence arises the occult character of this class of diseases. In the diseases, however, of other organs, as those of the chest, the fact is different. Formerly the affections of the thoracic organs were also, as those of the head still are, detected and distinguished by their functional symptoms and derangements only, and their diagnosis was consequently always more or less doubtful, and often exceedingly obscure. Now that we can ascertain, with so much precision, the existing anatomical state of the lungs and heart by auscultation, percussion, &c., and thus combine the physical with the functional diagnosis of pulmonary and cardiac diseases, much of the difficulty and obscurity that was formerly connected with their detection and discrimination has entirely disappeared.

In ascertaining the Diseases of the Uterus, we have it in our power to avail ourselves of both the classes of symptoms of which I have spoken; or, in other words, we may form our judgment of its morbid conditions, both by studying the vital Functional Derangements, local, sympathetic, and constitutional, that may be present; and by informing ourselves, by Physical Diagnosis, of the exact existing state or states of the organ itself. Each of these classes of symptoms may afford us most important information; and in all cases where both can be had recourse to, our diagnosis will be greatly more certain under their combined evidence, than if the data furnished by either were alone trusted to. Of the two, if we are to make comparisons between them, the physical symptoms, are assuredly, in most cases, by far the most valuable and trustworthy—and yet in the common course of medical practice, the testimony which they are capable of affording, is but too frequently neglected and overlooked—and the functional and much less faithful class of symptoms alone relied upon, as well in forming the diagnosis, as in directing the measures of treatment.

Thus, if we attempt to analyze the mode in which the medical practitioner usually endeavors, in any suspected case of uterine disease, to detect the presence and character, or ascertain the absence of such an affection, we shall find, I believe, that he generally proceeds by taking into consideration some or all of the following sources and varieties of information:

First, The local and functional state of the uterus, so far as it is indicated by the quantity, character, periodicity, &c., of the menstrual and mucous secretion of the organ; by the occurrence and non-occurrence of morbid uterine or vaginal discharges, as blood, serous fluid, pus, &c.; by the existence or not of morbid sensations in the region of the uterus, such as different modifications of pain, intermittent or continuous, feelings of heat, weight, tension, bearing down, &c.; and, if the patient be married, by the reproductive powers of the organ, as shown by sterility, by the recurrence of abortions, &c.

Secondly, The presence or absence of various morbid affections of the neighboring viscera, particularly of the rectum and bladder, and of branches of the vessels and nerves passing through the pelvis—as indicative either of their sympathetic irritation or of their mechanical compression by the enlarged or displaced uterus.

Thirdly, The existence or non-existence of secondary local neuralgic pains in the mammæ, along the lower extremities, in the loins, and at points along the course of the spinal column, in the parietes of the thorax or abdomen on one or other side, and especially under the left breast, and under the margin of the ribs, along the colon, &c., increased in their intensity by any causes of increased action in the uterus itself, by the erect posture, by menstruation, &c.

Fourthly, The state of the general constitution of the patient, as marked by various degrees of deviation from the standard of health—and especially by the supervention of nervous, hysterical, dyspeptic, chlorotic, or cachectic symptoms.

The several preceding series of morbid phenomena consist of derangements in the vital actions of the uterus—or of other parts and organs secondarily affected—or of the constitution at large—and so far strictly belong to the class of Functional or Dynamic symptoms only. Up to a late date in the history of uterine diagnosis, most practitioners remained, and some still remain satisfied with the degree of knowledge which is afforded by the above sources of information. No one, however, who is practically acquainted with the diseases of the uterus can have any hesitation in declaring that the symptoms derivable from these sources are utterly inadequate, in the general routine of such cases, for the purposes of correct diagnosis, and are constantly liable to lead into fallacy and error when their individual evidence is alone trusted to. In making this observation, I do not mean to allege that these classes of symptoms are not sufficient to give us the power, in most, though not in all instances, of detecting the actual presence of uterine disease. They are perfectly deficient, however, in this other point, that, by their single unassisted aid, we cannot ascertain what the exact character and nature of the existing disease is—nor, consequently, what may be the proper line of treatment requisite for its alleviation or removal. They may generally, in other words, enable us to decide that the uterus is the seat of some morbid condition, but are not adequate to inform us what that morbid condition really is. They may show that the organ is affected, without showing us how it is affected.

If we attempted to throw the generalities, regarding the diagnosis of uterine diseases, into propositions, we would, therefore, be inclined to lay down the following as our

FIRST PROPOSITION.

The general and local Functional Symptoms of Disease of the Uterus are such as enable us to localize, without enabling us to specialize, the exact existing affection of the organ.

One or two instances may serve to illustrate and impress more strongly than any mere abstract statement, the force and truth of the above proposition.

In the changed and changing state of the uterus during Pregnancy, we have in all cases a constancy in the primary site and character of the existing irritation—if we may apply a term so far pathological, to a state which, in its object, at least, is strictly physiological. But though in the pregnant female the local uterine irritation is constantly and specifically the same in its source and nature, the local and constitutional phenomena or Functional Symptoms to which it gives rise, are well known to vary infinitely in different individuals, and even in the same individual in different pregnancies. In one case, the health of the woman remains in all respects unimpaired, notwithstanding the altered condition of the uterus. In a second, we find her suffer from distressing local symptoms, such as weight, distension, and bearing-down sensations in the interior of the pelvis, derangements in the state of the bladder and rectum, œdema and stiffness in the lower extremities, &c. In a third, the local affections may be slight or altogether absent, but there may be severe and even serious sympathetic affections of the general constitution, or of particular and distant organs, such as of the stomach, with cardialgia or vomiting, or of the brain, with headache and sleeplessness, or even with convulsions or mania.

Again, in other instances, we occasionally find all the usual symptoms, both local and constitutional, of pregnancy present, in so marked a degree, as to impress both the patient and the practitioner with the certainty of the existence of that state; and yet these symptoms may all, after a short time, be found dependent on the irritation produced by some subacute functional or organic disease of the uterus, that has no relation whatever to utero-gestation, except in the identity of its primary seat, and in the similarity of the symptoms to which it gives rise.

The few well-known facts to which I have thus adverted directly point to the two following important deductions:—1st, That in pregnancy we have the same identical condition of the uterus, not always accompanied by the same identical symptoms; and 2d, That the usual concourse and succession of functional phenomena to which pregnancy generally gives rise, may be induced by other states of the organ than the state of utero-gestation.

The same two important inferences are true in regard to the various individual morbid affections of the uterus. The marked uncertainty which exists regarding the effects produced by the condition of the organ in *pregnancy*, holds equally good regarding the effects produced by it in its different states of actual *disease*. In uterine disease, as in pregnancy, the same specific affection of the organ excites sometimes very different phenomena in different cases; and the same specific phenomena frequently result from affections of the organ that are entirely at variance with each other in their pathological character, in their course, and in their treatment.

Probably the most common organic disease of the uterus consists in the development of those fibrous tumors which are so frequent in the tissues of the body and fundus, and so rare in those of the cervix of the organ. These tumors do not, in some cases; occasion any very decided symptoms, and are often, as I have repeatedly known, only accidentally discovered in the living subject, after reaching a very large size; or, as frequently happens, they are not suspected or detected before they are found on the post-mortem examination of the body. I have one such tumor in my museum, which weighed fourteen pounds, and where the principal or only symptom during life was the mere enlargement of the lower portion of the abdomen, produced by the presence of this great mass. But in other cases, these fibrous tumors of the uterus, even when still small, often produce distressing irritation among the pelvic viscera, and derange in various ways the physiological function of the organ—producing sometimes diminution or suppression of the menstrual secretion, or again, especially when they are situated near the mucous surface, inducing leucorrhœa and attacks of menorrhagia, that are occasionally most formidable, both from their severity and permanency. But the remarkable circumstance with regard to this, and almost all other structural diseases of the uterus is, that though the walls of the organ be the seat of extensive morbid transformations and deposits, the menstrual secretion frequently remains so regular and normal as to deceive both the patient and her medical attendant; and I have known conception—the principal physiological function of the uterus—to take place, not only where fibrous tumors were present, but, in more than one case, where the cervix of the organ was the seat of malignant disease, that destroyed the patient shortly after her abortion or delivery.

Let us consider for a moment one other instance, illustrative of the important practical fact that the same organic disease of the uterus is often attended by the most varied, and even apparently opposite sets of external symptoms.

Scirrhous degeneration of the cervix uteri is an affection constantly occurring in the course of practice. This disease sometimes gives rise, at an early period of its progress, to severe pains and sufferings in the uterine region; to great local irritation of the bladder and neighboring parts; and to the supervention of marked sympathetic and constitutional phenomena. In other numerous instances, however, it marches onwards to an advanced stage without occasioning almost one single symptom in the way of local pains, discharges, or otherwise, calculated to rouse the attention of the patient to the impending work of destruction that is, with slow but fatal steps, going on within her. I have repeatedly seen cases of the kind where the disease was under assiduous treatment for simple leucorrhœa, or menorrhagia, merely because no examination had been instituted, in order to learn upon what local states the leucorrhœa or menorrhagia depended. In other cases the intensity of the sympathetic or secondary symptoms may be such as to conceal and disguise entirely the primary disease. In an instance of fatal carcinoma uteri that occurred lately in this city, the symptoms complained of during the lifetime of the patient were entirely referred to the urinary, and not to the uterine organs. I have known the mammæ most actively treated by leeching, &c., for the sympathetic pains present in them, while the state of the uterus itself (the primary cause of the pains) was altogether held out of view, until at last, when attention was ultimately called to it, its whole cervix was found utterly destroyed by cancerous ulceration.

While we thus not unfrequently find the most malignant organic diseases of the uterus more or less latent or marked in their symptoms, we have, on the other hand, sometimes the most severe local and constitutional symptoms of uterine disease developed in instances of slight and remediable organic affections of the part, as in cases of simple Ulcerative and Granular Inflammation of the cervix; and these symptoms may be all present in their most aggravated forms for months and even for years, where the local examination and final result show us that there is certainly no organic disease whatever, as in cases of "irritable uterus," or hysteralgia. Indeed, in some females, we have all these symptoms strongly but temporarily excited at every recurrence of the catamenial discharge, in connection merely with that congestion and increased vital activity of the organ which accompanies its natural menstrual secretion.

We may meet, in short, with the same train of local and secondary functional symptoms indicative of uterine disease, in neuralgic, in congestive, in inflammatory, and malignant diseases of the organ; and whether we explain it by the slowness with which morbid depositions are apt to develope themselves in this organ—by the slight sensibility of the component tissues of the viscus—or by the intermittent and latent nature of its vital actions—there can be no doubt of the fact, that there seems to be no organ in which there is a less strict relation observable between the intensity and character of the existing pathological disease, and the intensity and character of the accompanying symptoms, or between the exact nature of the structural lesions that are present, and the exact combination and succession of functional derangements to which they give rise. Hence, in order to form in any case a correct diagnosis in regard to the existing state of the uterus, it is necessary to ascertain, as far as possible, if any anatomical alteration may be present in the structure and organization of the organ—as well as in its physiological functions—

and what these alterations are. In other words, we must institute a local examination of the organ itself, by the sense of touch, and, if necessary, by the use of the speculum. It is assuredly only by doing so, that we can hope with any certainty to decide upon the specific nature of the uterine disease that may be present. We may make the general diagnosis of the existence of uterine disease, by the consideration of the functional derangements to which such disease gives rise. We can only make the *differential* diagnosis of what the specific disease really is, by aiding this by the examination of the structural condition of the organ itself.

In deciding upon the existence or non-existence of pregnancy, especially in any cases of importance or doubt, no medical man, who valued his own professional character, would deem himself justified in offering a final and dogmatic opinion, from the mere functional symptoms only, which we have already seen to be sometimes very equivocal—nor would he venture to form a definite judgment, until he had made a sufficiently accurate physical examination of the state of the uterus itself. In deciding in the same way upon the pathological nature, and consequently upon the line of treatment which any marked uterine disease may require, we believe exactly the same caution to be necessary, and the same local examination to be demanded, where there exists any doubt, and where the examination is not otherwise counter-indicated. The local examination of the uterus is had recourse to, in the case of pregnancy, to settle a point which time itself would alone ultimately decide. The local examination is had recourse to in the case of uterine disease, for an object of much more immediate and practical moment, namely, to obtain that information which can alone enable us to form a proper and precise judgment of the nature of the case before us, and to select the proper remedial measures for its mitigation, arrestment, or timely removal. We have seen some unfortunate cases, where its unwarrantable omission in the earlier and curable stages of disease has allowed the morbid action to make so extensive and fatal progress before sufficient alarm was excited, as to be utterly beyond the reach of treatment. Consequently it appears to us, that in uterine diagnosis it may be most safely and justly laid down as a

SECOND PROPOSITION.

In almost all instances of diseases of the Uterus, it is only by the Physical Examination of the organ itself that we can distinguish the precise nature of the existing affection, and fix its character, extent, &c.

The information thus obtained may be merely *negative*, but it is not the less useful either in a diagnostic or in a practical point of view. In a case of morbid discharge of blood from the vagina, for example, we may only ascertain that there is no appreciable organic disease of the uterus. Our prognosis and treatment, however, of a case of menorrhagia would, under such circumstances, be very different from those which we should adopt, if, by the same examination, we discovered a state of simple ulceration of the cervix, or congestive enlargement of the uterus, or the presence of a polypus, or the existence of a cauliflower excrescence, or carcinomatous degeneration.

Local examination, however, usually affords us much *positive* and direct information with regard to the seat and character of the existing disease, by informing us of all those changes that have taken place in the vagina and uterus, which can be recognized by touch or sight. By it we are thus often enabled to detect the different morbid conditions of the cervix, whether congestive, inflammatory, or more strictly organic. We can generally distinguish, by the same means, these states from each other, and discriminate between the equally enlarged and dark-colored congestion of the cervix, and the different forms of inflammation to which its structures are liable, whether that inflammation has assumed the granular, ulcerative, aphthous, or pustular type;—between simple, syphilitic, corroding, and carcinomatous ulcers of this part;—between granular enlargements, cauliflower excrescence, and cancerous degeneration of the cervix;—between the vaginal tumors formed by prolapsus, or by inversion of the uterus, and those formed by the presence of a simply hypertrophied cervix, or of a true polypus;—and between the general diffused enlargement of the organ produced by hypertrophy of its walls, or distension of its cavity, and that irregular, roundish, knobbed form which it almost invariably assumes in cases of fibrous tumors, and in such cases only.

THIRD PROPOSITION.

The Physical Examination, as hitherto practised, seldom enables us to ascertain accurately, the organic condition of more than the cervix and lower part of the body of the Uterus.

If the uterus be large, and the woman of a spare and lax habit, we may indeed be able to feel the fundus of the organ through the walls of the abdomen, by the hand pressed in above the pubis. It generally, however, lies so low in the pelvis, and usually moves away so readily under the touch and pressure of the fingers, that, even in such persons, this means of examination is of no great avail. In all persons of an opposite habit, and in most cases where the uterus is misplaced without enlargement, the hypogastric examination is of little or no use whatever.

By examination per vaginam with the finger, we can only, except when the uterus is prolapsed, feel the cervix, and the parts resting on the roof of the vagina. Hence, diseased states of the cervix having been far more easily ascertained than diseased states of the body and fundus of the organ, have, probably, been very much overrated, both in frequency and importance, at least since the speculum has come into more general use. In regard to the parts that are felt through the roof of the vagina, the touch alone does not, in many instances, give us information at all sufficiently satisfactory and decisive. We can, no doubt, when the tumor is large, often ascertain with considerable accuracy, its size and form, by combining the vaginal examination with the aid derived from examination by the rectum, and above the pubis. But we are still, in many cases, left entirely in the dark as to whether the existing tumor is an enlargement of the whole mass of the uterus, or a distension of its cavity, or a morbid growth; and, if the latter, whether the growth is seated in the uterus itself, or in one of the ovaries or other neighboring parts. If the tumor is small, and cannot be felt above the brim, we then have it not even in our power to ascertain its size and form, as we can examine it only on one side, namely, on that next the vagina. Under these circumstances we are unable to tell whether it arises from a new morbid structure attached to the uterine parietes, or from a simple displacement, or flexion of the fundus of the uterus itself.

To meet these and other difficulties in uterine diagnosis, I have for some time past been in the habit of using a metallic Uterine Sound or Bougie of nearly the size and shape of a small male catheter, which, when introduced, as it can easily be done, into the interior of the uterus, and manipulated there in different ways, has proved to me of great service in rendering the diagnosis of the diseases in question, and more particularly those of the *fundus*, *body*, and *cavity* of the organ, parts usually considered beyond the reach of examination, much more accurate and precise than can be effected by any other means with which I am acquainted. I am induced to bring this means before the notice of the profession, under the strong hope that the instrument will be found of equal service in the hands of others; and the results which have been obtained by it seem already sufficient to enable us to place, among the generalities of uterine diagnosis, the following as a

FOURTH PROPOSITION.

It is possible, by the use of a Uterine Sound or Bougie introduced into the uterine cavity, to ascertain the exact position and direction of the body and fundus of the organ—to bring these higher parts of the uterus, in most instances, within the reach of tactile examination, and to ascertain various important circumstances regarding the os, cavity, lining membrane, and walls of the viscus.

Having already exhausted my present limits by these preliminary observations, I must reserve the particular description of the instrument,—the modes of introducing and using it,—and the different diagnostic indications which it is capable of fulfilling, for a second communication.

SECTION II.

PROPOSALS FOR THE IMPROVEMENT AND ELUCIDATION OF UTERINE DIAGNOSIS, BY MEANS OF A SOUND OR BOUGIE PASSED INTO THE UTERINE CAVITY.

(From Lond. and Edin. Monthly Journal of Med. Science, August, 1843, p. 701.)

In my former paper I took occasion to speak of the uncertainty of those symptoms of Uterine Disease that consist merely of derangements in the functions of the uterus itself, and of other contiguous and sympathizing parts. Some facts were adduced to show, that, in uterine pathology, this uncertainty was so marked, that frequently the same affection of the organ was accompanied in different cases by different trains of functional symptoms; while it held equally true, that occasionally the same train of symptoms was found indiscriminately in connection with a number of morbid conditions of the uterus, that were essentially and practically dissimilar from one another both in their nature and treatment. From this frequent want of relation between morbid states of the uterus and their functional effects—from finding identical lesions combined, in a variety of instances, with very diversified symptoms—and identical symptoms connected with very different lesions—it followed, that in order to form a sure and perfect diagnosis in this class of affections, it is generally requisite to take into consideration the exact structural state of the uterus itself, and hence to institute, for the ascertainment of this point, a careful local examination of the organ. In urging the necessity of such a physical examination of the uterus in uterine diseases, I endeavored only to claim for that organ a means of diagnosis, which, when practicable, is regarded as indispensable in the case of every other part of the system. In discriminating from each other, for example, the various kinds of morbid affections of the eye, no medical man would trust merely to the knowledge that he might acquire regarding the derangements of vision that might be present, the degree and character of the local and sympathetic pains, the quality and nature of the lachrymal and conjunctival secretions, the accompanying state of the system in general, &c.,—he would farther, in order to arrive at such an accurate diagnosis as would enable him to institute a rational course of treatment, examine as thoroughly as possible the local structural condition of the organ itself, as the only means of determining in what individual texture or textures, whether the conjunctiva, cornea, lens, &c., the disease was specially localized, and what the precise nature of the morbid action really was in the texture that was affected. In the same way, in a case of excessive morbid irritation of the urinary passages, nos urgeon would venture to decide precisely what the extent, seat, and nature of the affection was—and whether renal, vesical, or urethral—until he had made a strict local or physical examination of the urinary organs themselves. He might, by the kind of functional derangements present, be led to suppose that the morbid irritation was connected with a stone in the bladder, or an enlargement of the prostate, or a stricture of the urethra, &c.; but he could never be perfectly certain that one or other of these was the cause, until he had instituted, with the finger, sound, &c., a local examination of the parts implicated. It is exactly the same with uterine diseases. The external symptoms may show the presence of disease in the uterine organs, and occasionally may be such as to lead us to adopt some opinion as to its nature; but we can never, in any case of the slightest doubt, be certain of its exact character and extent, to such a degree as to serve for guides to our diagnosis, prognosis, and treatment, unless we have the aid of the knowledge of the local structural state of the viscus itself.

A portion of my former communication was further taken up in showing, that the methods of uterine diagnosis as hitherto practised, whether by touch, or the speculum, or both, were, except where the patient was of a spare and relaxed form, calculated principally or only to distinguish the various morbid states of the cervix uteri. Lastly, I ventured to suggest, as an addition to our other means of uterine diagnosis, that the higher and more interior parts of the uterus should be examined by a Sound or Bougie introduced into the uterine cavity, and stated that various points of great importance, and otherwise unascertainable, could be made out by its assistance.

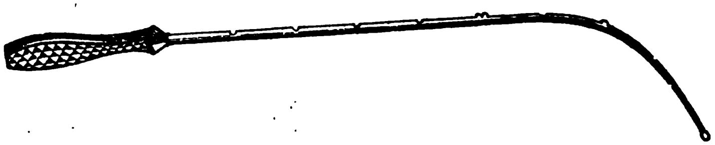
In the present chapter I intend briefly to state the kind of instrument I have used, and the modes of introducing and manipulating it, with some of the points of information which we can obtain through its employment.

DESCRIPTION OF THE UTERINE SOUND.

I have already stated that the instrument which I employ is somewhat similar to a small male catheter. It is, moreover, provided, like the common male sound, with a flat handle, to facilitate its manipulation; and terminates at its other extremity in a rounded knob or bulb, to prevent injury to the uterine textures. The intervening stem tapers gradually from the handle to the knob, the thickest part being nearly one-fifth of an inch in diameter, equal to the size of a catheter No. 8, and the thinnest part about one-tenth of an inch, or equal to a catheter No. 3. The greater thickness of the attached extremity is necessary to give that portion of the instrument the requisite degree of strength and resistance; it is more slender towards its other extremity, to allow of its easily entering into, and being moved in the orifice and canal of the uterus. The terminating bulb is about one-eighth of an inch in diameter. The stem is about nine inches in length, and graduated so as to render its employment, and some of its indications with regard to the measurement of the uterus, more precise. Different modes may be had recourse to in marking it, but the marks, whatever they may be, must be such that they can be easily felt with the finger while the instrument is within the vagina. For this purpose, they must be placed on the convex or posterior surface of the instrument, the surface, namely, with which the directing finger is in contact. The mode of graduation adopted in the instrument I have myself used, and which is figured in the engraving, is as follows:—At two inches and a half from the extremity of the instrument, this measurement being the usual length of the uterine cavity, there is placed a slight elevation or knob, which, in the employment of the Bougie, at once serves to show that it is introduced to the full extent into the interior of the organ, and at the same time forms a fixed or standard point from which the instrument may be farther graduated towards either of its two extremities. This farther graduation is marked by shallow grooves, which may be placed at the distance of either half an inch or an inch from each other, and, by their assistance, it becomes an easy matter to measure the exact length of the uterine cavity, when either it is diminished, or, as much more frequently happens, when it is prolonged to different degrees beyond its usual dimensions.

The alternate groove may be double, to facilitate the measurement by the finger. The form of curvature at the extremity of the instrument is nearly that of a common catheter, and like it, begins about three or four inches from its point; but the degree and extent of this curvature require to be varied according to the necessities of individual cases, and according to the indications which it is wished to bring out. Hence the stem of the Bougie requires to be formed of a metal that admits of being pretty freely and frequently bent without the risk of fracture, and at the same time is capable of adequately maintaining any form that may be temporarily given to it. These objects are fully attained when the stem is made of solid silver, but probably some composition of the inferior metals may also be found to have the necessary combination of pliability and strength. The handle of the Bougie is made of wood or ivory, is about three inches long, three-fourths of an inch at its broadest part, and rather more than a quarter of an inch in thickness. The posterior surface of it is smooth, whilst its anterior surface, or that corresponding to the concave aspect of the Bougie, is roughened, in order to make the operator constantly aware of the direction of the point of the instrument when it is hid in the uterine cavity—a circumstance which we shall afterwards see to be of great importance in reference to some of its diagnostic uses.

Fig. 1.



Mode of Introduction.—In introducing the Bougie or Sound into the uterine cavity, the patient may be placed either on her back or left side. If on her back, the fore-finger of the right hand is introduced into the vagina, and its extremity brought in contact with the indentation formed in the cervix uteri by the os tincæ, so as to act as a guide to the point of the instrument. The instrument itself is held in the left hand, and its point slipped along the palmar surface of the finger in the vagina, and directed by it into the uterine orifice. If the patient is placed on her side, she must lie with her body directly across the bed—a position which facilitates greatly the manipulations required both for common tactile examination, and for examination by the Sound and Speculum. In this case the fore-finger of the left hand is used as a guide to the os uteri, and the instrument is held in and directed by the right. In some instances where the parts are very lax, and the cervix uteri in any way displaced, the introduction of the Bougie is facilitated by passing both the fore and middle fingers into the vagina, fixing the cervix with them in the axis of the passages, and gliding the instrument along the groove between the two up to the os. In whichever of these ways the Sound is guided up to and passed within the os uteri, it generally afterwards glides easily, under a slight propulsive force, along the canal of the cervix and body, till, as shown by the elevated mark already alluded to as placed on the stem, its extremity has passed onward to the fundus of the organ. Sometimes the extremity of the instrument is slightly obstructed about an inch or less within the os tincæ, by the natural contraction existing there between the cavity of the cervix and cavity of the body. This obstacle is easily overcome by a little additional impulse, or, if that fail, by slightly retracting and altering the direction of the point of the Sound. The direction which the instrument should naturally follow in passing along the uterine canal, must always be held in view. In the normal condition of the parts, the uterus and vagina meet at a considerable angle, the former passing upwards and at the same time considerably forwards, and varying somewhat its inclination with the varying degrees of distension of the bladder and rectum. In using the Bougie, therefore, supposing the organ to be in the natural position, its concavity should be directed forwards towards the walls of the abdomen; or, in other words, it should be passed in the same direction as in most other manipulations in this part of the body, namely, in the line of the axis of the brim of the pelvis.

The degree of uneasiness felt by the patient during the passage of the instrument is in general very trifling, and not more, if so much, as is felt on passing the catheter along the urethra of the female, and certainly not by any means nearly so great as in using the Sound or Bougie in the male. In a few cases, I have seen it, like the passing of the sound in the male, produce a feeling of sickness and nausea. In the healthy state, however, of the organ, the lining membrane of the uterus does not in fact appear to be more sensitive than that of the vagina, so that the existence of any true and actual pain in making the examination with the Bougie is to be considered so far anormal, that it is generally, as we shall afterwards see, indicative of the existence of some morbid state or other of the part or parts with which the extremity of the instrument is at the time in contact.

In the average run of cases, the introduction of the Sound into the uterine cavity is probably not more difficult to accomplish than the introduction of the catheter into the bladder of the female. The os uteri is, in fact, usually much more easily and certainly detected by the finger, than the orifice of the urethra; and generally the one and the other instrument passes readily along its appropriate canal after it has once entered it. If it is otherwise, the very difficulty may be in itself important, as marking the existence of some anormal and probably diseased state. It is almost unnecessary to add, that the power of passing either instrument with perfect facility and certainty, is only to be gained by a little perseverance in the practical employment of them.

The manner in which the instrument should be manipulated, after it is fully introduced into the uterine cavity, varies according to the object or objects which we wish to ascertain. The mode of using it with these views will be best explained by now pointing out individually, and at some length, the different diagnostic indications which it is capable of fulfilling.

USES OF THE UTERINE SOUND.

I. *The Sound increases to a great degree our power of making a perfect and precise tactile examination of the Fundus, Body, and Cervix of the Uterus.*

I have already stated, that the body and fundus of the uterus are so deeply included in the cavity of the pelvis, and at the same time are generally so mobile under the pressure of the fingers, whether applied to the organ from above or below, that it is difficult to ascertain anything precise with regard to the condition of these parts, either by a common vaginal, or by a hypogastric examination.

The obstacles which thus so effectually oppose an accurate and complete tactile investigation of the surfaces and walls of the body and fundus of the uterus, may be in most cases, in a great measure, overcome by the aid of the Uterine Sound, if we employ it for the double purpose—first, of giving sufficient *resistance* to the organ for its exploration by the fingers; and secondly, of altering the *position* of its parts, so as to bring them each successively within the reach of tactile examination.

When the pliable and mobile uterus is held steady by the sound being placed in its cavity, and the central axis of the organ is for the time being thus rendered, as it were, *firm*, and *solid*—the examination of its external surface and of its walls becomes a matter of far more certainty and accuracy than if we had the organ indefinitely yielding and receding before each slight touch of the fingers.

But besides thus rendering the organ fixed and resistant for our examination, the Uterine Sound will, as a diagnostic means, be found of still greater use and importance, by the control which its presence in the uterine cavity gives us over the *position* of the whole organ, and by its enabling us to alter at will the situation of the viscus to such a degree, that we can in succession bring within the range of a tactile investigation different parts of its external surface and parietes, that are generally considered to be entirely beyond our reach. In reference to this remark, it must be specially kept in mind that in the healthy state the uterus is so loosely fixed in its situation in the pelvic cavity, that its position is capable of being changed to a very considerable extent, without incommodity or injury, by such exterior influences as may naturally or accidentally act upon it. Its position is so far constantly changed by the varying states of distension of the bladder and rectum. Under voluntary efforts of straining, it can in general be readily pushed down half an inch or an inch in the cavity of the vagina. It may be drawn down by instruments till the cervix reaches the external parts themselves, or even protrudes beyond them—a circumstance which facilitates immensely the operation of excision of this part of the organ. In consequence of the same anatomical peculiarity, we are able, through the use of the Uterine Bougie, to move the organ upwards, forwards, &c., to such degrees as are requisite for a complete hypogastric examination, without in general causing any marked inconvenience or pain to the patient.[1]

If, after the bougie is introduced into the uterine cavity, we carry the handle backwards towards the perineum, the upper extremity of the instrument—and consequently the fundus uteri placed upon that extremity—will be proportionally moved forwards into the hypogastric region. One hand placed above the pubes will now feel the fundus uteri with the central and thinnest part only of the abdominal parietes intervening between the fingers and the surface of the uterus. Provided the woman be not of a full habit, and the abdominal muscles sufficiently relaxed by position, we can now pretty accurately examine with the hand placed on the hypogastric region, the state of the uterus as it is held forwards on the end of the bougie, and we may always make ourselves still more certain of its condition by retracting and otherwise moving the handle of the instrument so as to bring the different parts of the superior and anterior surfaces of the uterine tumor under the touch of the fingers. By a slight turn of the instrument to either side, the lateral surfaces of the upper part of the viscus may, in the same way, be brought under our tactile examination; and in spare subjects, I have occasionally found it possible, when the fundus was pushed against the abdominal parietes, to extend the manual examination to some distance along the posterior wall of the organ.

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[1] In corroboration of the statements in the text with regard to the natural *mobility* of the uterus, I would beg to fortify the remarks I have there made, by quoting upon the subject the opinions of two of the latest authors who have adverted to this particular point—Cruveilhier and Lisfranc. When speaking of the uterus, in his excellent System of Descriptive Anatomy, Cruveilhier observes, "The looseness and extensibility of its connections enable it to float, as it were, in the cavity of the pelvis, and to be moved to a greater or less extent. The facility with which it can be drawn towards the vulva in certain surgical operations, and its displacement during pregnancy, when it rises into the abdomen, are proofs of its great mobility."—Vol. i. p. 618 of Dr. Madden's translation. Lisfranc's observations are still stronger. "There is," he remarks, "a physiological fact of importance, but little known, and which hitherto has scarcely been alluded to by any writer. The uterus in the normal condition, and even when affected with engorgement, has a truly extraordinary mobility. To prove this, a very simple experiment will suffice. Let a speculum be introduced as high up the vagina as possible, so as to embrace the cervix uteri within its upper extremity; then bid the patient bear down as if at stool; and you will perceive that as the instrument descends, the uterus follows it to the extent of one, or even of two inches; an immense advantage when the surgeon wishes to bring down the uterus to near the vulva. In cases requiring operation about the cervix, all that the surgeon has to do is, to lay hold of the os uteri with a hook, and draw it gently down, until it comes fairly within sight; this may be effected without difficulty, and with very little inconvenience to the patient."—Translated from the second volume of the Clinique de la Pitié, into the Medico-Chirurgical Review, for April, 1843, p. 360.

In those cases where this cannot be effected, the sound still enables us to make a more perfect tactile examination of this—the posterior, part of the uterus, than we could otherwise effect, by giving us the power of temporarily depressing and reflecting its posterior wall, so that it may be felt by a rectal examination. The vaginal examination of the lower part of its anterior surface may be in general rendered more complete, by a similar aid from the instrument.

In these different steps of examination, the degree and accuracy of the information obtained is varied in different individuals by the differences which exist in the thickness of the tissues placed between the uterine surface and the fingers; but in most instances the presence of any marked irregularity in the uterine structures—such as the presence of one or more small fibrous tumors—their hypertrophic thickness, &c., may be readily made out—and, under still more favorable circumstances, the exact physical conditions of the organ in relation to its volume and dimensions, the morbid tenderness of individual parts of it, &c., may be precisely determined.

When we employ the sound for the purposes alluded to in the preceding sentences, namely, for enabling us to make hypogastric examinations of the fundus and body of the uterus, the instrument, before its introduction, should have its extremity bent upon its stem at as nearly a right angle as the conformation of the genital canals admit, and, after being introduced, its handle should be well retracted towards the perineum. By attending to these circumstances, the fundus and body of the uterus will be more easily and fully turned forwards, and our examination of them very much facilitated. The same object will also be much promoted by retaining the directing finger at the cervix during the course of the examination, both to steady the instrument and to serve as a fulcrum to it. In that case the handle may be retracted or pushed backwards to any required degree by the forearm, while the other hand is employed in the hypogastric examination.

The preceding remarks apply to the examination of the fundus and body of the uterus, parts which—unless when much enlarged, or the patient very thin—are generally looked upon as beyond reach of any physical diagnosis.

The physical states and relations of the cervix uteri are generally ascertainable by the finger alone. Still, in various morbid conditions of the cervix, our tactile examination of the organ may be much promoted by the assistance of the bougie introduced into the uterine cavity. For instance, in chronic enlargement, cauliflower excrescence, and other organic diseases of that part, it is sometimes a matter of moment, both as regards our prognosis and our treatment, to ascertain if the existing diseased state stretches upwards so far as to involve or not the lower portion of the body of the uterus. In several such cases, I have found much assistance in determining this point by gently depressing the uterus by the bougie introduced into it, and having the power thus of examining the organ, immediately above the cervix, by compressing the structures of that part between a finger or two in the vagina, and the resistant sound placed in the uterine cavity, and consequently in the very axis of the viscus. In this way, each point in the circumference of this portion of the organ may be successively examined.

These observations apply generally to such indications as can be made out through the use of the bougie, when the uterus still retains that freedom of motion which we have seen it to possess, when it is itself in a healthy condition, and when there are no obstructions or impediments to its mobility in the surrounding parts. But there are cases where, from the organ having become more or less fixed and immobile, no advantage can be taken of those facilities which the power of partially displacing it in general allows us. In these instances, the very circumstance, however, of the mobility of the organ being lost, and still more the degree and extent of its immobility, often materially assists in pointing out the true nature of the affection that is present. Thus in scirrhus of the cervix, the early immobility of that part, in consequence of the morbid degeneration invading the contiguous tissues from almost the very commencement of the disease, is often one of the first and best characteristics of that dreaded malady. In this instance, the fixed state of the cervix of the organ is detected by the direct application of the finger. In other states of disease, the cervix remaining comparatively free and unaffected, the body and fundus may be immobile from various pathological causes, as from morbid adhesions, the consequence of inflammation of its peritoneal surface, from the pressure of tumors or abscesses, &c. Or, again, both cervix and fundus may be immobile at the same time, from general carcinoma of the organ, &c. In all these cases the immobility of the body and fundus, its degree, extent, and seat, can only be discovered by the bougie; and its use, along with other considerations, may further lead us to detect the special pathological state that may be the cause of the morbid attachment or fixture of these parts of the organ.

II.—*The previous introduction of the Sound facilitates and simplifies the subsequent Visual examination of the Cervix Uteri with the Speculum.*

In employing the speculum uteri, the principal obstacle which we have to contend with, is the impossibility of always catching easily and accurately the os and cervix uteri in the upper or internal extremity of the instrument, so that these parts may be brought at once and completely within the range of sight. Indeed the search after the cervix uteri, when it is not at first caught in the open end of the speculum, is sometimes so painful to the patient, and this part of the manipulation is occasionally so difficult to the operator, that every one who has made much use of the speculum will, I believe, be ready to confess that in some cases where the uterus is situated obliquely, or where the cervix is high and displaced, the object is almost impossible of attainment.[1] The previous introduction and use of the uterine sound, offers a simple and certain means, both of overcoming the difficulty in question, and of facilitating the employment of the speculum in our ordinary cases of examination with that instrument. After making such tactile examination as may be required with the sound, leave it in the uterine cavity, and using it as a general guide, slip the uterine extremity of the speculum, whether tubular or bladed, over its handle and along its stem, till the instrument be fully introduced. The upper or uterine extremity of the speculum is thus guided with almost unerring certainty along the stem of the sound, till the cervix uteri is touched and included in its opening. Further, if we still keep the sound in the uterus, we have in it a means of turning the cervix to one or other side at will, so as to give ourselves a complete view of the mucous membrane covering the whole vaginal surface of this part of the organ. This last step in the examination is much facilitated by first drawing aside the stem of the bougie into the cleft between the two blades of the speculum—provided we are using a double-bladed instrument.

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[1] Thus, in their excellent Treatise on Diseases of the Uterus, Boivin and Duges state that "The cervix uteri is sometimes so inclined backward that the speculum *cannot* show it by any movement."—Heming's Translation, p. 33. See, also, Lisfranc's Lectures as reported by Pauly, Maladies de l'Uterus d'après les Leçons Cliniques de M. Lisfranc, Paris, 1836, pp. 59, 60; and Téallier, Du Cancer de la Matrice, pp. 70, 71, &c.

In making these remarks, I presuppose that the patient, during the employment of the sound and speculum, is placed upon the left side in bed, in the position already pointed out as most favorable for making a common tactile examination. When occupying this position, with, as previously recommended, the body laid across the bed, the speculum can be employed with perfect ease and success, and, at the same time, without any of that revolting exposure of the person of the patient, which is unavoidable when she is turned, as is so commonly practised, upon her back during this operation. The instrument can be introduced readily without the assistance of sight, and if the bed and body-clothes be placed with a little care in contact with the surface of the patient, and the latter be closely arranged around the tube of the instrument, the mouth of the speculum is the only part which actually requires to be uncovered in order to enable us to examine the cervix uteri and canal of the vagina, and there ascertain those changes of color, superficial alterations of structure, &c., that the speculum is occasionally of so great service in divulging, and by which we may have an opportunity, in doubtful instances, of either confirming or correcting the previous evidence afforded by the sense of touch.

III.—*By the use of the Uterine Sound we may, in many instances of Pelvic and Hypogastric or Abdominal Tumors, ascertain the connection or non-connection of these Tumors with the Uterus.*

We have already seen the advantages of having the uterus fixed upon the bougie in facilitating the tactile examination of the outer surface of the organ. This use of the sound is of still greater importance, when a chronic pelvic, or hypogastric tumor is present, and we wish to ascertain whether this additional structure has its origin in, or any connection with, the tissues of the uterus, or is attached to some of the neighboring parts or organs. The power of making such a distinction leads, in some instances, to practical distinctions in the treatment, and in almost all cases to differences with regard to our opinion of the future progress of the disease. The prognosis, for example, is very different in ovarian dropsy, and in enlargement of the uterus from the presence of a large mass of fibrous tumors in its walls. I have found, however, no mistake to be more common in practice, than to suppose a tumor in the hypogastric or iliac region to be an enlarged and dropsical ovary, when it actually consisted of the other much less formidable disease of a great mass of fibrous tumors in the uterine structures. When these fibrous tumors "attain," to quote the words of Dr. Lee, "a large size, and come to occupy a great part of the abdominal cavity, they produce all the injurious consequences of enlarged ovaria, from which indeed during life, they are distinguished with the greatest difficulty."[1] "It is often," says Dr. Ashwell, "exceedingly difficult where a growth, occupying the abdominal cavity, is large, filling perhaps its greater portion, to determine whether the uterus or ovary, or both, may not be diseased."[2] This difficulty is, in some cases, more or less removed by the evidence afforded by the use of the uterine sound.

When the tumor which is present is uterine, and consists of either some general or partial enlargement of that organ, I have usually been able to gain satisfactory evidence of the fact by the bougie, when passed into the uterine cavity, entering, as it were, more or less directly into the very structure of the morbid mass, and by the tumor and instrument afterwards reciprocally moving in exact correspondence with all the possible motions imparted respectively to each of them.

In other instances, where the tumor is *not* uterine, I have repeatedly made myself and others certain of the fact, by first introducing the bougie, and so far giving us at once a knowledge of the exact position of the uterus, and a control over its movements, and then proceeding in one of three ways:—1. The uterus may be retained in its situation with the bougie, and then, by the assistance of the hand above the pubis, or by some fingers in the vagina, the tumor, if unattached to the uterine tissues, may be moved away from the fixed uterus:—2. The tumor being left in its situation, it may be possible to move away the uterus from it to such a degree as to show them to be unconnected:—or, 3. Instead of keeping the uterus fixed and moving the tumor—or fixing the tumor and moving the uterus, both may be moved simultaneously, the uterus by the bougie, and the tumor by the hand or fingers, to opposite sides of the pelvis, to such an extent as to give still more conclusive evidence of the same fact. In a case, for example, which I saw during the last winter, there were two distinct, firm, defined tumors to be felt at the brim of the pelvis when the hand was placed above the pubis. The smaller of these tumors was placed to the left, and somewhat anterior to the other. The two tumors lay so close as to seem to be connected with each other, and this, with their semi-globular form, impressed the physician who had charge of the case with the belief, that his patient had one of those masses of roundish fibrous growths affixed to the uterine walls, that I have already spoken of as not unfrequently met with in this part of the body. The hypogastric examination of the swelling induced me at first to adopt the same opinion, but the employment of the bougie readily showed us to be both in error. The instrument, when introduced through the os into the uterine cavity, passed directly upwards to the top of that tumor which lay towards the left side, and the apex of the sound could be felt through the intervening tissues, at once demonstrating that this apparent tumor was formed by the fundus of the somewhat enlarged and displaced uterus. Retaining the instrument in this position, the uterus was next moved by means of the bougie still further to the left, whilst the other tumor was at the same time pushed still further to the right or opposite side, by the pressure of the hand, with such ease and to such an extent, as to prove that it had no immediate organic connection with the uterus. The further examination of the tumor on the right side—its rounded form and other physical characters—its position behind the broad ligament, &c., showed it to be ovarian. Without, however, the aid of the bougie, the two hypogastric swellings in this case would assuredly have passed for uterine fibrous tumors, instead of the one being formed by the fundus of the uterus, and the other by an enlarged ovary.

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[1] Cyclopædia of Medicine, vol. iv. p. 388.

[2] Practical Treatise on the Diseases peculiar to Women, Part ii. p. 291.

The rules of diagnosis which I have been stating, evidently apply only to those cases in which the uterus and pelvic or hypogastric tumor are neither organically adherent to each other, nor so closely wedged together as to render them mutually immovable. But it often happens, that, in consequence of the existence of one or more of these last-mentioned conditions, none of the tests that I have just now spoken of can be applied, and in such instances the bougie affords no very affirmative evidence. Still, however, the knowledge which we can gain by it, and by it alone, of the state of the uterine cavity, of the increased or diminished length of that cavity, and especially of its relative *situation* and direction in regard to the existing morbid mass, are calculated in some of these more difficult and complicated instances to afford no small degree of assistance in the diagnosis.

In one common set of cases, the knowledge of the exact situation of the uterine cavity, and hence of the uterus itself, in relation to the tumor that is present, even when both were immobile, has in repeated instances appeared to me especially important. The ovary normally lies behind the uterus, being attached to the *posterior* surface of the broad ligament. If, therefore, in a case of chronic tumor situated in the pelvis, the sound shows the tumor to lie on the *anterior* surface of the uterus; or, in other words, if the uterine cavity runs up the posterior surface of the morbid mass, the disease may be considered as certainly not ovarian, and the further difficulties of the diagnosis will thus be so far very much simplified by way of exclusion. This remark particularly applies to those cases in which the tumor, of whatever nature it may be, is still not so large as to have passed out of the pelvic cavity and become abdominal.

I have found, however, advantage from the negative information given in other ways by the bougie, even when the tumor was abdominal in its seat. An example will best illustrate my meaning. In a case sent to Edinburgh a few months ago, for the purpose of having some opinion given in regard to its nature, an immense abdominal swelling that was present, and which had been supposed by some medical gentlemen who had seen the patient to be ovarian, was shown not to be so, by sufficient evidence of the following nature. The uterus was displaced obliquely backwards and the fundus of the bladder was displaced towards the right iliac region by the abominal enlargement—circumstances which were easily ascertained by introducing the uterine sound into the cavities of both of these organs. Further, the uterus, though displaced, was quite mobile: and when its fundus was turned by the bougie towards the site of either ovary, and the abdominal tumor lifted at the same time as high as possible towards the epigastrium, no obstruction was met with, nor was this great change upwards in the direction of the tumor found to produce *any dragging effects whatever* upon the uterus as held by the bougie, or upon its connections —effects which, unless under the improbable supposition of a pedicle several inches long, would have inevitably occurred if the diseased mass had originated in or was connected with the ovaries or uterine appendages. So far, the evidence was negative, but still, nevertheless, important. I may add, that other characters of a more positive nature—the history, particular form, and consistence of the tumor—its position in front of the intestines as ascertained by percussion, &c. &c.—seemed to show it, seeing that it was not ovarian, to be in all probability one of those hydatigenous tumors that sometimes form in the tissue of the omentum, and whose physical symptoms during life in many respects correspond with those of ovarian dropsy.

In a case, in which a very large hard and solid tumor was situated in the mesial line of the abdomen, and had been growing for years, the aid of the uterine bougie assisted us greatly in making a diagnosis of its pathological seat and character, by a kind of evidence which was exactly the reverse of that stated in the preceding instance. In the case we speak of, the os uteri was in its usual situation in the pelvis; the bougie, however, after passing through it, glided onwards, obliquely forwards, and towards the left, showing the body of the uterus to be displaced in that direction. Farther, it was ascertained, by moving the bougie in the uterine cavity, that the progress of the uterus towards the right side was entirely prevented by the presence of the opposing morbid mass, whilst it could be moved to the left side to a slight extent, but still so much so as to show that its surface was not *immediately* adherent to the tumor. Holding the bougie in the uterus, with a finger in contact with the cervix uteri, the body of the growth was next strongly pushed both upwards and to the right, with the other hand placed upon the lower part of the abdomen. When so moved, the tumor distinctly pulled upwards along with it the uterine bougie, and consequently, the uterus itself. The whole examination by the bougie thus showed; —that, *first*, The tumor was not a fibrous growth developed in the uterine structure, or intimately attached to its exterior surface, because the body of the uterus, though displaced by the presence of the growth, was still movable to a certain degree independently of it; *secondly*, It was probably an enlarged ovary, or tumor connected with the ovary or uterine appendages, because when moved upwards or to the right, it dragged the uterus along with it; *thirdly*, Though mesial in its position, it was attached to the side of the right ovary, or to the posterior surface of the right broad ligament, because the body and fundus of the uterus were displaced forwards and towards the left, and had some remaining mobility in that direction, but could not be moved by the bougie in any degree backwards or towards the right side, in consequence of the presence of the opposing mass of the tumor. These circumstances in the physical diagnosis of the tumor, as ascertained by the bougie, were confirmed by the other symptoms of the case; but these other symptoms would certainly, in consequence of the equivocal character and position of the tumor, have been in themselves insufficient to have fixed its true pathological seat and character.

In the preceding remarks, I have pointed out the uses of the uterine bougie, in so far as they aid our examination of the *exterior* of the uterus, or of the outer surface of the Fundus, Body, and Cervix, and enable us to distinguish between tumors of the uterus itself, and those situated in structures altogether external to it.

In continuing the subject in my next communication, I shall attempt to state the still more important uses to which the instrument can be put, in examining the *interior* of the organ, and the state of its walls, and in determining the presence of those common but hitherto little known forms of displacement that pass under the names of retroflexion, anteflexion, &c. I shall take the same opportunity of showing the circumstances which counter-indicate the use of the sound, and the cautions required in its employment.

SECTION III.

ON THE MEASUREMENT OF THE CAVITY OF THE UTERUS AS A MEANS OF DIAGNOSIS IN SOME OF THE MORBID STATES OF THAT ORGAN.

(London and Edinburgh Journal of Medical Science, Nov. 1843, p. 1009.)

No organ of the body is, in its normal and physiological state, more subject than the uterus to great and striking alterations of volume. Witness its rapid increase during pregnancy, and its still more rapid diminution in the puerperal female. In some of its anormal and pathological conditions, the same organ strongly displays the same tendeney to enlarge in different directions and dimensions. "No tissue," observes Cruveilhier, "is more extensible, more malleable (*plus extensible, plus malleable*), than the tissue of the uterus, when it has undergone the softening which constantly accompanies its hypertrophy. It becomes elongated and extended in all directions; and it is only when its distension is carried beyond measure, that upon its hypertrophy supervenes atrophy."[1] Again, on the other hand, the cavity of the unimpregnated uterus is so small, and its canal of communication with the vagina is so narrow, that they are liable to be found more or less obliterated by malconformation or disease; while the uterine walls themselves are sometimes observed, by their partial displacement and inversion, to shorten, and even to efface the internal cavity of the viscus.

The changes in the capacity and dimensions of the uterus, which are thus so often produced by disease, have been long well known to the pathological anatomist. They have hitherto, however, in a great measure, escaped the observation of the medical practitioner, because, though frequently discovered by him upon the dead body, he possessed no certain means of detecting their presence during the lifetime of his patient. In the following communication, it is my purpose to show, that this blank in uterine pathology may be filled up, and that we can ascertain, by means of the bougie, with sufficient facility and precision, upon the living subject, the varying sizes of the uterine cavity. And here, as in all other cases in which physical signs have enabled us to determine pathological lesions during life, we shall find that the knowledge of them may be rendered of the highest avail in diagnosis and practice.

With these views, we shall attempt to illustrate, as our next proposition, with regard to the uses of the instrument which we are describing, that

IV.—*The Uterine Bougie is capable of affording valuable diagnostic information, by enabling us to measure the Length of the Uterine Cavity.*

The uterus varies in its dimensions, and consequently in its length, in different persons, independently both of pregnancy and disease. In the unimpregnated state, and in its normal and healthy condition, I have found the cavity of the organ to measure, on an average of a pretty extensive number of observations made both upon the living and dead body, about two inches and a half from the os to the fundus. The bougie has, as already stated, an elevated mark upon its stem at this distance, two inches and a half, from its point, for the purpose of showing whether or not the instrument is completely introduced into the uterine cavity. The cavity is, in its normal varieties, more often above than below the standard. Further, the instrument, as previously mentioned in the description that has been given of it, is so graduated by inch or half-inch marks upon its stem, as to indicate, when it is introduced up to the fundus uteri, the exact degree of elongation, or of shortening of the interior of the organ. In illustrating its utility in this respect, I propose to demonstrate its application to the discovery, *first*, Of some morbid states in which the uterine cavity is preternaturally increased; and, *secondly*, Of others in which it is preternaturally diminished in length. In the present communication, we shall limit our attention to the first of these divisions, or to

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[1] Anatomie Pathologique, livr. xiii.

INSTANCES OF INCREASED LENGTH OF THE UTERINE CAVITY.

The cavity of the uterus may be found elongated under a variety of circumstances which we shall consider somewhat in detail.

1. *Morbid Permanence of the State of Puerperal Hypertrophy.*— This peculiar condition does not appear to have as yet attracted the attention of obstetric pathologists, as the cause of one of those forms of chronic hypogastric tumors that are occasionally met with during the first weeks and months after delivery. The want of any decisive means of recognizing it has doubtlessly led to this omission. The notes of a case will show better than any comment, the nature of the affection to which we allude, and the facility and certainty with which it may be recognized by means of the uterine bougie.

During the summer of 1842, I attended, along with Dr. Abercrombie, a lady, who, after a premature confinement in the country, had suffered from a smart attack of puerperal fever. After so far recovering for a few weeks she was sent from a considerable distance into town, to be treated for what appeared to be a large tumor, stretching upwards from the pelvis into the right iliac region. The tumor had not been observed before delivery, and was somewhat painful to the touch. It seemed at first sight extremely doubtful whether the mass consisted of an inflamed uterine fibrous tumor, or enlarged ovary, or of one of those chronic purulent collections which are apt to form towards one or other iliac region in connection with puerperal fever or inflammation. The sound, when introduced into the os uteri, passed easily and directly upwards for several inches to the superior end of the tumor, and its apex could be felt there by the hand placed externally. This at once showed the supposed diseased mass to consist of the enlarged uterus. Further examination proved that there was nothing strictly abnormal about the uterus except its great size. In fact, it was a case where the organ had apparently remained nearly undiminished after delivery, probably from the puerperal attack arresting the usual progress of its absorption and diminution. It decreased rapidly and fully under leeches and other local antiphlogistic treatment.[1]

May not some instances of pelvic or hypogastric swellings after delivery, which have been recorded by Puzos and others[2] as lacteal collections (*depôts laiteux*) or inflammatory effusions, and that ultimately yielded without suppuration, be of the above nature? The lateral position of the swelling is, as shown by the preceding case, not a sufficient criterion.

In one of those cases of subacute inflammatory tumors that occasionally form in the pelvis of the puerperal female, and that lately came under our care, the information afforded by the sound was perfectly the reverse of that obtained in the preceding instance, and yet still of considerable importance. I did not see the patient till nearly two months after her confinement. She had not regained much strength, and there were symptoms present which indicated a degree of subacute inflammation in the region of the uterus. A rounded and circumscribed tumor could be readily felt rising from the pelvis to the height of an inch or two above the brim of the pelvis. Was this a swelling resulting from some limited inflammatory effusion among the pelvic viscera? was it the uterus itself, or some neighboring organ, enlarged and diseased? or what was its nature? On passing the bougie through the os uteri, which was a matter of some difficulty, in consequence of its distance from the vulva, it slipped easily upwards to the top of the hypogastric swelling, and showed the apparent tumor to be the uterus itself, situated much higher, and more anteriorly than usual. It showed farther that the uterine cavity was of its usual length, and consequently that, though displaced, the organ was *not* enlarged. It was, however, found to be firmly fixed in its abnormal position by some morbid cause, which prevented its fundus and body from being moved by the bougie either backwards or laterally. On searching further into the nature of the morbid cause which could thus anormally displace and fix the organ, I found a large swelling situated at the upper part of the vagina, and between the uterus and rectum. It pressed downwards upon the roof of the vagina, and backwards upon the rectum, and pushed upwards the uterus in front. Its extent and situation in the pelvis were made out by a combined rectal and vaginal examination. Its nature was more obscure, and the feeling of fluctuation, if any, was very indistinct. In the presence of Mr. Ziegler, Dr. Keith, and Dr. Murray, a very slender exploring needle was passed from the rectum through the dense walls of the tumor, in order to ascertain whether it contained fluid, and if so, the nature of that fluid. A drop or two of thin pus escaped along the tube of the instrument. This at once satisfied us of the nature of the tumor. I immediately made a larger perforation into it from the bowel with Pouteau's trocar. A quantity of pus escaped, and continued to do so for a considerable time. The local and general symptoms under which the patient was suffering speedily abated, and she has since entirely recovered her general health and strength. The catamenia have returned. The uterus, however, still remains preternaturally fixed and immobile.

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[1] Long after the above case and the observations preceding it were written, I met with the following passage in Dr. Hooper's work, showing that the diseased state that I had recognized during life, was known to him as a post-mortem appearance: "When a fœtus has been recently expelled, it is, in some instances, a long time before the uterus returns to its original state; and it is larger and softer during the period. I have examined uteri four times their natural size from this cause, two months, and even more, after the fœtus was expelled."—Morbid Anatomy of the Human Uterus, p. 5. Is this the disease alluded to in Kleinert's Repertorium, Bd. ii. 1838, s. 51, as described by Kopp under the name of Hysteranesis, in the first volume of his Denkwürdigkeiten, p. 168? I regret that I have no access to this work.

[2] See Levret's Art des Accouchemens, p. 168. Des Engorgemens Laiteux dans le Bassin, &c. Deleurye's Traité des Accouchemens, p. 509. Des depôts laiteux dans les ligamens larges, &c., &c. In a MS. copy of William Hunter's Lectures, belonging to the College of Physicians, Edinburgh, among the remarks on puerperal diseases, is a short chapter on the "iliac abscess."

In the case which I have just detailed, the results were to me the more gratifying, inasmuch as some months previously I had seen, in the Lying-in Hospital, an instance of the same disease, in which the affection proved fatal, from the collection of matter bursting into the general cavity of the peritoneum. On the post-mortem inspection of the body, it was evident that from the thinness of the partition intervening between the purulent collection and the cavity of the rectum, the abscess would have soon burst into the bowel, if it had not given way superiorly into the peritoneum itself; and it was equally evident that an artificial opening, made from the rectum into the purulent sac before the period of its rupture, might have saved the life of the patient.

M. Martin of Montpellier, who has written one of the best memoirs extant upon this subject,[1] under the title of *Des depôts des*

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[1] See also among the more recent descriptions of the disease, the observations of Dance and Husson in Breschet's Repertoire d'Anatomie, tom. iv. p. 172 (8vo. edit.); Meniere in Archives Génér. de Médecine, tom. xvii. p. 529; Grisolle in ibid. tom. xlix. p. 37; Kyll in

annexes de la Matrice qui surviennent a la suite des Couches, recommends, in the treatment of the purulent collection, that, in order both to open it externally, and to produce previous adhesions of the containing sac to the abdominal parietes, caustic potass should be applied to that part of the hypogastrium which forms the most prominent point (*le point le plus saillant*) of the swelling.[1] This rule of practice, which seems to have been very successful in M. Martin's own hands, would, if universally followed out as a mode of evacuating the collection, sometimes lead to an irreparable and fatal blunder. In the case that I have detailed, the most prominent hypogastric point of the swelling was, as we have seen, formed by the uterus itself, as demonstrated by the bougie. The use of the instrument would easily guard against such a mistake as applying—as might have happened in such an instance—the caustic or knife over the uterus, instead of over the purulent cyst itself.

In these cases of subacute or chronic purulent collection in the pelvis after delivery, the pus seems to be in the first instance effused beneath the peritoneum of the uterine appendages, and between it and the pelvic fascia. The original inflammation appears to be seated in the structures intervening between these two membranes. I have seen several cases in the female of chronic "pelvic inflammatory tumors" of the same seat and nature, unconnected with the puerperal state, and where the fixed state of the body of the uterus, the surrounding tumefaction, and the apparent, almost ebony induration produced in the roof of the vagina at one stage of the disease, by the tenseness and distension of the pelvic fascia, gave rise to the idea that the affection was organic and carcinomatous, and not simply inflammatory. I may revert to this subject in a future part. Already the present digression on it is much too long.

2. *Normal Elongation of the Puerperal Uterus as a Sign of Delivery.*—Immediately after delivery, the uterine cavity is in general from six to eight inches in length. I have seen it measure between eight and nine inches in three women, who each died within twenty-four hours after delivery, with the organ imperfectly contracted. In one of these cases death apparently resulted from the

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Rust's Magazin für die Ges. Heilkunde, Bd. xli. s. 311; Imbert in his Traité des Maladies des Femmes, tom. i. p. 160; Dupuytren in his Leçons Orales, tom. iii. p. 347; Meissner in Kleinert's Repertorium, Bd. vi. 1842, s. 38; Charlton in the Edinburgh Monthly Journal for 1841, p. 329; Doherty in the Dublin Journal for 1842, p. 199. I observe that in the last or September number of the Dublin Journal, my friend Dr. Churchill has published an excellent memoir on this affection, but does not seem to be aware that the disease has already attracted the attention of many modern writers.

[1] Mémoires de Médecine et de Chirurgie Pratique, p. 312.

introduction of air into the venous circulation through the free openings of the uterine sinuses; the second sank after a twin labor and severe hemorrhage; the third died about six hours after delivery, having labored under convulsions and deep coma during the whole previous day; and in both these last cases extensive and well-marked granular disease of the kidney was found on dissection.[1] In the course of the natural changes of the puerperal state, the uterus gradually diminishes and regains its natural size in the course of four or five weeks, and, in some, not till a longer period after parturition. In two cases which I have lately seen, where it was requisite for the purposes of criminal law to ascertain the existence of the signs of delivery, the uterine cavity was, about the seventh day after the accouchement, still between four and five inches in length. In a third case, in which the date of the parturition is still uncertain, two most intelligent medical men gave in a report, certifying the existence of all the ordinary signs of delivery upon the body of a woman nearly fifty years old, with the single but important exception, that no uterine tumor could be felt by them above the pubis, probably in consequence of the organ being so flaccid or so low in the pelvis, as not to be felt by the common hypogastric examination. Eight days subsequently, I saw the accused along with one of the medical reporters, and at that time found that the uterine cavity still measured fully four inches in length, and that the fundus of the organ could be pressed easily forwards by the end of the introduced bougie, so as to make us both perfectly certain of the existence of enlarged uterine tumor. This completion of the evidence was the more satisfactory, as the person still pertinaciously denied the crime of concealment in connection with child-murder, on an accusation for which she was at the time incarcerated, and the proof, independently of the facts ascertained by the medical witnesses, was very vague and uncertain.

In this last case, the use of the bougie proved the presence of the enlarged uterus, when, as long as a week previously, it could not be felt above the pubis by an abdominal examination carefully conducted in the ordinary mode. It showed the uterine tumor to exist when it could not be felt by the usual means of examination. In other instances of the same kind, it may prove useful in the converse way, by demonstrating the tumor that may be felt, not to be formed by the enlarged uterus. Dr. Montgomery, in his essay On the Signs of Delivery, justly states, that "the chief points of attention ought to be the state of the *uterus*, of the external parts, and of the breasts."[1] But in regard to the first of these, as felt above the pubis, he properly remarks, "a tumor may be felt so situated, and yet may not be the uterus."[2] The introduction of the bougie into the uterine cavity would at once decide this point.

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[1] The third case alluded to in the text, offered me the first opportunity of confirming, by inspection after death, an opinion that I had been led to adopt from the examination of the symptoms during life, and had publicly taught for the last two sessions, viz., that patients attacked with puerperal convulsions had almost invariably albuminous urine, and some accompanying, or rather preceding, dropsical complications, and hence probably granular renal disease. Pathologists are now well aware of the occasional great tendency to convulsions and other head symptoms in patients affected with Bright's disease, and of the influence which that affection has over the progress of both medical and surgical affections. The present occasion is not a proper field to discuss its bearings in relation to parturition and the puerperal state, or otherwise its importance might be easily illustrated. A very able and zealous investigator (Mr. Lever of Guy's Hospital) has already so far entered on the inquiry, by pointing out that, as in one of the cases adverted to in the text, inertia of the uterus and hemorrhage are sometimes the accompaniments, if not the consequences, of an albuminous state of the urine, and granular disease of the kidney.—See Guy's Hospital Reports, vol. vii. 1842, p. 325.

In any doubtful case, the evidence derivable from the measurement of the uterus might be made the more complete by repeating it from time to time, so as to note the gradual diminution of the length of the organ, till it ultimately returned to its natural size.

This sign, like all the other proofs of delivery, can never be relied upon alone, but must always be taken in connection with the other data that are present. It may be considered, however, as an addition to their number, which is the more valuable in this respect, that it can be ascertained at a date later than most of them. To render its evidence still more certain and precise, it would be necessary to know the general rate of diminution in the length of the uterine cavity after delivery, and its variations. An extensive series of observations, both upon the living and dead subjects after delivery, could alone fully determine these points. The examinations of puerperal uteri after death, which have been recorded by Ruysch, Roederer, Montgomery, &c., will aid in such an inquiry.[3]

In cases of feigned delivery, the sign that we have pointed out from the measurement of the uterus, might be equally useful in demonstrating the organ to be of its normal length and dimensions.

3. *Increased length in Metritic and Congestive Hypertrophy of the Body of the Uterus.*—When the body and fundus of the uterus are

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[1] Loc. cit. p. 317.

[2] Ibid. p. 307.

[3] I may add, that in one of the women above alluded to, and whom I saw in prison, along with Dr. Graham Weir, we detected another sign of delivery, not hitherto pointed out, as far as I know, by any medical jurist. All of them mention the value of the evidence afforded by the tactile examination of the os uteri. The *visual* examination of this part with the speculum afforded in the instances alluded to a still stronger proof. The swollen, ecchymosed, and gaping state of the labia and os uteri presented such striking peculiarities, as made it almost impossible to confound it with any other morbid condition of the parts, such as the inequalities, ulcers, &c., attendant on syphilis, and against the similarity to which as ascertained by touch, Gardien (*Traité d'Accouchemens*, tom. i. p. 132), so particularly guards us.

the seat of any continued morbid irritation, the walls of the organ become hypertrophied in the same manner, though by no means to the same degree, or with the same uniformity, as under the continued normal irritation of pregnancy. The most common pathological cause of uterine hypertrophy, when uncomplicated with organic disease, is chronic congestion or metritis. It happens in hypertrophy from this source, that, as in cases of excentric hypertrophy of the heart, the cavity of the organ becomes in general enlarged, along with the enlarged state of the parietes. This enlargement of the cavity generally takes place in all directions, so that it is increased in its length, as well as in its other dimensions. The increase of length or elongation of the uterine cavity, may be ascertained and measured by the bougie. There are as yet few or no precise data which can be referred to, to indicate the degree of hypertrophy and consequent elongation in such cases. In an excellent specimen of simple hypertrophy of the uterus, contained in Dr. William Hunter's museum, the length of the cavity of the organ, from the os to the fundus, is exactly three inches and one-fourth. It is set down in the catalogue, as a "Uterus split open from before; it is the size of the impregnated uterus at two months; the woman, however, was not pregnant, but had the furor uterinus."[1] The hypertrophy and accompanying elongation may be occasionally greater. I have repeatedly found the uterine cavity measuring as much as three and a half inches. When speaking of hypertrophy as a result of chronic metritis, Boivin and Duges observe, that in this state "the uterus is often distended throughout, and its volume assumes the dimensions presented in the second month of pregnancy." And they add, "In some cases it enlarges so as to fill the hypogastrium, and reach the umbilicus."[2] "With this state (of simple hypertrophy)," says Dr. Hooper, "the whole of the uterus is of a preternatural size, more especially the body of the uterus, without any other morbid or unnatural appearance; and this increase of size is caused by an unusual formation of the healthy structure of the organ. With regard to the extent of this unnatural occurrence, I have found the uterus more than twice the usual size; and this may be considered as the mean, or most common size in hypertrophy; but it is sometimes much larger."[3] "I have known," observes Dr. Burns, "the cervix thicker, the body evidently swollen, and, if not deluded, the fundus rising *above* the pubis, and yet the tumor disappear."[4] Dr. Hamilton says he has seen the "uterine tumor as large as the womb in the fifth month, and yet it was removed."[1]

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[1] Printed Catalogue, p. 147, No. 97, s.

[2] Chapter on Chronic Metritis, p. 352.

[3] Morbid Anatomy of the Human Uterus, p. 5.

[4] Burns's Principles of Midwifery, p. 122, 10th edit.

We would qualify these remarks upon the increased admeasurement of the uterus in congestive or inflammatory hypertrophy, by adding that, judging from our own experience of it, probably this mode of physical diagnosis will, in the morbid condition under consideration, be found of more use practically in showing us with sufficient precision the gradual diminution of the organ, and hence the rate of progress towards recovery, under the treatment that we may be following, than in forming by itself, in the first instance, a perfect diagnostic criterion of the original existence of the disease. For we must hold in view, that the length of the cavity of the organ is, from natural conformation, liable in different persons to exceed the usual standard by a few lines, even when no disease is present; and in chronic hypertrophy the increase of the longitudinal dimensions of the uterus will not be to any very marked or notable extent, unless the affection has proceeded to a considerable degree. Besides, there may exist, although we cannot call to our recollection any example of it, a form of metritic enlargement, in which, as sometimes occurs in the heart, the hypertrophy is concentric, and not excentric, to borrow the cardiac nomenclature of Bertin, and where the length of the uterine cavity would consequently not be increased by the presence of the disease.

In employing the bougie in the diagnosis of simple hypertrophy of the uterus, the instrument is of use in other respects than by merely enabling us to measure the length of the uterine cavity. It gives us at the same time the power of examining, in the mode that we have already shown, much more accurately than we could otherwise do, the condition of the fundus and anterior and posterior walls of the uterus, through the abdominal parietes and rectum—so that we can at the same time ascertain, that while the organ is increased in length, its external surface is quite regular, and presents none of the inequalities which indicate the existence of any more fixed or formidable organic change.

4. *Longitudinal Hypertrophy of the Uterus, and especially of the Cervix.*—In that form of simple or metritic hypertrophy which we have last described, the morbid enlargement of the organ affects equally all its dimensions, and increases its breadth as well as its length. Occasionally, however, it happens, that in hypertrophy of the uterus, the increase in the dimensions of the organ is principally, or even entirely in its longitudinal direction, and that in consequence the uterine cavity becomes so elongated, as to afford us

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[1] Pract. Observ. p. 65.

a much more decisive physical sign of its morbid state, when submitted to the measurement of the Uterine Sound. One of the most remarkable examples of this type of uterine hypertrophy hitherto placed on record is that mentioned by Dr. Kennedy, in an excellent paper[1] in the Dublin Journal, as having occurred in a case of extra-uterine gestation. The uterus was "developed in its length, with very little increase of breadth, to the extent of twelve inches."[2]

When this longitudinal hypertrophy takes place in the unimpregnated uterus, it almost always results from morbid elongation of the cervix. The body and lips of the organ are natural in size and dimensions; but the intervening part, or cervix, appears as if its tissues had become ductile, and been extended and drawn out to a greater or less degree. The elongated cervix may either retain its normal diameter, or, as more rarely happens, it is attenuated in thickness nearly in proportion to its increase in length.

Two different varieties of the morbid hypertrophy that we are considering seem to occur, and have not been sufficiently distinguished by authors. In one, the elongated portion of cervix is placed altogether *above* the reflection of the roof of the vagina; in the other, the hypertrophy is situated *below* that reflection, or affects only the vaginal cervix, as it has been termed. In the former variety, the altered uterus is reduced in form to the shape and type of the organ in infancy. We have seen this variety in some cases of complete procidentia of the uterus,[3] and found the uterine canal, as measured by the bougie, to be stretched out to four and five inches in length. Mr. Heming supposes that this form of cervical elongation is most apt to take place in connection with prolapsus of the uterus, "complicated with hernia at its posterior part," whilst the cervix remains unchanged in prolapus "complicated with hernia at its anterior part."[4] I have examined one case where the reverse holds true; the os uteri is prolapsed an inch or two beyond the vulva, and the cervix much elongated; but the posterior wall of the vagina remains in situ, forming a deep reflection behind the protruding tumor; and the anterior wall of it with the bladder forms part of the external swelling. I may add that this variety of hypertrophy is met with also in instances in which the elongation of the body of the organ takes place in connection with the presence of fibrous or other tumors, when they happen to drag and raise upwards into the abdomen the fundus uteri, while the cervix remains fixed. Cruveilhier has represented an example of this complication, in which the canal of the cervix alone is stretched out to the length of five or six inches.[1]

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[1] "On Hypertrophy of the Os Uteri."—Dublin Journal of Med. Science, vol. xiv.

[2] Ibid. p. 221.

[3] See good illustrative sketches of this form of the neck of the uterus in the cases of prolapsus uteri figured in Cloquet's Pathologie Chirurgicale, 1831, pl. viii. fig. 3, and Froriep's Chirurgische Kupferfeln, t. 417, figs. 1 and 4.

[4] Med. and Phys. Journal, vol. lxviii. p. 107.

When the portion of cervix which is below the vaginal reflection is hypertrophied, the elongated structure projects downwards from the vaginal roof, like the cylindroid finger of a glove or a cow-teat. I have had occasion to excise one, in consequence of the constant discomfort which it produced. In another case of this kind, in which the patient has suffered often from recurrent attacks of severe uterine irritation, and has the os uteri almost projecting through the vulva, the neck of the organ is about two inches in length, and the body of the uterus is, as I have ascertained by the sound, acutely bent or retroflected backwards and to the right; and it has its fundus firmly adhering in this anormal position to the pelvic peritoneum, covering a firm solid tumor lodged behind the rectum, and filling up the upper part of the hollow of the sacrum. The shape and consistence of the tumor at first gave me the idea that it was one of those osteo-sarcomatous masses that are sometimes seen in this locality; but the use of the exploring needle showed that the induration was not from any bony deposit. When of still greater size than in these instances, the hypertrophied cervix has prolapsed externally, and by its shape and configuration has been mistaken for the male penis. Some of the cases of spurious hermaphroditism in the human female, as those described by Saviard, Valentin, and Home, appear to have been merely examples of this disease.[2] Its presence generally prevents impregnation; and in fact, the tapering, conical form of the cervix uteri, which is so commonly found in women long married without becoming mothers, is a minor degree of this same diseased state. Ollivier, Lisfranc, and others, have mentioned the frequent connection of this state with sterility; and I have repeatedly had occasion to verify it. In a case that came under the care of Dupuytren, the elongated cervix was reduced to its natural length by the knife. The patient had been previously eight years married, and asked medical advice on account of her want of family. Two months after the operation she became pregnant.

The morbid elongation of the cervix uteri is spoken of by Roux as "a pretty frequent condition (*état assez fréquent*), and one that has been hitherto mistaken for procidentia or prolapsus by the greater number of practitioners."[1] Roux ascribes the first notice of this morbid state to Lallemand and Bichat.[2] It was, however, long before described by Morgagni,[3] Levret,[4] and Hoin,[5] and has since been commented upon at greater or less length by Desormeaux,[6] Murat,[7] Gardien,[8] Dance,[9] Heming,[10] Boivin and Duges,[11] Lobstein,[12] Cruveilhier,[13] Davis,[14] Kennedy,[15] &c. &c.

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[1] Anat. Path. livrais. xiii. pl. 2.

[2] Dr. Todd's Cyclopædia of Anatomy, vol. i. p. 690.

[3] Dumont's Thèse sur l'Agenesie, l'Impuissance, et la Dysgenesie, Paris, 1830. No. 2231; and Archives Gén. de Médecine, tom. xxv. p. 266.

The longitudinal hypertrophy of the cervix uteri is liable to be confounded with other morbid states of the organ. "This disease," says Dr. Kennedy, "has been mistaken for polypus, and its removal has been attended with fatal results. In the hypertrophy of the neck, from the os remaining open, with the exception of its altered position, it is, however, most likely to be mistaken for prolapsus. The detecting, by a carefully conducted examination per vaginam, and, if necessary, by the rectum, the elongated or outstretched neck of the uterus, whilst the fundus of the organ is perceptible of its natural size, and in its usual position in the pelvis, will sufficiently establish the diagnosis."[16] The exact height and position of the fundus uteri, which are thus requisite for the diagnosis in some cases, can only, as we have already shown, be ascertained by the aid of the bougie, when the abdominal parietes are sufficiently dense to oppose a perfect tactile examination at the hypogastrium. In hypertrophy of the cervix, the use, however, of the bougie is still greater in another view. "A sound," says Desormeaux, in his article on "*Allongement du col de l' Uterus*," "carried into the os tincæ penetrates to the depth of five, six, seven inches, or even more." "This last circumstance," he observes, "is primarily important in fixing the diagnosis."[17] Desormeaux is at the same time certainly wrong when he adds that we may thus "easily distinguish elongation of the cervix from complete prolapsus of the uterus, inasmuch as, that in the latter a sound would scarcely penetrate above two inches in depth." In complete prolapsus, the uterus, it is true, does occasionally retain its normal length; but, as I have already stated, the reverse is much more generally the case, and the prolapsus is usually accompanied with cervical elongation of the organ, more especially if its fundus, as it very often happens, has become morbidly adherent to some of the neighboring portions of pelvic peritoneum, during the course and increase of the displacement. Even in simple and incomplete prolapsus, the uterine cavity is not unfrequently somewhat elongated; because that prolapsus in recent and subacute cases is very often the *result* merely of the general hypertrophy and increased weight of the organ; and here its increased length may so far form a valuable sign, in showing that in the treatment we must act against the hypertrophy, as the pathological cause, before we can hope to ameliorate the prolapsus, which is simply its mechanical effect.[1]

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[1] Preface to Bichat's Anatomie Descriptive, tom. v. p. 7.

[2] Ib. tom. v. p. 282.

[3] Epist. xlv. obs. 11.

[4] Jour. Ancien. de Méd. tom. xl.

[5] Journal de Médecine, tom. xl. p. 352.

[6] Dict. de Médecine, tom. ii. p. 12.

[7] Dict. des Sciences Méd. tom. xxxi. p. 186.

[8] Traité d'Accouchemens, &c., tom. i. p. 118.

[9] Archives Générales, tom. xx. p. 524, 1829.

[10] Medical and Physical Journal, vol. lxviii. p. 107.

[11] Loc. cit., tom. i. p. 193, &c.

[12] Path. Anat. tom. i. p. 57.

[13] Anat. Path. livraison xxxiv. pl. 2.

[14] Obstetric Medicine, p. 208.

[15] Dublin Journal, vol. xiv. p. 322.

[16] Ibid.

[17] Dictionnaire de Médecine, tom. ii. p. 13.

When the cervix is elongated downwards from the upper part of the vagina, we may easily measure by the uterine sound, and by a finger in the vagina, the exact extent of its morbid prolongation; and, on the other hand, when the hypertrophied portion is situated above the vaginal roof, we may attain the same object by tracing with a finger, in the posterior reflection of the vagina, or in the rectum, the progress of the sound as it passes upwards through the elongated and tapering cervix, until it reaches the entrance (and there the canal is generally contracted) of the cavity of the broader and bulging out body of the uterus itself. In making such an examination we must guard against the error of supposing the instrument to be fully introduced into the cavity of the body of the uterus when it has only reached the upper part of the occasionally dilated cavity of the cervix. Lobstein describes an instance of the disease in which the error might have been easily fallen into. "The museum," he observes, "of our faculty (at Strasburg) contains a very interesting example. The neck of the uterus has a length of three inches and one line, its thickness as its superior part is six lines; in descending it becomes enlarged, and its cavity dilated so as to give it a size of an inch and seven lines. At first view it looks like a piece composed of two uteri touching each other by their necks; no change of tissue is discoverable."[2]

5. *Hypertrophy of the Uterus and Uterine Cavity, from the growth of Fibrous Tumors in the Parietes of the Organ.*—When fibrous tumors, the most common of all the organic diseases of the uterus,

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[1] The partial prolapsus, which often takes place in metritic hypertrophy, "furnishes," observes Duparcque, "the means of assuring ourselves more directly of the state of the uterus. Unfortunately, practitioners rarely know how to profit by it. They consider solely the prolapsus, and apply a pessary, and they are astonished when it cannot be borne, or that so far from putting an end to the various pains and uneasiness which they attributed to the prolapsus, the presence of the instrument exasperates them, or becomes even the exciting cause of more serious alterations."—Traité sur les Alterations Organiques de la Matrice, p. 201.

[2] Traité d'Anat. Pathologique, tom. i. p. 57.

grow in the walls of the viscus, they are generally accompanied with a hypertrophied condition of the uterine parietes, exactly resembling in its nature and appearance their hypertrophied state in pregnancy. Under the irritation of an isolated fibrous tumor or tumors the surrounding hypertrophy is usually local in its seat, and confined to the uterine walls in its immediate vicinity; when the fibrous masses are larger or more numerous, the uterine hypertrophy becomes more general.[1] I have a preparation of a case of this kind, in which the organ resembled in size, and in the thickness and characters of its parietes, the uterus of the female a day or two after delivery. "The natural substance of the uterus," observes Dr. Hooper, "in which this tumor is imbedded, is almost always found to have undergone a decided change, having become more distinctly fibrous, and the fibres more obviously fasciculated. The quantity of these fibres, that is of the natural fleshy fibres of the uterus, is very much increased, so that the diseased structure is often surrounded by walls much thicker than those of a healthy uterus." "In these instances," he remarks in another page, "the uterus very much exceeds its natural bulk and weight after the morbid structure has been completely removed. I have found the uterus in many of these cases weigh two pounds, after having dissected out masses of subcartilaginous (fibrous) tumors or other substances."[2]

In cases of fibrous tumors leading to uterine hypertrophy in the manner just described, the cavity of the uterus is frequently enlarged, as well as its parietes. "When fibrous tumors," says Bayle, in his admirable essay on this disease, "are numerous or very large, they deform altogether the body of the uterus. Almost always when they become so large as a man's head, they enlarge (*aggrandissent*) the uterus in every direction, and dilate considerably its cavity (*dilatent considerablement sa cavité*)."[3]

The cavity of the uterus, however, is not always enlarged, even when the fibrous tumors are both large and numerous. This result in regard to the cavity, seems to be regulated by the seat and relations of the tumors. We have observed in the disease the three following conditions of the uterine cavity:

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[1] "The hypertrophy of the uterus is," observes Cruveilhier, "general when the fibrous tumor directs itself towards the side of the uterine cavity; it is partial when the tumor follows an opposite course; if it occupies the fundus, it is the fundus alone which becomes developed, and the rest is moulded upon it; if the anterior wall is its seat, it is the anterior wall which becomes developed; if it approaches the peritoneum, the fibres intervening between it and that membrane undergo the change. In the cases of partial hypertrophy of the uterus, the remainder of the organ may be in its natural state, but this is rare, because if the uterus is not solicited to development in the direction of its thickness, it is so in the direction of its *height*, in consequence of the slow or rapid increase of the fibrous bodies. Now a development in height or length is still always a hypertrophy."—*Anat. Pathol.* livr. xix. pl. 1, 2.

[2] Morbid Anatomy, pp. 11 and 6.

[3] Dict. des Scien. Méd. tom. vii. p. 72.

First, The cavity is elongated and enlarged with the enlarging state of the tumor and uterine parietes, when the morbid mass grows from any point towards the interior of the organ, or when it is originally situated in the lateral walls of the body of the uterus, and so during its development tends to stretch out these walls, and consequently the contiguous cavity along with them, in a longitudinal direction.

In cases in which the fibrous tumor is seated on one side or walls of the organ, and becomes of a great size, the lengthened uterus sometimes comes to be wrapped and welded for some distance around the exterior surface of the fibrous growth. "I have seen," says Cruveilhier, "in these cases the uterus considerably elongated, and form a kind of a half-belt (*demi-cintre*) around the tumor."[1] The degree of elongation and dilatation of the uterine cavity under these circumstances is very various. I have repeatedly found it, both upon the living and dead subject, measuring three, four, and five inches in depth. Madam Boivin describes and represents a uterus of a conical shape, containing in its parietes several fibrous tumors, and having its cavity prolonged in the form of a narrow canal, to the length of nine inches.[2] I have in my museum a preparation of this kind, in which the uterine canal measures above ten inches. The greatest elongations of the uterine cavity that I have as yet ascertained by the bougie in cases of fibrous tumors in the living subject, are two instances where the canal was, in the one six inches in length from the os uteri to the fundus, and in the other seven and a half inches. Dr. Beilby saw with me the former, and Dr. Girdwood of Paddington the latter case, and both of these gentlemen assured themselves, by personal examination with the uterine bougie, of the accuracy of the measurements that I have stated. This hypertrophy of the walls and cavity of the uterus seems liable to occur in its greatest degree when the tumor or tumors are rapid in their growth, and make their appearance during the childbearing periods of life. In using the bougie in these cases the instrument sometimes requires to be so much unbent as to be nearly straight before it can be introduced; and where the elongated uterus curves around the exterior of the fibrous mass, the apex of the sound must be passed so as to follow the particular direction of the uterine cavity.

Secondly, The uterine cavity may retain nearly its natural dimensions and depth, or be only very slightly increased, even when the fibrous tumors are both large and numerous, provided they grow from any part towards the exterior surface of the organ, or are originally seated in the fundus uteri and not in its lateral parietes. I

[1] Anat. Path. livr. xiv. pl. 1, 2.

[2] Heming's Boivin and Dugès, p. 179, pl. 14.

have seen a mass of uterine fibrous tumors so large as to have been mistaken by one practitioner for an enormous hepatic, growth where the uterine cavity was only about three inches in depth. In this instance the tumors were evidently all growing from the peritoneal surface of the uterus, and could be felt through the thin abdominal walls adhering to it with pedicles of varying sizes, like a number of exterior polypi.

Thirdly, In connection with the existence of fibrous tumors in the uterus, the uterine cavity may be found apparently shortened in consequence of some portion of it being obliterated by adhesive inflammation from the pressure together of its opposing surfaces by the presence of the tumors. I shall state afterwards a case of this kind, where the uterine canal, as examined from the vagina, appeared only an inch in depth. In this, as in most other such cases, the cavity of the body was found concealed higher up, enlarged to some degree, but forming a short cavity, from the obliteration of its inferior part. This result, as the effect of the presence of fibrous tumors, is principally observed when the tumors have been of very long standing, and when the hypertrophy of the uterine tissues that accompanied their earlier development has at last been followed by a state of atrophy of the included and compressed walls of the organ.

The practical deductions that may be drawn from the preceding remarks regarding the length of the cavity of the uterus in fibrous tumors of the organ, are so evident as to require little or no comment. I have already stated my opinion, and would repeat it here in the strongest terms, that no error is more common in practice than to mistake a fibrous tumor of the uterus for a dropsical ovary, or a dropsical ovary for a fibrous tumor.[1] I shall take another opportunity, in these contributions, of offering some suggestions for the more perfect diagnosis of ovarian tumors and their connections. In the mean time I may merely state that in the *lengthened* state of the uterine cavity, as easily ascertained by the bougie during life, we have in many instances, an additional physical sign between the two classes of diseases alluded to, subject to exceptions which I will subsequently mention ; and if the elongated uterine cavity is discovered by the bougie to run *behind* the tumor that is present, it is another and still more decisive reason for concluding that the affection is uterine and not ovarian. At the same time it is to be held in recollection that if the uterine cavity is of its *natural* depth, it is no sufficient reason that the hypogastric or abdominal growth is not a fibrous uterine tumor, and more careful examination under such circumstances will often show the tumor or tumors to be such, and to have either a more or less pediculated form, and attachments to the external uterine surface, or to be imbedded in the walls of the fundus. Lastly, the uterine canal is sometimes greatly *shortened* in the way described, in connection with the existence of fibrous tumors in the uterine walls. I am not aware that it is ever in this way diminished in depth from the presence of ovarian disease.

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[1] In illustration of this remark, I shall content myself with adducing one fact. Few men in England seem to have attended more zealously to the subject of ovarian tumors, and no one has labored so much to reintroduce the excision of them, as Mr. Clay of Manchester. Yet out of five cases which he has lately recorded, of operations for the extirpation of enlarged ovaries, in two it was found, after the abdomen had been laid open, that the disease was not ovarian, but consisted "of anomalous and uterine tumors."—See his Cases of Peritoneal Section for the Extirpation of Diseased Ovaria, p. 18.

6. *Enlargement and distension of the Uterus from Polypi, &c., in its Cavity.*—When polypi or other morbid structures form in the cavity of the uterus, they act like the presence of an ovum, enlarge the interior of the organ in proportion to their growth and size, and generally at the same time induce, as in pregnancy, a corresponding degree of thickening and hypertrophy in the uterine parietes. In such cases the Uterine Bougie shows an increased depth of the uterine cavity. The degree of increase will necessarily vary in every different instance. I have seen a retained polypus enlarging the uterus to the size of the organ at the fourth month of pregnancy. Cruveilheir has represented a large fibrous polypus as included within and distending the uterine cavity, so that the organ measured above eight inches from the os to the fundus. We shall see afterwards that it is not the mere length to which the Bougie may pass that forms the diagnostic mark under such circumstances, but that it is by the possibility of passing it on more than one side between the surface of the tumor and the interior of the uterus, or by the power of so far revolving it round, and isolating, as it were, the contained mass, that we draw the distinction between tumors situated in the cavity and those which, as described under the last head, are still imbedded in the parietes of the organ.

7. *Elongation of the Uterus in Hernia of the Organ.*—Hernia of the unimpregnated uterus is a disease of very rare occurrence. The organ, however, has been found protruding into the tumors formed by different varieties of abdominal hernia. It had passed through the crural ring in cases met with by Lallemand[1] and Cruveilhier;[2] through the inguinal canal, in instances detailed by Desault and Chopart,[3] and Lallemand.[4] Most of the cases of hernia of the impregnated uterus which have been recorded by Sennertus, Hildanus, Ruysch, Ladesma, Fisher, &c.,[5] were instances in which

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[1] Bulletins de la Faculté de Médecine, 1816, tom, i. c. 1.

[2] Anat. Pathologique, livr. xxxiv. pl. vi.

[3] Traité des Maladies Chirurgicales, tom. ii. p. 305.

[4] Mémoires de la Soc. Méd. d'Emulation, 3me année, p. 323.

[5] See the details of them collected by Dr. Cormack, in Edinburgh Monthly Journal for 1841, p. 491, and ibid. for 1842, p. 28.

the uterus seems to have become displaced after conception took place.

When the unimpregnated uterus forms the subject of hernia, it seems in general to be considerably elongated. In Chopart's case the organ is described as smaller than usual, rounded and lengthened in form (*arrondie, alongée*); in Cruveilhier's plate the displaced viscus is represented as drawn out to the length of five or six inches. In fact, the uterus itself must in all such cases be either much drawn out in length, in order to allow its fundus to be the subject of such great displacement, or the vaginal canal must be extended into the neck of the hernial sac, as happened in the example already referred to, as recorded in the *Bulletins de la Faculté de Médecine.*

In one of the cases of hernia of the uterus described by Lallemand, the patient applied for advice in consequence of the supervention of symptoms of strangulation. The sac, on examination, was supposed to contain a portion of intestine: but the age of the patient, &c., prevented an operation being attempted. On examination after death, the hernial tumor was found to be composed of two folds of omentum, two hydatic cysts, the ovaries, Fallopian tubes, and uterus. In relation to the difficulty of the diagnosis in this and similar cases, Murat observes, that "hernia of the unimpregnated uterus is most frequently confounded with that of other parts of the abdomen, and there are *no* pathognomonic signs by which we can recognize it."[1] We may, as Nauche advises, so far overcome the difficulty, by finding, on vaginal examination, "the uterus situated high in that canal more or less devious, the orifice turned to the side opposite that of the hernia, and on pressing it with the finger, we impart a certain mobility to the hernial tumor."[2] A much more decisive means of solving the problem will be afforded by the use of the Uterine Bougie. If, instead of trusting to the preceding points as ascertainable by the examination of the os and cervix with the finger, we pass the Bougie into the uterine orifice and its elongated cavity, both the direction which the instrument will, during the course of its introduction, take towards and into the mass of the tumor, and the power of feeling its apex in the fundus uteri, through the hernial walls, after it is fully introduced, will at once place the matter beyond the possibility of doubt. In this procedure it will be proper to recollect, that in some cases a slender, and it may be even a flexible Bougie, will be required, in order to overcome the contractions and irregularities of the uterine cavity that may be present.

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[1] Dict. des Sc. Méd. tom. xxxi. p. 228.

[2] Maladies de l'Uterus, p. 114.

SECTION IV.

ON THE MEASUREMENT OF THE CAVITY OF THE UTERUS AS A MEANS OF DIAGNOSIS IN SOME OF THE MORBID STATES OF THAT ORGAN.—*Continued.*

(From London and Edinburgh Monthly Journal of Medical Science, March, 1844, p. 208.)

In our last communication we described a number of morbid conditions of the uterus, in which the cavity of that organ is more or less elongated. We showed that the diagnosis of these affections could be greatly advanced by ascertaining, through the use of the Uterine Bougie, the exact extent and degree of the existing elongation. It was at the same time remarked that in some pathological states of the uterus its cavity is shortened and diminished in depth. On the present occasion, we purpose to describe briefly this latter set of cases, and will point out in what respects their discrimination during life may be promoted by the employment of the Uterine Sound.

INSTANCES OF DIMINISHED LENGTH OF THE UTERINE CAVITY.

The cavity of the Uterus, when shorter than natural, may have its depth diminished as the result of malformation, of disease, or of displacement.

1. *Preternatural shortness of the Uterus from original malformation of the organ.*—Few of the malformations of the uterus, with the exception of the duplicity and absolute deficiency of the organ, have as yet attracted much attention. It would, however, be easy, we believe, to bring together, by a little patient research, a considerable series of cases, in which the organ was found less than its normal length by an inch or an inch and a half.[1] In some instances, in fact, the cervix of the uterus is alone present, and the body and fundus of the organ are imperfectly or totally undeveloped. In more strict terms, the lower extremities of the Fallopian tubes, instead of coalescing and becoming evolved into the body and fundus of the uterus, remain separate, retain their tubal character, and open into the superior part of the single cavity of the cervix. Such appears to us to be the explanation of the case, for example, described by Lauth, in which the cervix was sufficiently well formed, with the tubes opening into it almost directly, and only separated by a small cavity.[2] Morgagni speaks of a uterus in which the distance from

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[1] For further remarks on this subject, see p. 113.

[2] Andral's Anatomie Pathologique, tom. i. p. 677.

the os to the fundus was not so much as the breadth of the thumb. The genital organs were in other respects malformed.[1] Dr. Dewees mentions an instance of amenorrhœa, in which the uterus was of "a size not exceeding the thumb of a man."[2]

In that particular variety of malformation which various pathologists have described under the name of "oblique uterus," and where the organ originally lies with its fundus directed to one side of the pelvis, and has one set of its lateral ligaments shorter than another, the organ is sometimes, though not always, shorter than natural. Tiedemann describes an oblique uterus two inches and a line in length, and in which the cavity would measure considerably under two inches.[3]

In all cases in which the uterus is malformed and shortened in the modes we have alluded to, the Uterine Sound will probably afford us important diagnostic information, by enabling us to measure the exact degree of diminution in the length of the cavity of the organ. To make the information more certain and precise, it will be necessary at the same time to ascertain, by a hypogastric or rectal examination, that the apex of the instrument has really reached the superior end of the uterine body, and hence, that it is not arrested in its progress by any structure in the cavity, or by any flexion in the walls of the viscus, such as we shall afterwards fully describe.

2. *Uterine canal shortened from stricture or partial obliteration.*— "A stricture," says Dr. Baillie, "is sometimes formed within the cavity of the uterus, so that its cavity at one part is obliterated entirely. This," he adds, "I believe almost always to take place at one part, namely, where the cavity of the fundus uteri terminates, and that of the cervix begins, for in this place the cavity of the uterus is narrowest. As the sides of the cavity round this place lie very near each other, and form naturally a small aperture, it is probable that some slight inflammation may unite the parts together, and shut up the aperture; or the parts may gradually approach each other without this cause, as in the stricture of the urethra."[4]

The morbid state which Dr. Baillie has so accurately described in the above quotation, as the result of his observations upon the dead body, could only be detected upon the living subject, by examination with a uterine probe or bougie. Nor would the diagnosis be difficult, for at the same time that it was found that the Bougie was completely arrested in its progress upwards, at the distance of an inch, or an inch and a half from the os tincæ, it might be ascertained, by a hypogastric examination, that it was still far from having reached the fundus uteri.

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[1] Epist. xlvi. 20, Alexander's Translation, vol. ii. p. 661.

[2] System of Midwifery, p. 69, ed. of 1837.

[3] Von den Cowperschen Drusen des Weibs, &c. p. 26.

[4] Morbid Anatomy, edit. of 1812, p. 379.

Since the time that Dr. Baillie wrote, Professor Mayer of Bonn has shown that in old persons, the os internum or cervico-uterine orifice is so often obliterated, that it may be almost looked upon as a normal condition, connected with the general atrophy of the viscus that takes place in very advanced life.[1] In one or two aged patients I have found it impossible, during life, to pass the smallest probe through the os internum, probably owing to the contraction in question. The cases I allude to were those of females who in earlier life had produced children, and where there was therefore no original stricture or malformation.

Stricture of the os internum is, like diminution in the size of the os tincæ, not unfrequent in females affected with dysmenorrhœa, and who, though married, have never had children. In several cases I have met with difficulty and obstruction in passing through the opening between the cavities of the neck and body of the uterus, a sound or probe that had already passed easily and freely through the os tincæ. I have at present under my care a case of this kind, in which there is a most remarkable degree of antroversion of the whole uterus. Jahn, in his Essay on the Oblique Uterus, mentions an instance of that malconformation where, after death, the os internum was found so narrow that a fine probe could not be passed through it (*ut subtilissimo stilo transitum denegaret*); and yet the os tincæ was well formed.[2] In Ruysch's Catalogue a uterus is mentioned, with the os internum so small as not even to admit the head of a small needle (*ne caput quidem aciculæ minoris admittere posset*).[3] Occasionally the whole cavity of the body of the uterus is obliterated, from the os internum to the fundus, and yet the cavity of the cervix continues patent. Cruveilhier speaks of having seen a case of this kind, "in which there was no trace of a cavity in the body of the organ, although the cavity of the neck remained."[4] An example of the same kind has been represented by Madame Boivin in the plates attached to her treatise on diseases of the uterus.[5]

The cavity of the uterus is, in some rare cases, partially obliterated, in consequence, as has been already stated, of the development of fibrous tumors in the walls of the organ. Under such circumstances, the obliteration is the result of inflammatory adhesions, formed between those portions of the opposed surfaces of the lining membrane of the uterus, that happened to be maintained in a state of close and morbid apposition, by the presence and pressure of the neighboring tumors. The adhesive inflammation thus excited occasionally extends to some distance from its original seat. In all cases, however, the cavity of the cervix seems to escape, and the portion of the cavity of the body that is placed above the stricture frequently becomes distended and enlarged, from the accumulation of morbid mucous secretions within it. Cruveilhier has described a very illustrative case of this kind.[1] A preparation was presented to the Anatomical Society of Paris, as a morbid ovary. In external appearance, it had much more resemblance to an ovarian than a uterine disease. On more minute examination and dissection, however, it was found to consist of an agglomerated mass of fibrous and fibro-calcareous tumors, the whole forming a shapeless body, with an irregular, tuberculated surface. The tumors varied in size, from a pea to a large compound one weighing by itself about 1½ pounds. On cutting into the midst of the mass, a cavity was found filled with reddish serum, which it was easy to see was the cavity of the body of the uterus. This cavity was a shut sac, there being no opening from it. Inferiorly, its communication with the cervix uteri was entirely obliterated. When examined as to its vaginal relations, the uterus, or uterine canal, seemed to terminate at an inch above the os tincæ. A blunt stilet, adds Cruveilhier, passed in all directions, could not discover above this point any opening into the uterine cavity. If the same means had been employed, as a matter of diagnosis during life, the same result would have been obtained as in the post-mortem examination, and that result would have afforded pretty conclusive evidence that the disease was not ovarian, because the uterus does not seem to be liable to become occluded in the course of its cavity, in connection with ovarian growths; and besides, the use of the Bougie would have shown the uterus imbedded in the tumor, and probably the partial canal of the cervix running in such a direction in relation to the tumor as to add further testimony to its non-ovarian character.

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[1] Beschreibung einer Graviditas Interstitialis Uteri, p. 14.

[2] De Situ Uteri Obliquo, in Schlegel's Sylloge Operum Obstetr. tom. i. p. 268.

[3] Thesaurus Anatomicus, vi. No. 85.

[4] Descriptive Anatomy, vol. i. p. 621, of Dr. Madden's translation.

[5] Loc. cit. pl. xiii. fig. 3.

3. *Diminished depth and effacing of the Uterine Cavity in Inversion of the Uterus.*—We believe that the diminution in depth, and, in extreme cases, the total obliteration which the canal of the uterus undergoes in inversion of the organ, will generally give us the power of distinguishing this morbid state from all others to

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[1] Anatomie Pathologique, livrais. xiv.

which it is symptomatically allied; and more especially from those forms of polypus that occasionally so strictly resemble it, and with which it has been frequently and sometimes fatally confounded.

When inversion of the uterus occurs immediately after delivery, it can, in general, be easily distinguished from a polypus that has passed through the os uteri after the expulsion of the child, or placenta, or both, because, omitting other considerations, though in each we may find in the vulva or vagina, if we are led to make an examination by the severity of the symptoms, a large fleshy tumor, yet this tumor, in the case of polypus, can be proved not to be the displaced uterus, as the fundus uteri can still be felt above the pubis, whilst the reverse is true of inversion. If any doubt remained, in consequence of difficulties in the way of the hypogastric examination, or otherwise,[1] a Bougie cautiously passed by the stalk of the vaginal tumor, would, if it were a polypus, prove at once the uterine cavity to extend upwards to the length of six or seven inches, and if desired, would enable us further to feel more distinctly through the hypogastrium, the fundus uteri still remaining in situ. If the tumor were the inverted fundus of the uterus, the cavity of the organ would, on the contrary, be found shortened to an inch or two on all sides, and it would be found difficult, or impossible to bring, as usual, any part fully within the reach of an abdominal examination. In those rare instances in which the inversion was complete, the orifice and cavity of the uterus would be found entirely effaced.

The difficulty, however, of distinguishing between polypus and inversio uteri is greatly increased when the diagnosis is attempted after the puerperal uterus has diminished to its normal volume, or when the patient applies for advice at a distance from the period of confinement, and the inversion is only partial. Under such circumstances it has repeatedly happened that a polypus has been mistaken for an inverted uterus,[2] or the still more dangerous error committed of considering an inverted uterus to be a polypus, and treating it accordingly.[3] Such errors involve not only the comfort but the life of the patient.

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[1] For cases of polypus of the vagina after delivery mistaken for inversion, see Ramsbotham's Practical Observations, vol. ii. p. 473, and Gooch's Account of Diseases of Women, p. 282; and for cases of inversion under the same circumstances, mistaken for polypus, see Gazette Médicale for 1832, p. 422.

[2] See cases in Ausiaux Clinique Chirurgicale, p. 207; Acta Havniensia, 1818, tom. v. p. 51.

[3] On cases of inverted uteri mistaken for polypi, see Morgagni, de Sedibus, &c., epist. xlv. No. 4; Palletas' Exercitationes Pathologicæ, pp. 17, 18. Gooch in his Diseases of Women, 1831, p. 265, gives an instance where Dr. Hunter applied by mistake a ligature to an inverted uterus. A preparation illustrative of this error was in the museum of the late Dr. Hamilton; the patient, as in Dr. Hunter's case, having died with the uterus partially cut through with the ligature. Occasionally the uterus has been included without fatal effects; see numerous quotations in Dr. Burns's Principles of Midwifery, 1837, p. 128.

It is true that a diagnosis can generally be established with considerable certainty between polypus of the uterus and partial chronic inversion of the organ, by attending, amongst other points, particularly to the two circumstances, that however similar in other respects, the tumor formed by inversion is, 1st,—in its history, traceable to having appeared immediately after delivery; and, 2d, in its character, it differs from polypus in being sensitive to a greater or less degree to the touch, while the polypus is not so. We must, however, at the same time, hold in view that some fibrous polypi, when they carry down before them and are covered with a layer of true uterine tissue, or when temporarily inflamed, are found to be sensitive,[1] and that the inverted uterus becomes almost insensible when long exposed;[2] and again the fact, that the tumor first appeared immediately after delivery, is not itself conclusive, because in some cases of inversion the accident has not caused such severe symptoms as to be recognized at that moment,[3] and again, the first time a polypus passes from the uterus to the vagina is occasionally immediately after labor.[4] In their physical character, the two diseases often very nearly resemble one another. "When the uterus," says Dr. Gooch, "is only partially inverted, that is, when its fundus only is drawn down through its orifice into the vagina, and the patient has survived for many months, the tumor feels exactly like a polypus of the fundus. . . . In the smoothness of its surface, the roundness of its body, the narrowness of its neck, and its being encircled by the orifice of the uterus, it sometimes *exactly* resembles polypus of the fundus."[1]

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[1] "It is said that an inverted uterus is sensible to the touch, while polypi, on the contrary, are void of feeling. This can never be an accurate mode of forming a diagnosis, as we can only judge of the sensibility of the tumor by the expressions of the patient, which are regulated more by disposition than by the extent of her sufferings. I lately attended a lady with uterine polypus, and had I judged solely by the complaints of my patient, I should have pronounced the polypus to have been more sensible than an inverted uterus usually is."—Dr. Chas. Johnstone in Dublin Med. Reports, vol. iii. p. 488.

"We shall always find it difficult to distinguish between the sensibility of the tumor and sensation occurring in neighboring viscera, which are irritated by the process of examination; while, too, it must be remembered, that the sensibility of the inverted uterus is greatly diminished in its chronic stage, and that the sensibility of polypus may be increased by the presence of inflammatory action."—Newnham on Inversio Uteri, p. 83.

[2] "It is said that the polypus is usually indolent, and that the fundus of the uterus has an exquisite sensibility, but very often one meets with painful polypi; and it must be so, since they are very often covered with the tissue of the uterus. Their lower part may be insensible, because this envelope is too thin, or it may be perforated. On the other hand, it is certain that the sensibility of the inverted uterus gets duller after a time."—Lisfranc, Clinique de la Pitié, tom. vii. p. 133.

"In time, the surface of an inverted uterus becomes less sensible to external impressions."—Jourdan, Dict. des Sciences Médicales, tom. xxiii. p. 288.

"Dr. Montgomery has in his museum a preparation of inverted uterus which had during life been quite insensible to the application of the needle."—Dr. Burns's Midwifery, p. 561.

[3] "The nature and even the facts of the accident have often not been discovered till after the lapse of many days, weeks, or months subsequently; and in a smaller number of cases not till after the death of the subject."—Davis's Obstetric Medicine, p. 1088.

[4] Two cases of this nature are given by Levret in his paper on uterine polypi, in the Mémoires de l'Académie Royale de Chirurgie, tom. iii. pp. 543, 545.—See also Gardien's Traité des Accouchemens, tom. iii. p. 316; Delpech's Précis des Maladies Chirurgicales, tom. ii. p. 586; three cases in the Journal Hebdomadaire de Médecine, No. 44; Ramsbotham's Observations, vol. ii. p. 473. I have seen one come down after a premature labor.

To show still more strongly the difficulties which occasionally intervene in the distinction between chronic inversion of the uterus and polypus, I will adduce the testimony of one or two writers, whose authorities on this point are such as to command all confidence.

Mr. Newnham, in his learned essay on Inversion of the Uterus, after having brought together the opinions of many authors—ancient and modern—British and foreign—upon this question in diagnosis, adds, "On reviewing the foregoing testimony we shall be induced to conclude that it is always difficult and sometimes impossible, with our present knowledge, to distinguish partial and chronic inversion of the uterus from polypus."[2]

In his elaborate work on Operative Midwifery, Professor Kilian offers a nearly similar observation—"An inverted uterus may so deceitfully (*tauschend*) resemble a polypus that the diagnosis is scarcely possible."[3]

In a clinical lecture on inversion of the uterus, Velpeau, in alluding to the diagnosis of that disease from polypus in some very difficult instances, adds, "There are cases in which doubt is the only rational opinion (*le doute est la seule opinion rationelle.*)"[4]

"When," observes Lisfranc,[5] "the polypus or inversion has only partially opened the os uteri, we are assured that the diagnosis is impossible—authors do not even consider the case." "From the facts we have adduced," he adds,[6] "one may easily conceive the immense difficulties met with in cases of this nature; thus under a great number of circumstances, the most distinguished practitioners have believed that they had tied polypi, when they had included the organ of generation itself in the ligature; and in other cases they have supposed they had removed the uterus either wholly or in part, when they had only relieved their patients of polypous tumors."

In order to resolve the difficult problem in uterine diagnosis to which these remarks refer, Malgaigne some time ago proposed a means which is thus described:[7] "A curved catheter is introduced into the bladder; it is then carried backwards, and its concavity directed downwards, so as to bring the point of the instrument into the bottom of the uterine sac; the index finger is then introduced into the vagina, where the point of the catheter is as easily felt as it is in the hypogastrium in ordinary catheterism." Thus the diagnosis is established: "But there are circumstances," continues Lisfranc, "in which the catheter, in consequence of the adhesion of the organs, cannot penetrate into the sac of the inverted uterus; then it might be erroneously believed that we had to do with a polypus." Setting this last objection aside, we doubt entirely the applicability of such a means of diagnosis in this case as that proposed by Malgaigne. The urethra of the female is placed in such a direction in relation to the other pelvic contents, that if the stem of an ordinary male catheter be held in it, the apex of the instrument, when turned backwards, will look posteriorly to the hollow of the sacrum, and cannot, without lacerating the urethral connections, be made to return downwards towards the vagina. To enable it to do so, the instrument would require to be curved at an acute angle, and not at an obtuse—and if of the former shape, it could not be made to pass into the bladder, and even if passed, could not probably be used with safety. We make these remarks after having tried repeatedly the experiment upon the dead subject.

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[1] Loc. cit. p. 255.

[2] Essay on the Symptoms, &c. of Inversio Uteri.—London, 1818, p. 82.

[3] Loc. cit. Bd. ii. p. 280.

[4] Clinique Chirurgicale, Paris, 1841, tom. ii. p. 425.

[5] Clinique, tom. iii. p. 135.

[6] Ibid. p. 136.

[7] Lisfranc, Clinique de la Pitié, tom. iii. p. 137.

A more safe and easy means of distinguishing a polypus from a partially inverted uterus, will be found in the measurement of the uterine cavity, by the introduction of the sound. In some doubtful instances its aid will afford us a certain mode of completing the diagnosis in this, a class of uterine cases in which, above all others, the prognosis and treatment are almost entirely dependent upon the accuracy of our diagnosis.

To avail ourselves of the information afforded by the measurement of the bougie, it is to be held in view that in polypus the depth of the uterine cavity is not necessarily diminished, but sometimes the reverse, while in inversion it is always diminished to a great and notable degree.

In four cases of polypi of considerable size, projecting through the os uteri, and which I have had occasion to remove within the last three months, the Bougie in all, when passed by the side of their pedicles into the interior of the uterus, showed its cavity to be of the natural depth. In some preparations, I have seen the cavity elongated, in consequence probably of its distension by the polypus previous to the protrusion of the latter. On the other hand, in partial chronic inversion of the uterus, with the fundus uteri passed downwards, and projecting through the os, the uterine cavity must be necessarily diminished on all sides in depth, by this doubling up of the organ; and further, this diminution of its depth must be proportionate in its degree to the degree of the inversion, so that ultimately, if the inversion becomes, as sometimes happens, complete, the cavity will of course be entirely obliterated. In a common case of such partial inversion and protrusion of the fundus as is liable to be confounded with a polypus projecting through the os, the depth of the interior cavity would be found diminished at least by more than a half, or might measure on all sides about an inch or less in depth, instead of the usual length of two and a half inches.[1] In making this measurement with the Bougie, it will always be requisite to ascertain accurately that the depth of the uterus is lessened in the same way at every point around the stem of the tumor, otherwise we might mistake a polypus, which by adhering to one side of the cavity, diminishes by the presence of its pedicle the depth of that one side merely, for an inversion of the uterus which diminishes the depth of the cavity equally on all sides. It is for this reason that we believe the employment of the finger alone, to ascertain this shortening of the uterine cavity, as recommended by Boyer[2] and Dupuytren,[3] is quite insufficient. In most cases it is too large a body to be passed without force and pain, if at all, into the contracted cervical cul-de-sac—and even if passed to its full depth, it could never leave us perfectly sure that there was not a narrow communication at some point with the uterine cavity beyond.

As a general rule then, it will, we believe, be found that, in cases of tumors projecting through the os uteri, and when the other symptoms leave any doubt as to whether the tumor be a true polypus or merely the fundus of the organ chronically inverted, the employment of the Uterine Bougie will enable us to decide the diagnosis, and hence also, in a great measure, the prognosis and treatment, by the positive or negative information which it affords with regard to the shortening or non-shortening of the uterine cavity. For—

1. If the Bougie passes into the uterine cavity to its usual depth, of two inches and a half or more, the disease is not inversion of the fundus—a fact, the certainty of which may, while the Bougie is still in utero, be further corroborated, by the fundus in situ being actually felt through the hypogastric walls whilst it is pushed forward on the apex of the instrument, or through the rectum, whilst

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[1] In a case in which the inverted uterus was tied in the Lyons Hospital, under the idea that it was a polypus, the patient died on the fifth day. On the post-mortem examination, the vagina and uterus were of the natural size, and the uterine cavity was on all sides reduced to *seven or eight lines in depth* (dans tous les points de son etendue sept ou huit lignes de profondeur). Petit, in whose practice the case occurred, relates that four "Maitres de l'Art," after a careful tactile examination, all pronounced the inverted uterus in this instance to be a polypus. Recueil des Actes de la Société de Santé de Lyon, 1798, p. 103.

[2] Maladies Chirurgicales, tom. x. p. 583. [3] Leçons Orales, tom. iii. p. 535.

by the same means it is retroflected in the mode already described upon the front wall of the bowel. In this case the tumor is one which is in general safely and easily removable. But—

2. If the Uterine Bougie cannot pass at any point around the stem of the tumor to a greater extent than about one inch, the uterine cavity may be considered as shortened by inversion, and the protruding mass cannot be interfered with, without imminent danger to the patient. When in any case this last point is positively ascertained, another consideration may arise. Is the general shortening of the uterine canal the result of simple inversion of the uterus, or of inversion complicated with, and produced by the attachment of a polypus to the interior of the fundus uteri? The decision of this point may be of the first importance, both as regards the propriety and the safety of any further interference. If the disease be simple inversion, no operation would be attempted, unless under the call of very anxious and urgent reasons. If the inversion be the result of the weight and dragging of a polypus, then by removing the latter, the uterus may itself become replaced, and the patient be restored to the enjoyment of perfect health. In making this additional diagnosis between simple and complicated inversion, the previous history of the patient, and the characters of the tumor, may be sufficient to guide us, but both again may be liable, in particular instances, to lead us into error. The length of the whole interior of the uterus, as made up by the *double* measurement, first of the depth of the cul-de-sac of the cervix, and secondly, of the inverted portion from the roof of this cul-de-sac to the apex of the inverted tumor, may afford us more positive information. When added together, the two measurements will, in a case of simple inversio uteri, not exceed much, if at all, the normal length of the cavity of the organ; in a case of inversion complicated with polypus, they will *exceed* this standard in a ratio proportioned to the size of the polypus and the probable elongation of the uterine tissues which it has produced.

"The *only danger*," says Dr. Gooch, "attendant on the operation on polypus is, that the ligature may include a portion of the uterus."[1] This danger, which has led to a fatal result in many master-hands,[2] will be easily avoided, by the previous use of the Bougie in the way we have suggested, so as to ascertain the presence or absence of any coexistent degree of inversion. If the polypus be found complicated with inversion, but yet has a sufficiently marked narrower part or pedicle connecting it with the fundus uteri, its removal in the usual way, by the ligature or knife, may be safely accomplished, if great caution be employed. If the polypus, however, adheres by a broad base, and is decidedly fibrous or cartilaginous in its structure, the case would probably form an appropriate one for the operation that has been had recourse to in several instances of late for the removal of fibrous tumors on the wall of the uterus—namely, dividing by a longitudinal or crucial incision, the thin layer of uterine tissue, covering the projecting part of the tumor, and afterwards enucleating the mass of which it consists. In this way the source of danger pointed out in the quotation from Dr. Gooch would be so far avoided.

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[1] Loc. cit. p. 264.

[2] See, for example, a case of Dr. Denman's in his introduction to Midwifery, p. 106, ed. of 1816. Herbiniaux, Traité sur divers accouchemens, &c. tom. ii. p. 35, and obs. xvii. &c.

In the preceding observations I have not adverted to the distinction between *complete* chronic inversion of the uterus, where the cavity is entirely effaced, and polypus of the neck or lips of the uterus.

The remaining chapters of the preceding unfinished memoir were, we believe, intended to point out several other diagnostic uses of the Uterine Sound, or Metroscope, and especially the important indications which the *direction* of the introduced sound, and consequently of the uterine cavity, affords in distinguishing the mere displacement of the organ, from tumors in its walls, &c. This subject will be found, however, discussed to some extent in a subsequent memoir on Retroversion of the Unimpregnated Uterus.—(Ed.)

ANTIQUITY OF UTERINE SOUNDS AND PESSARIES.

Last year Dr. Simpson made a communication to the Obstetric Society on the History of the Uterine Sound and of Intra-uterine Pessaries. He showed that the introduction of a probe or sound into the os and cavity of the uterus is repeatedly mentioned in the Hippocratic writings; as is also a form of intra-uterine pessary, worn for some days in the uterine cavity. A uterine probe or bougie is alluded to also in Avicenna and some Arabian authors, and in the sixteenth and seventeenth centuries it is spoken of by Hilken, Cooke, and other writers. These authors, however, like the late Dr. Mackintosh, of Edinburgh, employed the introduction of sounds or probes into the cervix of the uterus more as a means of treatment—and particularly for the mechanical dilatation of a contracted or strictured os—than as a means of physical diagnosis, such as is described in the preceding memoir.—(Ed.)

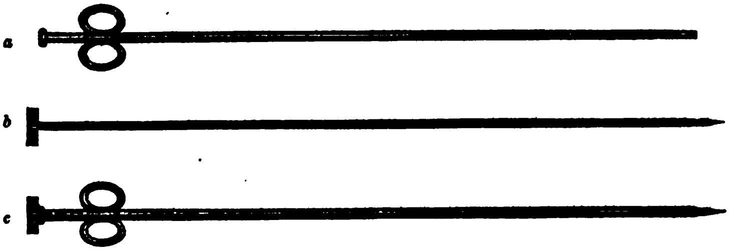
ON THE USE OF THE EXPLORING NEEDLE

IN THE DIAGNOSIS OF DOUBTFUL FORMS OF PELVIC AND OTHER TUMORS.[1]

(From Edinburgh Monthly Journal of Medical Science, February, 1850, p. 196.)

Those authors who, some years ago, wrote at great length upon acupuncture, as Beclard, Cloquet, Carraro, &c., all spoke of the impunity with which they found that acupuncture needles could be introduced into the muscles, vessels, and even the viscera of the living body. It was well known that small punctured wounds did not bleed, and the parts punctured generally closed immediately, and left little or no trace of the separation of their tissues by the puncturing instrument, provided it were small.

Fig. 2.



Taking advantage of the knowledge of these facts, it has been found that using a small grooved needle, or very slender trocar, we can introduce it into various morbid parts, so as to ascertain the nature of their contents. Surgeons had used such exploring needles for this purpose, in cases of doubtful tumors, in order to ascertain whether they were abscesses, or cysts, or aneurisms, &c. They have been used to explore even large aneurisms without any dangerous result. Dr. Simpson alluded to a case in which a celebrated surgeon was showing to his pupils the use of the exploring needle, in detecting matter in what was supposed to be an inflamed bubo, before laying it open. Air, however, rushed out, instead of pus, showing the swelling to be formed by the skin inflamed over a hernial sac.

Dr. S. mentioned that he had repeatedly used the exploring needle to detect the nature and contents of various kinds of pelvic tumor, when no other means of diagnosis were sufficient for that purpose. He especially adverted to its advantages as a means of diagnosis in some doubtful cases of pelvic abscess and ovarian tumors, and in cases in which tumors existed about the cervix uteri, the cystic or other nature of which it was otherwise impossible to determine. Would it serve to diagnosticate cases of extra-uterine pregnancy, either by the instrument striking against bone, or by any contents that might pass through the tube?

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[1] Read before Obstetric Society of Edinburgh, December, 1849.

The instrument which he employed was that figured in the woodcut on page 99. It was simply a very slender silver trocar and canula, the former tipped with a short steel point, of the form of that of a graving instrument. The tube of the trocar is open for nearly an inch along one side at its point, as is seen in the cut, so as to admit more easily of the escape through the canal of the tube, of any fluid in which its point may be placed. Sometimes he had applied an exhausting syringe to the outer end of the instrument, in order to procure the flow along its tube of any more viscid fluid. *a*, shows the canula, or tube of the instrument; *b*, the trocar; and *c*, the canula, with the trocar introduced as ready for use. Thin fluids like those of most ovarian cysts, flow readily along the tube, and can be recognized by their microscopic and other characters. When introduced into a sac containing pus, generally a few drops only of the fluid enter the tube of the instrument, from which, however, it can be readily forced, after withdrawing the instrument, by blowing through the tube. If our microscopic characters of specific tumors and morbid structures were more exact than they are at present, the pathologist might ascertain the nature of most morbid tumors that appear in the living body, by the use of such an instrument, for he could remove by it a sufficient amount of its structure or contents for histological purposes.

ON THE STATE OF ARTIFICIAL ANÆSTHESIA

AS A MEANS OF FACILITATING UTERINE DIAGNOSIS.

(From MS. Proceedings of Obstetric Society of Edinburgh, 14th March, 1855.)

SINCE the first introduction of ether and chloroform into obstetric practice in 1847, Dr. Simpson stated that he had annually endeavored to point out in his lectures the great advantages that were occasionally derivable from their employment, in the way of facilitating obstetric diagnosis. The production of a state of anæsthesia has been found specially useful as a means of physical diagnosis, under the following circumstances:

1. In cases of difficult parturition, the state of anæsthesia enables the accoucheur to ascertain more easily and exactly, by his tactile

examination, any peculiarities in the position or presentation of the child, or in the nature and amount of any impediments existing in the pelvic bones or maternal passages.

2. In instances of uterine or ovarian disease, connected with neuralgic tenderness of the abdominal walls and pelvis, it is often impossible to make a complete and useful tactile examination, unless the patient be previously anæsthetized.

3. In spurious pregnancy, with its usual characteristic, abdominal distension, the use of chloroform at once, as is now well known, enables the practitioner to decide the nature of the case; the abdominal enlargement disappearing as the state of deep anæsthesia supervenes.

4. In the two preceding morbid states, and in any other cases of uterine or ovarian disease, requiring a very accurate tactile examination, the previous production of anæsthesia not only allows the tactile examination to be gone through without suffering, but further it very greatly facilitates the examination by the state of local and general relaxation which it induces. Under such relaxation, the physical examination of the uterine organs by touch is rendered infinitely more perfect.

5. In instances where a required examination is objected to from motives of delicacy, the state of anæsthesia saves the feelings of the patient—a matter of no slight moment in the practice of the obstetric profession.

INFLAMMATORY ERUPTIONS

UPON THE MUCOUS MEMBRANE OF THE CERVIX UTERI.[1]

(From Edinburgh Monthly Journal of Medical Science, April, 1850, p. 386.)

The common forms and effects of inflammation of the cervix uteri, viz., ulceration, hypertrophy, and induration of the cervix, were now well known to the profession. But the surface of the cervix was liable to other types of inflammation of an eruptive character, which apparently had hitherto been little, or not at all, studied by obstetricians; and were not yet described in works upon the pathology of uterine diseases. Among these special inflammations of the cervix uteri and top of the vagina, Dr. Simpson had observed eruptions referable to the vesicular, pustular, tubercular, papular, and erythematic orders of the classification of Willan and Bateman. *Herpes* (*herpes uterinus*) he had seen following the usual course of

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[1] Read before Medico-Chirurgical Society of Edinburgh, March 6, 1850.

herpes labialis in two or three instances, in patients who had months previously been under treatment for common ulceration of the cervix; and Dr. S. suggested that perhaps this and other eruptions were occasionally the origin and basis of the common variety of granular cervical ulcer. *Acne*, in the form of chronic hard tubercles and pustules, was by no means uncommon, and often coexisted with common ulceration. A papular form of eruption sometimes supervened in chronic cases of uterine disease, and was usually diffused over both the cervix uteri and interior of the vagina; sometimes having the characters of *Lichen;* in other instances presenting the appearances and severe itching symptoms of *Prurigo*. *Eczema* and patches of *Aphthæ* also occurred. The treatment required to be varied according to the nature and character of the eruption, and consisted of the application of nitrate of silver, medicated washes, and medicated pessaries, &c. In severe and distressing cases of prurigo of the cervix, vagina, and vulva, brushing the affected surface often with hydrocyanic acid (the strength of that of the Edinburgh Pharmacopœia), was mentioned as frequently giving the greatest relief.

MEDICATED PESSARIES.[1]

(From Edinburgh Monthly Journal of Medical Science, June, 1848, p. 886.)

In diseased states of the cervix uteri and vagina, medicinal substances have been applied locally to those parts under various forms, but principally, either in a solid state, as nitrate of silver, potassa, &c., or in a liquid form, as in the great varieties of medicated injections in common use in leucorrhœa, &c. When thus used, the local application is temporary, and applied for a few minutes only. But in various forms of disease it seems an indication of no small importance to have the medicated substance applied continuously, and not temporarily. Medicated pessaries, which Dr. Simpson first introduced into practice several years ago, and which have since been extensively adopted by various practitioners in London and elsewhere (see descriptions of them published by Dr. Stafford Lee,[2] Dr. Oldham,[3] &c.), enable us to fulfil this indication. By their use, for instance, we can keep the cervix uteri, when ulcerated and indurated, constantly imbedded in mercurial or iodine ointment for weeks, and sometimes with the most marked benefit and success.

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[1] Read before Obstetric Society of Edinburgh, March 12, 1848.

[2] On Tumors of the Uterus, p. 25.

[3] Guy's Hospital Reports, vol. vi. pt. i. p. 193.

They fulfil another indication in cases of irritation and inflammation of the mucous membrane of the cervix uteri and vagina. They keep the opposed diseased surfaces from coming in contact; and it is well known how important a matter this is in the pathology of mucous and cutaneous surfaces.

Dr. Simpson has been in the habit of applying a variety of substances in the form of medicated pessaries, particularly zinc and lead ointment, &c., as simple emollients; mercury and iodine as discutients, and particularly the iodide of lead; tannin, alum, and catechu, as astringents; opium, belladonna, &c., as anodynes. The pessaries are made of the size of walnuts, and can be easily introduced by the patients themselves; one or two in the twenty-four hours. They are composed of the medicine used, mixed up in the form of an ointment, and brought to a requisite degree of consistence with one or two drachms of yellow wax to the ounce of ointment. Messrs. Duncan and Flockhart, druggists, have found the following proportions requisite in the subjoined forms, those in most frequent use in Edinburgh; and they might serve as models for the others. After being made up in the proper form, they are usually coated by the druggists with a firmer covering, by dipping them into an ointment made up with wax and resin, kept liquid by heat. About an ounce of the different ointments makes four balls.

1. *Zinc Pessaries.*—℞ Oxydi Zinci ʒj, Ceræ Albæ ʒj, Axungiæ ʒvj, Misce, et divide in pessos quatuor.

2. *Lead Pessaries.*—℞ Acet. Plumbi. ʒss, Ceræ Albæ ʒiss, Axungiæ ʒvj, Misce.

3. *Mercurial Pessaries.*—℞ Unguent. Hydrarg. Fort. ʒij, Ceræ Flavæ ʒij, Axungiæ ℥ss, Misce.

4. *Iodide of Lead Pessaries.*—℞ Iodidi Plumbi. ℈j, Ceræ Flavæ v, Axungiæ ʒvj, Misce.

5. *Tannin Pessaries.*—℞ Tanninæ ℈ij, Ceræ Albæ ℈v, Axungiæ, ʒvj, Misce.

6. *Alum and Catechu Pessaries.*—℞ Sulph. Aluminis ʒj, Pulv. Catechu ʒj, Ceræ Flavæ ʒj, Axungiæ ʒvss, Misce.

7. *Belladonna Pessaries.*—℞ Extr. Belladonnæ ℈ij, Ceræ Flavæ ʒiss, Axungiæ ʒvi, Misce.

FOR APPLICATION OF GASES TO VAGINA,

See Chapter on Local Anæsthesia, in Volume II.

EMPLOYMENT OF CHLORIDE OF ZINC

IN ULCERATION OF THE CERVIX UTERI.[1]

(From Edinburgh Monthly Journal of Medical Science, July, 1851, p. 82.)

Dr. Peddie having read a paper before the Society on the employment of chloride of zinc as an escharotic, Dr. Simpson stated that last year he had had chloride of zinc made up for applying to unhealthy ulcerations of the cervix uteri, by Mr. Hunter. But instead of using it as Dr. Peddie did in flat plates, he had then got Mr. Hunter to run it into moulds—so as to be formed into round sticks like those of nitrate of silver or potassa fusa, when it could be much easier used. When quite pure, it very rapidly deliquesced and was very apt to run. As, however, we can always readily restrain and arrest potassa fusa from spreading by partially filling the speculum with vinegar, and injecting freely acetic acid into it immediately after the potassa was applied, so he found that chloride of zinc could be similarly rapidly decomposed and restrained by using, instead of vinegar, a solution of carbonate of soda. Less pure preparations of the chloride, like the potassa cum calce, did not deliquesce so rapidly, and he believed Dr. Peddie had used such a preparation. He doubted if a slough separated by chloride of zinc left a more healing granulating surface than a slough separated by potassa fusa, or potassa cum calce. But he believed chloride of zinc would be found a manageable caustic, particularly if the neighboring parts were protected with an alkaline solution—or if the preparations were made impure by being mixed with other material so as to prevent it deliquescing very rapidly: it was usually sold in this impure form.

TREATMENT OF INFLAMMATORY INDURATION OF THE CERVIX UTERI

BY DEEP CAUTERIZATION WITH POTASSA FUSA.[2]

(From Edinburgh Monthly Journal of Medical Science, July, 1847, p. 71.)

Dr. Simpson stated that his own observations fully confirmed the recorded opinions of Dr. Bennett and others regarding the general dependence of leucorrhœa upon inflammatory ulceration and induration of the cervix uteri. He had found inflammatory enlargement and induration of the tissues of the cervix very frequent in practice, and existing, in fact, in most cases of very chronic and aggravated leucorrhœa. In practice he had seen it mistaken for the induration and ulceration of carcinoma, &c. Formerly, in the treatment of these common cases, he had employed the frequent local application of leeches, and counter-irritation to the sacrum, &c., with the use of pessaries of mercurial and iodine ointment, keeping the indurated tissues imbedded in these applications, &c. The cure in this way is tedious, and months are often required before the indurated parts become reduced. Various local escharotics, partly to destroy the indurated tissues by direct decomposition, and partly to soften down the remainder by new inflammatory action, had been in modern times employed for the same purpose, and with much more certain and expeditious effect. He had in this way employed in a number of cases nitrate of silver often applied, Vienna paste (*potassa cum calce*), and nitric acid. He stated the particulars of a case which he had treated successfully four years ago with nitric acid, and he had lately seen the patient in perfect health; it was at the time supposed that she had cancer uteri. Latterly, he had abandoned these and other escharotics, and now always used the common *potassa fusa.* He had found it far more manageable, speedy, and certain than any other method. He used it of course through the speculum, applying a stick of it freely with a proper caustic-holder to the ulcerated and indurated tissues. It required to be rubbed or held *strongly* for a time against the part which was to be destroyed. In general a piece three-quarters of an inch, or an inch long, was melted down. The decomposition produced by it often caused a hissing sound. If the induration is extensive, and the whole cannot be removed at once, increased action and absorption are set up in what remains, and the parts adjacent become softened and diminished in size. Absorption in this way is truly one of the results or consequences of inflammation, though still an undescribed termination. In some aggravated cases two or more applications of the caustic are required, at intervals of eight or ten days. He had never seen pelvic cellulitis, or any other bad result follow. The appearance after the operation is as if a portion had been clean cut out with the knife. A large quantity of vinegar and water is immediately thrown up through the speculum to neutralize the potassa, and prevent it from injuring the sound parts. A copious purulent discharge usually follows for several days, requiring the use of astringent washes, or zinc ointment pessaries. When the whole of the induration is once removed, the remaining ulcer heals rapidly and permanently. An ulcer over an indurated part may be cicatrized, but it is almost certain to break out again and again till the induration itself is reduced.

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[1] Extracted from Proceedings of Obstetric Society of Edinburgh, March 26, 1851.

[2] Ibid. April 13, 1847.

MORBID DEFICIENCY AND MORBID EXCESS IN THE INVOLUTION OF THE UTERUS AFTER DELIVERY.

(From Edinburgh Monthly Journal of Medical Science, August, 1852.)

The enormous increase which occurs in the parietes of the uterus during the nine short months of pregnancy has long attracted the attention of professional observers. It is a kind of physiological hypertrophy unequalled, either in regard to its magnitude or its rapidity, in any other organ in the adult human body. For, during the forty weeks of utero-gestation, the uterus enlarges from nearly 3 inches in length and 1¾ of an inch in breadth, to 12 or 15 inches in length and nine or 10 inches in breadth. It increases from about 2 ounces in weight to 25 or 30 ounces. The cavity of the uterus before impregnation is less than one cubic inch, while at the full term of pregnancy it is extended to above 400 cubic inches; and the surface of the organ increases from about 5 or 6 square inches to nearly 350 square inches. Before impregnation, the uterine cavity would not hold above a drachm or two of fluid: at the ninth month of utero-gestation, its contents usually weigh from 120 to 150 ounces.

The rapidity, however, with which the uterus diminishes in size after delivery, is perhaps still more marvellous than the rapidity with which it increases in size after impregnation. The celerity of its involution in the puerperal state is in fact more striking and remarkable than the celerity of its evolution during the pregnant state. If the process of absorption of organs in the adult is ever studied successfully anywhere, it will probably be by making observations on the reduction or involution of the uterus in women or in the lower animals subsequent to parturition.[2] Whilst the human uterus takes forty weeks to attain the dimensions pertaining

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[1] Read before Edinburgh Obstetric Society, February 11, 1852.

We have preferred placing this Essay among those on uterine diseases, rather than including it with puerperal derangements; inasmuch as instances of it will not be likely to come under the notice of the obstetrician during the first few weeks after parturition, but at a later period when such local or constitutional disburbance has arisen as to lead the patient again to seek the advice of her medical attendant.—(Ed.)

[2] In the Swedish Hygieia of last year, my friend, Professor M. Retzius, of Stockholm, published some interesting observations on the process by which nature effects the reduction of the puerperal uterus. He found in a series of anatomical and histological observations on the subject, that the process of absorption of the walls of the puerperal uterus was preceded, as absorption of other deposits is, by fatty transformation of its component fibres; and that the blood during puerperal convalescence shows under the microscope a corresponding superabundance of globules or granules of fat.

to the fully developed state of pregnancy, it requires only, on the contrary, from four to eight weeks to decrease from the extreme size of the organ peculiar to pregnancy, down to the small size peculiar to the same organ in its unimpregnated condition.

But in the vital mechanism of the involution or reduction of the uterus after delivery, various pathological derangements are liable from time to time to occur. This, like every other process in the animal economy, is apt, for example, to fail, either in the way of defect or of excess. Some years ago, I endeavored to point out to my professional brethren, that occasionally, as one of the derangements in this mechanism of involution, the uterus is morbidly slow in regaining its original dimensions—its involution becomes impeded or arrested—and the organ is in consequence liable to be found, weeks or even months after parturition, still so large and unreduced, as at first to be readily mistaken for a tumor of the uterus or ovary. I described this peculiar condition of the puerperal uterus, under the name of "Morbid Permanence of the State of Puerperal Hypertrophy,"[1] and illustrated it with the following example:

CASE I.—During the summer of 1842, I attended, along with Dr. Abercrombie, a lady, who, after a premature confinement in the country, had suffered from a smart attack of puerperal fever. After so far recovering for a few weeks, she was sent from a considerable distance into town, to be treated for what appeared to be a large tumor, stretching upwards from the pelvis into the right iliac region. The tumor had not been observed before delivery, and was somewhat painful to the touch. It seemed at first sight extremely doubtful whether the mass consisted of an inflamed uterine fibrous tumor, or enlarged ovary, or of one of those chronic purulent collections which are apt to form towards one or other iliac region in connection with puerperal fever or inflammation. The uterine sound, when introduced into the os uteri, passed easily and directly upwards for several inches to the superior end of the tumor, and its apex could be felt there by the hand placed externally. This at once showed the supposed diseased mass to consist of the enlarged uterus. Further examination proved that there was nothing strictly abnormal about the uterus, except its great size. In fact, it was a case where the organ had apparently remained nearly undiminished after delivery, probably from the puerperal attack arresting the usual progress of its absorption and diminution. It decreased rapidly and fully under leeches and other local antiphlogistic treatment.

In alluding to the pathological state of the uterus, of which the preceding case is a well-marked example, Dr. Lever has adopted the same designation which I originally applied to the affection.[1] More lately (1851), Dr. Snow Beck described to the London Medical Society a case of this malady, under the simple term of "A New Disease of the Uterus."[2] On examining microscopically the structure of the hypertrophied uterus in this instance, no inflammatory or heterologous deposits could be detected; but the tissue of the organ was, it is stated, similar in its histological characters to the tissue of the uterus at the ninth month of pregnancy, except only that its component muscular fibres were smaller in size, or like those of a uterus at the middle period of utero-gestation.

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[1] See Memoir on the Uterine Sound, ante, p. 72.

Retarded involution or reduction of the uterus after delivery is not unfrequent in its less marked degrees; especially when inflammatory or febrile action supervenes and interferes with the phenomena of the puerperal state. It is often, for example, observable both during life and after death, in women who are the subjects of puerperal fever, pelvic cellulitis, and phlegmasia dolens. Chronic hypertrophy of the uterus in any excessive degree, from morbidly retarded or arrested involution, is more rarely met with. I have sometimes, however, seen it ten or twelve weeks subsequent to delivery, in the form of an apparent tumor, twice or more the bulk of the normal uterus. In lesser, though still sufficiently remarkable degrees, it often persists for many long months, or even years, after parturition; particularly when combined, as I have repeatedly found it, with anteflexion or retroflexion of the fundus uteri, or with the state of prolapsus.

The day after (July 18) the preceding remarks were written, I saw with Dr. Retzius the following instance of this combination of retarded involution with retroversion of the uterus, in a lady, who came for advice from the north of England.

CASE II.—The patient, aged 28, and married for three years, was delivered of her first child two years ago. She was so well as to be allowed to leave her bed at the end of a fortnight. The lochia, however, were very abundant, and continued for eight weeks. She nursed her child for seven months, and during this period of lactation the menses recurred, as they have done since, regularly, profusely, and each month somewhat prematurely. She complains of pain in the back, weakness, etc. In making a vaginal examination, the body of the uterus, which is retroverted, feels large and heavy, like a uterus at the third month of pregnancy. Its cavity is, as appears by examination with the sound, nearly an inch greater than the natural length. The cervix is large, and fills entirely the extremity of the largest sized speculum. Its surface is red and congested, but presents no appearance whatever of abrasion or ulceration. The os is unusually patent, and admits the tip of the finger for about half an inch. The lining membrane of the cervical cavity feels, with the os, hypertrophied, and thrown into prominent folds, and some of the Nabothian glands are much enlarged. The hypertrophied body and fundus of the uterus seem quite free from fibrous tumor, or any other heterologous disease.

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[1] Guy's Hospital Reports, vol. ii. 1844, p. 17.

[2] Lancet for 19th April, 1851, p. 447.

Sometimes hypertrophy of the uterus, from arrested reduction or sub-involution of the uterus, follows upon abortion or premature labor. I have at present the following instance under my care:

Case III. The patient, æt. 35, and the mother of six living children, had a premature labor on the 11th December, 1851, under the charge of my friend, Dr. Dickson of Bathgate. The labor came on about the fifth or sixth month of pregnancy, and was in itself simple and easy. The convalescence, however, proved slow and imperfect; and she was indeed in a great measure confined to bed for three or four months afterwards. The lochial discharge stopped on the second or third day. Menstruation has recurred regularly, but with some degree of menorrhagia. There is slight leucorrhœa. The uterus feels heavy and hypertrophied when examined per vaginam, but without any organic disease in its walls. The cervix is also much enlarged, particularly the anterior lip, which is considerably thicker than the posterior. Immediately around the os there is a line or two of granular ulceration. The uterine cavity measures between three inches and three inches and a half.

The preceding instances and remarks refer to deficient, impeded, or arrested involution. But a morbid *excess* of involution or reduction in the uterus after delivery (*super-involution*) is still more rare than a morbid *defect* in it (*sub-involution*); and I am not aware that hitherto any obstetric pathologist has described the former as a diseased state of the uterus. The following remarkable instance of such super-involution, as ascertained both during life and by dissection after death, has lately fallen under my notice:

Case IV. The subject of this rare pathological affection began to menstruate at the age of thirteen; and the catamenia recurred regularly every four weeks till she became pregnant when eighteen years old. Utero-gestation went on without any unusual phenomena to the full term; and her parturition was natural but tedious, a male child being born after a labor of seventeen hours. Nothing unusual occurred during her puerperal convalescence and lactation. But subsequently to delivery she never menstruated. She was, however, subject to frequent attacks of diarrhœa, which she herself believed to be generally most severe at recurring monthly intervals; and the dejections were then sometimes tinged with blood.

Two years after her accouchement, she became a patient in the female ward of the Royal Infirmary, complaining of the state of amenorrhœa, with attendant broken health. She suffered from pain in the back and hypogastrium, with a sensation of weight and pressure in the pelvic region; dysuria; a furred tongue; and a weak compressible pulse, generally beating from 80 to 90 in the minute. She was thin, feeble, and anæmic in appearance. The mammæ were shrunk and flat. For some time before admission she had suffered much from occasional headaches and giddiness; frequent nausea and vomiting; palpitation and occasional rigors.

On making a vaginal examination, I found the uterus small and mobile. The cervix uteri was much atrophied, and the vaginal portion of it scarcely made any projection into the canal of the vagina. The os uteri was so much contracted as to admit a surgeon's probe with difficulty. It was dilated by a slender Bougie being left in it for two or three days; and, when the uterine sound was subsequently used, the uterine cavity was found to be only one and a half inches in length, or about an inch less than normal.

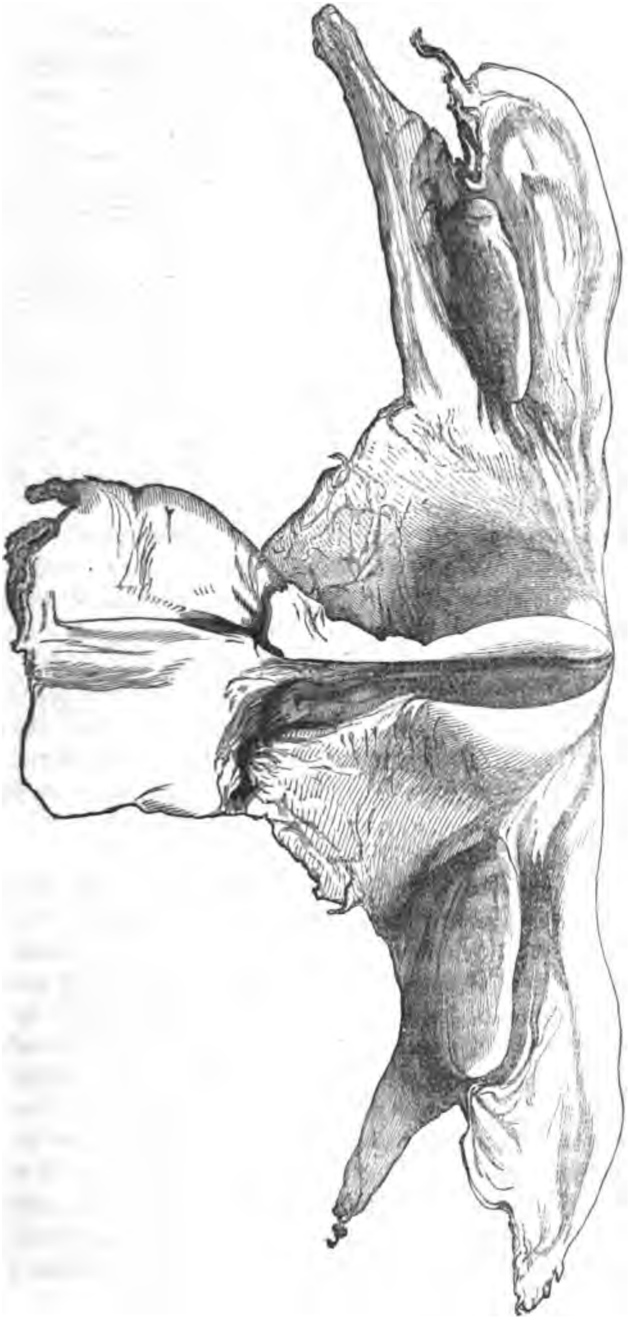
A variety of means were employed with the view of benefiting the general health of the patient, and of exciting action in the uterine system, but with little or no effect.

Diarrhœa repeatedly occurred during the three or four weeks she remained under my care, requiring the free use of opiates for its restraint; and as the uterine symptoms did not at the time seem to admit of special attention and treatment, the patient was transferred to one of the general wards of the hospital, where she was placed under the care of my colleague, Dr. Bennett.

During the following month, the diarrhœa recurred from time to time very severely. At last, anasarca in the lower extremities and albuminuria supervened; ascites followed; and shortly afterwards her face and arms became œdematous. About a month after these symptoms appeared, delirium at last came on, the fæces passed involuntarily, and ultimately she died in a state of prolonged coma.

On post-mortem inspection, some crude tubercles were found in both lungs—especially in the left. The liver was enlarged, and showed some fatty transformation. The kidneys presented also some stearoid degeneration; and in the right there was, in addition, a small tubercular abscess. The large intestines were very much thickened in their parietes, and contracted in their calibre; while their mucous membrane was ulcerated in various parts. Along the

Fig. 3.



lower end of the ileum several large ulcerations were seen running circumferentially around the interior of the bowel. One or two ulcerations were also found in the stomach. The uterus was very small, and atrophied in its length and breadth; its size being diminished about a third below the natural standard in all its measurements; and its parietes were correspondingly thin and reduced. The whole length of the uterine cavity from the os to the fundus was not more than one inch and a half, while the normal uterus usually measures in this direction two inches and a half. When a section was made of the posterior wall of the organ, the thickness of its parietes at their deepest or most developed point was not above three lines, instead of the normal measurement of five or six lines. The tissue of the uterus appeared dense and fibrous, and the section of it presented the orifices of numerous small vessels. The ovaries seemed also much atrophied, and smaller than natural. Their tissue was dense and fibrous, and presented no appearance of Graafian vesicles. There was no inflammatory deposit on the peritoneal surface of the uterus or its appendages; but some thick pus, or tubercular matter, existed in the distended cavity of the right Fallopian tube.

The wood-cut on the preceding page presents the uterus and upper part of the vagina, the broad ligaments, and ovaries, of the exact size which they presented, and their degree of atrophy may be easily judged of by comparing the sketch with the same parts when of the normal size. The sketch represents the posterior surface of the uterus and broad ligaments, with the uterine cavity exposed, in order to show the diminished thickness of the parietes of the viscus. The whole parts represented in the wood-cut weighed only one ounce, four drachms, twenty-five grains, in apothecaries' weight.

In females, subsequently to the cessation of the menses, the uterus, along with the ovaries, undergoes a slow but marked reduction in size. The uterus in this way, in some extreme instances, recurs subsequently to menstruation nearly to the small dimensions appertaining to it previously to puberty; it becomes atrophied and shrunk in all its measurements and dimensions; the vaginal portion of the cervix can be specially felt flattened and reduced in size; and occasionally the canal of the cervix, especially at its upper part, is found so contracted as not to admit of the passage of a probe. This form of senile atrophy of the uterus is doubtlessly connected with the natural suspension of the functions of the reproductive organs; and in this respect is so far similar to the results which we sometimes see in other viscera, as the testes, when the functions of these viscera are arrested in the course of nature, or when they happen to be prematurely suspended by chronic inflammation, or other forms of disorganizing disease.

The principal peculiarities in the instance of the marasmus, or atrophy of the uterus, which I have detailed, are two-fold:—1. The occurrence of the affection in a very young female, as a consequence of pregnancy and parturition; and 2. The excessive degree of that atrophy or super-involution—the uterus being, as we have seen, reduced fully a third in size below its natural dimensions.

During the last few years I have seen a number of cases of permanent amenorrhœa connected with an atrophied, or rather undeveloped condition of the uterus. In these cases the cavity of the uterus generally measures from one and a half to two inches in length, as ascertained by the use of the uterine sound; the shrunk cervix usually projects but slightly into the cavity of the vagina; and the opening of the os is small and contracted. In these cases the uterus has apparently not taken on its usual degree of evolution and growth at the period of puberty. The organ, by a kind of malformation from defective development, as teratologists would describe it, retains after the date of puberty the type and size which normally pertain to it in the state of girlhood. But the particular case of undersized uterus which I have described in the preceding paragraphs is quite different from these; for in it the uterus, after being fully developed at puberty, and after performing normally the several functions of menstruation, pregnancy, and parturition, returned, as it were, suddenly to the type peculiar to the organ antecedently to the commencement of menstrual life; or, perhaps, we may more correctly say, it assumed, at the early age of nineteen, by an excess of the natural involution or absorption pertaining to the puerperal state, a degree of anatomical atrophy of its structures, and physiological arrestment of its functions, such as does not occur normally till the age of forty-five or fifty; and even then, at that advanced period of life, the degree of physical reduction in the size of the organ only rarely becomes so very great as was observed in the case which I have detailed above.

The case itself affords no precise data for determining whether the atrophy of the ovaries and uterus stood to each other in any respect in the relation of cause and effect; or whether they were both simultaneous results of one common agency.

Instances from time to time occur, in which, as in the preceding case, permanent amenorrhœa follows parturition. It will not, of course, be found that all such instances depend upon excess in that process of natural absorption or involution which follows upon delivery. But the case in question shows that this super-involution of the uterus may be expected to be met with, in some instances, in connection with this type of amenorrhœa.

At present I see professionally, from time to time, a case in which amenorrhœa has followed parturition, and in which the uterus is also reduced below its natural size. The following are the principal points in its history:

CASE V.—The patient, now aged 30, has been married ten years. She has borne three children. From the time of her third labor, which occurred four years ago, no menstruation has recurred. The catamenia thus ceased at the age of twenty-six. After this third labor she made a good recovery, and left her bed on the eighth day. She nursed the child for fourteen months. I first saw her about two years subsequently to the birth of her last child. She then supposed herself to be again near her confinement; but the case was only a marked example of spurious pregnancy. The uterus, instead of being enlarged, felt looser and smaller than natural; and the vaginal portion of the cervix was specially reduced below the natural standard in length and breadth. The cavity of the organ was somewhat diminished below the usual length, and did not allow the stem of the sound to pass up to quite two inches and a half. In this patient the mammæ are flat and atrophied; and she is thin, weak, pale, and impaired in health and strength.

Sometimes super-involution of the uterus follows abortion, or premature labor. During the present month, I have had placed under my care an instance of this complication, in a patient from Canada.

CASE VI.—A mother of eight living children, now aged 35, had, on the 29th July, 1851, a dead premature child, about the sixth or seventh month, under the charge of Dr. Campbell. Nothing occurred to impede her convalescence. Before, however, her confinement, her health was not so good as usual, and she was disappointed to find it remained so after delivery. She has only once seen the catamenia during the past twelve months—viz., in September. There is slight leucorrhœa. She is anæmic and chlorotic, with palpitation, etc. The mammæ, which she states were previously very full and large, are now shrunk and flaccid. The uterus is of nearly its natural length, but the vaginal portion of the cervix is very short and atrophied, with the lips somewhat everted. The superior portion of the cervix, above the reflection of the vagina, can be felt small, firm, and cylindrical; and the body and fundus of the uterus when grasped between the left hand placed above the pubes, and the two first fingers of the right hand introduced per vaginam, appears under examination unusually mobile and slender, and altogether reduced below the usual standard of size.

Since the publication of this essay we have had ample opportunities of verifying the correctness of Dr. Simpson's views with regard to these morbid conditions of involution of the uterus. As observed by him, we have found the condition of super-involution by far the rarer of the two. Cases of sub-involution, however, we have frequently seen in Dr. Simpson's practice; within the last few days we have met with an unmistakable instance. The patient, aged twenty-one, had suffered from amenorrhœa since her first confinement, two years ago, and presented on examination a distinct hypogastric tumor. The cavity of the uterus, in this instance, measured fully six inches, while there was no trace of tumor in its walls.—(ED.)

ON THE MORE COMMON TERMINATIONS AND ON THE TREATMENT OF FIBROID TUMORS OF THE UTERUS.

BY far the most frequent form of organic disease met with in the body and fundus of the uterus, is that species of growth which usually passes under the name of the "fleshy," "fibrous," or "fibroid" uterine tumor. These tumors, which generally affect a roundish form, and are almost always gregarious, vary greatly in size. They are sometimes not larger than peas or hazelnuts, and occasionally they acquire, singly or gregariously, the size of a uterus at the sixth month, or even at the full term of pregnancy. Their anatomical relations and characters, their symptoms and general treatment, I have dwelt upon at some length in another work.

In former times, the common fibrous or fibroid tumors of the uterus were generally considered as malignant in their type, and too often confounded with cancer. Some obstetric writers of the present day even look upon these fibrous uterine growths as having a kind of scirrhous or carcinomatous nature. And no doubt the texture of a chronic fibroid tumor may, like any other tissue, natural or morbid, become occasionally, though rarely, the seat of carcinomatous deposit. But primarily and essentially, they have nothing carcinomatous in their character, nor any tendency to undergo the changes to which carcinomatous structures are liable.[2] On the contrary, the changes to which they are subject are pathologically very different, and vary in different instances.

[1] See Essays on the Diseases of the Uterus, in the Library of Medicine, vol. iv. p. 332.

[2] I have now several times seen the uterine tissue with which a pediculated fibrous tumor or polypus was long in contact, become in those predisposed to cancer, the seat of carcinomatous degeneration and ulceration, apparently from the constant irritation of the tumor as a foreign body upon the contiguous uterine tissue. Carcinoma of the cervix as well as of the cavity of the uterus is sometimes induced indirectly in this way, without the fibroid tumor or polypus itself degenerating into cancer.

1. Fibroid uterine tumors sometimes go on growing—usually very slowly—sometimes intermittingly—and retain throughout life their original fibrous character, increasing merely in volume, without any specific change in their structure.

2. Often, after increasing for years, they entirely cease from further growth, without any further marked change in their size or structure. This termination appears to occur principally when the tumors are imbedded in the uterine walls, or are intramural in their seat, and when they are restrained in their development by the strength and denseness of the tissues surrounding them.

3. When they cease growing, however, the fibroid structure of the tumor is frequently found to become changed into cartilaginous tissue, and subsequently to become more or less the seat of osseous or calcareous degeneration; a transformation observed equally in the smallest fibrous tumors as in those of a larger volume. This calcareous degeneration in uterine fibroids seems to indicate the death, and consequently the cessation of the reproductive action, of those cells which form the essential growing constituent in these tumors.[1]

4. In a few cases observed by Clarke and other pathologists, fibroid tumors of the uterus have appeared to undergo another and most important change, viz., they have gradually atrophied and become diminished both in volume and density.

If we looked upon fibrous uterine tumors as purely heterologous structures, it would be difficult indeed to believe in the possibility of their atrophy and involution—either spontaneously or under any form of treatment. But the more I have examined these structures of late years, the more have I become convinced that they are essentially and primarily homologous structures only, and that they essentially consist, in fact, of local masses, *nodose* collections, or hypertrophies, if I may so speak, of the normal fleshy or fibrous tissue of the uterus. Most physicians further are, we believe, ready to acknowledge that mere hypertrophies of the natural structures of organs sometimes diminish and are amenable to treatment; and we all know that the uterus, immediately after delivery, is in a state of great physiological hypertrophy, from which it is rapidly reduced by a normal process of involution or atrophy. During the process of puerperal involution the uterine structures undergo a fatty form of degeneration. Occasionally on dissection we have observed uterine fibroid tumors presenting what we conceived to be an analogous white fatty-like transformation. And during the last few years I have watched, upon the living subject, the progress of a number of cases of uterine fibroids, that have appeared to me gradually to involve and atrophy to a greater or less extent under treatment.

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[1] I have elsewhere tried to show (see Proceedings of Obstetric Society, in Edinburgh Monthly Journal for August, 1847, p. 137), that we have a similar change and deposit in the case of some human entozoa—for example, the *trichina* and *cysticercus*—the cyst belonging to and enclosing these entozoa ossifying or becoming calcareous after their death. At the same time I mentioned the interesting experiments of Rayer, in which that pathologist induced the artificial transformation of normal fibrous tissue, such as the ear of the rabbit, into cartilaginous and osseous substance, by the repeated or continued irritation of it. And as a result of these remarks, the possible artificial induction of osseous transformation as an indication of treatment in fibrous uterine tumors, was suggested.

In Germany, the waters of Kreuznach have obtained some repute for their supposed efficiency in the reduction of tumors, and especially of fibroid tumors, in the walls of the uterus. I have seen a considerable number of such cases after they had been subjected to full courses of this water drank internally, and applied locally. In most of them, this treatment had most completely failed; but one or two have appeared to benefit by it. I have seen a far larger proportion of cases do well under a long-continued course of bromide of potassium, given in doses of from three to five grains taken thrice a day; and sometimes combined with its local application, leeching, &c. The Kreuznach waters contain bromine; but only in very small quantity. If the bromine is, as it is supposed to be, the chief deobstruent in these waters, the methodic exhibition of this ingredient artificially in more powerful and continuous doses ought to be followed, as I believe it is, by more beneficial and decisive effects than the waters themselves. It stands thus in the same relation to the Kreuznach waters, as quinine does to cinchona.[1] One of the first patients who used the bromine alone, with this view, I saw along with my friend Dr. Wyse. The lady had been at Kreuznach, and taken full courses of the waters there without any benefit whatever. The uterine fibrous tumor was of large size, and incommoded her much from its weight and pressure. Under a course of bromide of potassium and local leeching, continued in perseveringly for many long months, the tumor involved and decreased in a most marked manner; and her health and power of walking and exertion became quite restored. I have found the same remedy succeed in several similar cases, in arresting and more or less reducing uterine fibroids, and again in others I have seen it fail. But to most persons the bromide of potassium can be given for a great length of time without any interference with the general health. In fact, it usually serves apparently as a tonic as well as a deobstruent, in this respect having a marked advantage over iodide of potassium.

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[1] On the efficacy of bromine in fibroid tumors, and on their nature, see a few remarks by Dr. Simpson at a meeting of the Medico-Chirurgical Society, December 7, 1853, in Association Medical Journal for that year, p. 1116.—(Ed.)

5. When fibroid tumors are situated more immediately under the serous or mucous coats of the uterus, they usually become gradually more or less pediculated, forming fibroid polypi. Sometimes the pedicle becomes more and more attenuated and at last gives way, allowing the tumor when subperitoneal to fall into the cavity of the abdomen (of which I have seen several instances); or, when it is submucous, the tumor or polypus drops into the cavity of the uterus and vagina, and thence escapes. The accoucheur imitates this latter process in the surgical removal of uterine polypi.

6. When the tumor is non-pediculated and intramural, another termination is occasionally observed. The tumor sometimes inflames, and sloughs, and the intervening uterine tissues ulcerating, it is ultimately eliminated from the genital canals in smaller or larger masses. I have seen several instances in which nature thus reduced and threw off successfully by ulceration and gangrene, large portions and masses of intra-mural fibroid tumors. Attempts have been made by Amussat, Maisseneuve, Atlee, and others, to imitate by the knife this process of spontaneous or natural elimination; but we believe it to be, from the cases which we have seen, an operation that, as at present practised, will very rarely indeed succeed, and which ought to be very rarely, if ever, undertaken.

ARTIFICIAL REMOVAL OF A LARGE FIBROUS TUMOR,

IMBEDDED IN THE POSTERIOR WALL OF THE UTERUS.[1]

(From Edinburgh Monthly Journal of Medical Science, March, 1848, p. 695.)

Dr. Simpson explained to the Society the mode in which he had lately removed a large fibrous tumor from the posterior wall of the uterus. The patient was forty-six years of age; she had borne ten children, the youngest being six years old, naturally strong and healthy. During the last two years she had been subject to severe floodings, which recurred every fortnight, and had reduced her excessively. Her general health had become quite broken up, and for several months she had suffered much from sore throat, and general irritation of the mucous membranes. He found, on exa-

[1] Extracted from Proceedings of Edinburgh Obstetric Society, Dec. 22, 1847.

mination, a large fibrous tumor imbedded in the back wall of the uterus, and protruding downwards upon the top of the vagina in a rounded form, the os uteri and uterine cavity lying in front of it—as ascertained by the uterine sound.

Accompanied by Drs. Malcolm, Ziegler, Weir, and Keith, he made an opening, by means of caustic potass, into the most prominent part of the tumor, about one inch behind the os uteri. It extended through the layer of uterine substance, and into the mass of the tumor itself. On inserting the finger into the hole thus made, it was found that it could be passed easily between the substance of the tumor and the uterine wall. The tumor, in fact, seemed to be very loosely connected with its uterine envelope, and could be separated from it with great ease as far as the finger could pass between them. Two days after the caustic was used, he found the artificial opening enlarging like the os uteri in labor, and the tumor beginning to protrude through it. It opened up gradually, the patient taking some ergot, and on the fifth day a large piece of the tumor was found pushed low down into the vagina, while the edge of the uterine wall could barely be felt, encircling it like the rim of the os uteri when fully dilated. The abdominal tumor had subsided considerably. On the eighth day he attempted to pass a ligature round the mass, but found it could not possibly be made to include more than a very small portion. He separated, however, and brought away a small fragment, not without giving a good deal of pain. The tumor now gradually and more completely filled the vagina. The uterus, however, seemed unable to throw it off entirely, and the patient was getting exhausted from the quantity of the discharge, which was very fetid and offensive. On the twelfth day, Dr. Simpson, while she was completely under the influence of chloroform, passed up his hand by the side of the tumor, completed the separation of the remaining adhesion, like an adherent placenta, and brought away the tumor in one mass. This was done in a very few minutes. The patient awoke quietly, and said she had felt no pain whatever; nor did she complain at all of pain in the region of the uterus subsequently. She proceeded very well for three days, her pulse not above eighty; when, in consequence of her nurse having taken unwarrantable liberties with her in making her get out of bed, washing, &c., she was seized with rigors, followed by severe sore throat and irritative fever. This completely exhausted her remaining strength, and she died six days after the tumor was removed. The body was examined by Drs. Bennett, Scott, Simpson, and Keith. The peritoneal surface of the uterus was perfectly healthy, and showed no trace whatever of inflammatory action. The cavity in the back wall, in which the tumor had been imbedded, was much reduced in size, and appeared to be tending to a healthy cicatrization.

The tumor weighed three pounds eight ounces when removed. It must have previously been somewhat heavier, as the great discharge for several days was no doubt from partial softening and decomposition of the tumor. Various French surgeons had cut down upon such tumors, and enucleated them by instruments, or by the hand. In the above case Dr. S. had merely formed an artificial opening into the uterine cyst, as it were, containing the tumor, and then allowed the uterine walls, which are hypertrophied around such masses, exactly like the uterus in pregnancy, to push down and expel the foreign body. It was, he believed, the first time this new operation had been performed, and the largest fibrous tumor ever yet enucleated. The tumor, on division, was shown to be the common fibrous tumor of the uterus.

ON THE DIAGNOSIS OF POLYPI GROWING FROM THE LIPS OF THE CERVIX UTERI.[1]

(From Lond. and Edin. Monthly Journal of Med. Science, April, 1845, p. 319.)

In polypi arising from any part of the interior of the uterus, and projecting into the vaginal cavity, the stalk of the tumor is always found more or less encircled by the lips of the dilated os and cervix of the organ. The tracing, with the finger, of this circle of the cervix, round the pedicle of the polypus, forms always an important diagnostic mark in such forms of the disease. Thus Dupuytren remarks, "The essence of diagnosis is always the exploration of the neck—of its central orifice, and of the circular cul-de-sac, which separates it from the vagina."

When, however, a polypus arises from the edge of the os uteri or vaginal surface of the cervix, the above important diagnostic mark is wanting; and the case, in consequence, becomes one, the nature of which is often much more difficult to determine. Speaking of this form of the disease, Dr. Gooch states strongly the difficulty of diagnosis in such cases. "I have seen," he observes, in regard to them, "the most experienced practitioners in London puzzled to tell what was the nature of the tumor in the vagina, and what ought to be done for it;" and Lefaucheux describes the appearances on dissection of a case of this form of polypus, which during life could not be discriminated from complete inversion of the uterus.

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[1] Previous to the appearance of the paper on the Detection and Treatment of Intra-uterine Polypi, we find published the accompanying extract from a MS. clinical lecture, which, as it illustrates some of the difficulties of diagnosis of polypi, and the aid to be derived from the Uterine Sound, we have thought it well to reprint. It also discusses the differential diagnosis of Uterine Polypi and Inversion of the organ, hitherto only alluded to. See ante, p. 43.—(Ed.)

The difficulty of diagnosis of polypi of this kind, arising from the edge of the os uteri, or from the vaginal surface of the cervix, does not depend merely upon our not finding the pedicle of the tumor encircled, as usual in other forms of uterine polypi, by the dilated orifice of the uterus, but also from this still more fallacious circumstance, that the os uteri, though traceable in the stalk of the tumor, is generally so displaced in situation, and altered in form, as to render its identity doubtful. "In these cases," remarks Lefaucheux, "the orifice of the uterus has always lost its natural form. It is found situated obliquely, and the lip of the cervix which is not the seat of the pedicle is the most elevated, the pedicle elongating, on the other hand, that lip to which it is attached. When the pedicle," he adds, "has taken its rise in the cellular tissue of the greater part of the circumference of the orifice of the uterus, it is sometimes very difficult to distinguish between such a polypus and inversion of the organ, for the remainder of the orifice forms only a kind of fissure, which is detected with difficulty, and through which it is impossible to introduce the finger." The difficulties attending the diagnosis of the form of polypus to which these remarks refer, would in most, if not in all cases, be perfectly removed, if we could assure ourselves that the body of the uterus itself was of the natural size, and in its natural position, and that the opening or imperfect cleft that may be traceable on the inside of the tumor was in reality the os uteri. If these points could be fixed with certainty, the attachment and nature of the tumor would become at once evident—the question of the propriety of its removal would be resolved—and the exact point of its removal more safely and certainly determined, than it otherwise could be; in those cases where the stalk grows from the cervix, the rule being, as laid down by Gooch, that the knife or ligature ought to be applied a little below the orifice, if it can be distinguished. These important points in diagnosis and practice we would, in future cases, propose to fix by introducing the sound into the uterine cavity, so as to determine the real situation of the os, and the position and state of the uterus itself, as ascertained by the direction and length of its cavity. The introduction of the instrument in particular cases of this complication will require unusual care and patience, in order to pass it through the displaced and altered uterine orifice; and a sound of a smaller size than usual may in some instances be necessary. But the clear information afforded by the examination in a set of cases that are so often doubtful and perplexing in their character, will amply repay the mastering of any such difficulties as I have presupposed in the employment of the means.

ON THE DETECTION AND TREATMENT OF INTRA-UTERINE POLYPI.[1]

(From Edinburgh Monthly Journal of Medical Science, January 1850, p. 3.)

After a polypus, or pediculated tumor, arising from any part of the interior of the uterus, has once passed downwards into the vagina, the diagnosis of the disease is, generally speaking, very easy, the operation for its removal comparatively simple, and the result of the treatment in the highest degree successful and satisfactory.

But before a uterine polypus has passed through the os uteri—in other words, as long as it is still intra-uterine, or shut up and contained within the uterine cavity—the disease has hitherto been usually regarded and described as entirely beyond the reach of legitimate diagnosis and treatment. "It very frequently happens," observes Dupuytren, "that polypi concealed in the uterine cavity, inaccessible to our senses and instruments, give rise to severe symptoms, the true cause of which *cannot* be determined."[2] "When polypi," he again states, "are entirely included within the uterus, the rational symptoms afford room only for conjecture; and examination by the finger or speculum are both alike insufficient."[3] "So long," remarks Madame Boivin, "as the polypus is concealed within the uterus, all that can be ascertained is the increased size of that organ."[4] "If the polypus," says Dr. Ramsbotham, "be still included within the uterine cavity, and if the mouth of the organ be closely shut, it is impossible to reach it by the finger, and consequently quite out of our power to ascertain its presence."[5] "So long," according to Mende, "as a polypus is enclosed in the uterine cavity, its diagnosis is scarcely possible."[6] "True uterine polypi, while they remain enclosed in the uterine cavity, furnish," observe Roche and Sanson, "none but equivocal symptoms, which may be confounded with those of pregnancy. These different symptoms may also depend on chronic inflammation of the womb; and it is often impossible to distinguish this affection from polypus. In the actual state of the science, there is but one case in which a certain diagnosis may be formed, viz., when, the neck being effaced, and partly opened, it is possible to feel the rounded tumor within."[1]

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[1] Read before Medico-Chirurgical Society of Edinburgh, November 21, 1849.

[2] Leçons Orales, vol. iii. p. 542.

[3] Ibid. p. 490.

[4] Practical Treatise on Diseases of the Uterus.—Heming's Translation, p. 200.

[5] Medical Gazette, vol. xvi. p. 406.

[6] Krankheiten des Weibes, p. 591.

These, and other passages that might be cited, show that intra-uterine polypi are generally considered at the present day as placed beyond the pale of any certain means of detection, or any possible means of operative removal. And some of the older pathologists, indeed, would seem to have believed that there was no necessity for devising such means, inasmuch as, in their opinion, no danger was connected with the disease as long as the polypus remained intra-uterine. They held that the great source of prostration and peril attendant upon uterine polypi—namely, the hemorrhage or menorrhagia which accompanies them—is not liable to supervene, till the polypus has passed through the os uteri. Levret, for instance, was of opinion that, as long as a polypus remained within the uterine cavity, there was no accompanying hemorrhage, and that floodings appeared only after the tumor had left the uterine cavity.[2]

Several years ago, I saw, with Dr. Alexander Wood, a case, the result of which was distressingly opposed to this doctrine.

CASE I.—The patient was about fifty-five years of age, and unmarried. She had been suffering long under severe menorrhagia. The face was pale and anæmic, and her health and strength broken down. On examining, per vaginam, the os uteri was found closed; but the uterus felt somewhat large and distended; and Dr. Wood believing with me, that the hemorrhagic drain which was present might be the result of an intra-uterine polypus, the mechanical dilatation of the uterine cavity was advised, but given up in consequence of local treatment being objected to. In a few weeks the patient sank, under the continuance of the hemorrhage. On opening the body, Dr. Wood and I found the lower part of the cavity of the uterus distended by a polypus, of the size of a small plum, and attached to the back wall of the uterus by a narrow half-broken stalk. The lining-membrane of the uterus was white and bloodless, but the polypus was red, from engorgement and effusion of blood in its tissues. Its structure was fibrous; and there was another small fibrous tumor imbedded in the walls of the uterus, near the uterine extremity of the right Fallopian tube. It had descended lower down than at the time we examined, so as to have already dilated the cavity of the cervix; and the os, at the time of death, had begun to open. It was evident that, if the cavity of the os and cervix could have been artificially dilated during life, the polypus would have come within reach, and the patient's life been saved.[1]

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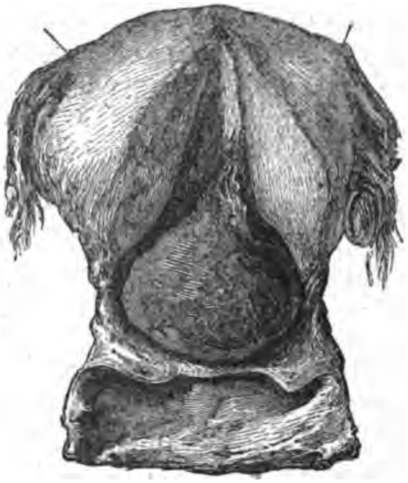
[1] Nouveaux Elémens de Pathol. Med. Chir. tom. iii. p. 284.

[2] Levret, Sur la Cure Radicale de Plusieurs Polypes de la Matrice, p. 25, &c. &c.

Fig. 4.



I have seen several other cases of intra-uterine polypus, where the hemorrhage was both long in continuance and great in quantity. Some years ago, along with the late Dr. Henderson of Corstorphine, I excised a slender pendulous polypus, hanging from the os uteri, in a patient who, some time previously, had nearly died of excessive uterine hemorrhage of several days' duration, at Leamington; but at the period of that dangerous attack, the attendant physicians had been unable to discover any uterine organic disease, to account for the discharge. The polypus had not yet passed the os uteri.[2]

When nature, in cases of intra-uterine polypi, begins to expel the tumor, and open up the os uteri, we may, at that stage, as stated in a preceding quotation from the work of Roche and Sanson, find it possible to make a diagnosis of the disease by being able to "*feel* the rounded tumor within." If art could furnish us with any means of producing, at will, the same extent of opening of the os uteri, it would enable us in the same way to "*feel* the rounded tumor within" with our finger; and it is evident that, by this means, we would possess a power of detecting, with all the certainty of physical diagnosis, the existence or not of the disease within the cavity of the uterus, in cases in which the attendant rational symptoms—as the menorrhagia, uterine leucorrhœa, and perhaps the swelled state of the neck or body of the uterus—might lead us to conjecture the probable presence of an intra-uterine polypus.

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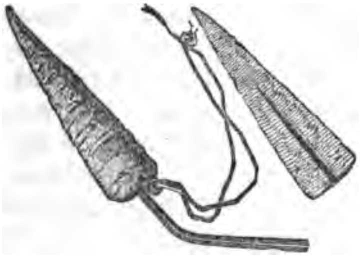
[1] The uterus and its included polypus, from this patient, are in the University Museum; and the above wood-cut presents a faithful sketch of them, diminished two-thirds, made from the preparation, and showing the size and site of the polypus, its place and mode of attachment, and the slight dilatation of the os, that had taken place before death.

[2] In this, and in one or two other instances in which I have seen extreme degrees of flooding attendant upon small polypi, the narrower elongated polypus was of a cellular structure internally, and externally spotted and roughened over by numerous small linear-placed elevations, like those on the shell of the echinus.

In 1844, in a communication[1] laid before the Medico-Chirurgical Society of Edinburgh, I proposed a means of safely opening up the cavity of the cervix and body of the uterus, to such an extent as might enable us to introduce a finger into the uterine cavity, for the purposes of diagnosis and operation in this and other diseased states of the organ. The means described consisted in the introduction of sponge-tents into the os and cavity of the uterus, so as gradually to dilate these parts to the degree required. For several years past I have been constantly employing this means of dilatation of the uterine os and cavity, for a variety of purposes and indications. The sponge-tents used by myself and my professional brethren in Edinburgh are manufactured by Duncan, Flockhart, and Co. They are of a narrow conical or pyramidal form; and used of many different sizes and lengths, according to the object in view. One of nearly the medium size is represented in the next cut. These tents are made by dipping a piece of sponge in a strong solution of gum-arabic, tying and compressing this sponge around a central wire, as its axis, into the required conical form, by a continuous layer of whip-cord, drying it thoroughly, removing the cord, and subsequently slightly coating the surface of the tent with tallow, or axunge and wax, to facilitate its introduction. The central wire passes only for half an inch or an inch into the base of the cone (see section of one above); and the opening left by it serves as an aperture to transfix the tent with the tip of the metallic director

Fig. 5.



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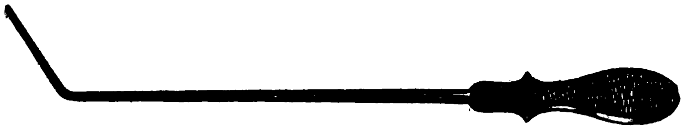
[1] "Mechanical Dilatation of the Cavity of the Os and Cervix of the Uterus, as a Means of Diagnosis and Treatment in some Affections of that Organ." See abstract of it in the Monthly Journal of Medical Science, August, 1844, p. 734.

(Figs. 5 and 6), used for guiding and introducing the tents through the os uteri. This metallic director, which, at the distance of nearly

Fig. 6.



two inches from its extremity, is bent at an obtuse angle, thence tapering to a blunt point, is introduced like the uterine sound or the catheter; its handle, with the sponge affixed to it, is held and manipulated by the left hand, while the fore-finger of the right hand touches the os uteri, in order to guide and direct the apex of the tent into that opening. The old forms of sponge-tent used by surgeons, and made of sponge steeped in preparations of wax, required for their expansion and development the aid of heat, in order to dissolve their retaining ingredient. The tent I have described, made by steeping sponge in a solution of gum, requires moisture, and not heat, for the solution of its retaining material, and for the expansion of the sponge. Very generally the secretions of the surrounding mucous canal afford a sufficient quantity of moisture for these two purposes; but if not, a small quantity of tepid water may be injected from time to time into the vagina. Usually a well-made tent takes twenty or thirty hours to expand to its full extent in the os uteri; and dilates to four or five times the diameter it presented in its orginal compressed state. Generally the first tent opens up the os and cavity of the cervix, and allows the finger ample space to examine sufficiently its contents, and the state of its parietes. If it is necessary to open the uterine cavity higher, to enable the finger to pass into the cavity of the body of the organ, a succession of tents are usually required; and they must be passed completely through the os internum or narrow portion, lying between the cavity of the cervix and cavity of the body of the organ. The use of the tent for a day, generally, as I have already stated, dilates the os uteri and cavity of the cervix sufficiently; and the employment of the sponge is accompanied with little or no feeling of uneasiness. When it is necessary to examine the state and conditions of the interior of the cavity of the body of the organ, the persevering use of a series of larger and larger tents for several days is usually requisite; and the dilatation of the os internum and body of the organ sometimes, but not always, causes a feeling of uneasiness and pain, that may require the use of an opiate. I have omitted to state that the tent is always prepared with a string affixed to its base, to allow of its easy removal. (See Fig. 5.) In using sponge-tents, it should be remembered that, when sponge is in contact with the maternal passages for some hours, it always exhales, when removed, a very fetid odor.

For dilatation of the unimpregnated os uteri, the tent should be selected as regularly conical as possible; and with the apex neither too blunt and rounded to pass the os, nor too slender and flexible, so as to double back in the attempt. The spirally grooved surface of the tent, resulting from the compression of it by the whip-cord during its manufacture, tends to retain it in situ, till its expansion commences. It perhaps ought to be added, that the introduction of the sponge-tent into the os and cavity of the uterus should be effected without the use of the speculum. The sense of touch serves in this and some other analogous operations, infinitely better than the sense of sight.

By the use of sponge-tents introduced daily, and of increasing size and length, we may reach a polypus when affixed and sessile even upon the fundus uteri. One of the first cases in which I dilated the uterine cavity to its extreme height, was the following:

Case II.—In 1844, a patient, æt. thirty-six, under the care of Dr. Graham of Dalkeith, had a miscarriage, from the effects of which she never satisfactorily recovered. Previously she had borne four children. When I first saw her, two or three years afterwards, she was emaciated and extremely pallid from the excessive loss of blood which she had been sustaining for some time; and her weakness was such, that it was with difficulty she could rise and walk across her bed-room. In August, 1847, I dilated fully the interior of the cervix and body of the uterus by a succession of sponge-tents, and at last felt a hard round fibrous polypus seated at the very fundus of the uterus, and projecting, of the size of a walnut, into the upper part of the cavity of the organ. Dr. Ziegler, Dr. Toogood of Torquay, and other professional friends, confirmed this diagnosis. It was impossible to ascertain how it was pediculated, or to operate upon the pedicle. We could only reach the round body of the tumor; and that I compressed strongly and repeatedly in the blades of a lithotomy forceps, with the view of breaking down its tissue, so as to destroy the vitality of the polypus. A purulent discharge followed, and three largish pieces of organized structure were subsequently cast off. Her recovery of health, after these discharges ceased, was gradual but perfect. There has been no return of menorrhagia. About a year ago she called upon me, and the change from excessive pallor and emaciation of the face, to the hue and ruddiness of health, was so great, that I had difficulty in being convinced of the identity of my former patient.

The *Symptoms* which might, *a priori*, induce a practitioner to conjecture the probable existence of an intra-uterine polypus, are, as we have seen in the quotations I have already given, of a very uncertain and equivocal character. The polypus, while still included within the uterus, is principally liable to give rise to the following groups of symptoms:

1st. Menorrhagia, in consequence of the discharge of blood from the surface of the tumor.[1] The attendant hemorrhages take place particularly at the menstrual periods, but are apt to recur also at other times; and the blood is sometimes fluid, sometimes coagulated; occasionally there is an almost constant red-stained discharge. The effects of these repeated floodings upon the constitution of the patient vary with their amount; but if they go on increasing, as they usually do, in quantity and frequency, the patient's constitution becomes gradually more and more shattered and broken down by the amount of hemorrhagic discharge; and all the symptoms of anæmia, in their most marked degree, at last supervene, as pallor of the face and lips, great muscular debility, palpitation, vertigo, dyspepsia, œdema, &c.

2dly. The discharge of mucous, purulent, or serous matter from the cavity of the uterus, in consequence of the mucous membrane of the organ becoming often irritated, inflamed, and even ulcerated by the presence and pressure of the polypus. If a severe leucorrhœal discharge is present, and we ascertain by the speculum that it does *not* originate in ulceration or other morbid state of the external surface of the cervix, or of the vagina; and if we further detect, with the speculum, the discharge issuing from the cavity itself of the uterus, the probabilities of it originating in some pathological irritation within the uterus, will be necessarily increased. Sometimes the discharge, in cases of polypi, is fetid, especially if it be retained, or mixed with decomposing blood.

3dly. Increased size of the cervix and body of the uterus, in consequence of its interior being distended by the presence of the polypus, is traceable in those cases in which the polypus is of any great size. Not unfrequently intra-uterine, like vaginal polypi, are found combined with the presence of fibrous tumors in the walls of the uterus; and by these tumors the magnitude of the organ is increased, and its shape rendered more or less irregular. Fibrous tumors of the uterus are seldom or never situated in the walls of the cervix; and if the swelling and distension affect the cervix, there is consequently much more chance of its being a polypus and not an interstitial fibrous tumor, than when we have similar symptoms attendant upon a similarly augmented state of the body of the organ. Further, the probability of the disease being intra-uterine polypus would be increased, if, on successive examinations, we had an opportunity of ascertaining that the enlarged and distended state of the cervix was descending gradually lower and lower down towards the os; for polypi in their progress and descent, as seen in Case I. p. 123, gradually dilate the cervix from above downwards in the same way as happens in pregnancy or abortion. They are born by a kind of chronic labor.

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[1] The following notice of the source of hemorrhage in cases of uterine polypus occurs in the Proceedings of the Edinburgh Medico-Chirurgical Society, March 6, 1850, as reported in the Monthly Journal of Med. Science for April, 1850:

Dr. W. T. Gairdner having inquired whether Dr. Simpson could throw any light upon the source of hemorrhage in cases of uterine polypi, Dr. S. replied, that in certain cases of profuse hemorrhage, lasting for months, which had fallen under his own observation, he believed that the blood proceeded not from any laceration, but from the mucous surface of the tumor. If, as some authors asserted, the bleeding took place from the general mucous surface of the uterus, why should hemorrhage cease when the polypus was tied?—(Ed.)

4. There may be symptoms of irritation and pressure upon the bladder, rectum, &c., if the polypus happen to be so large as to exert mechanical compression upon these or other parts, or dysmenorrhœa if it fills up the cavity of the cervix. And sympathetic pains may be present in the loins, limbs, &c.; or there may be sympathetic disturbance of the stomach, heart, &c., if the uterus is much irritated and excited by the presence and distension of the polypus.

But one or more of the preceding groups of symptoms may be altogether absent, though the uterus contain an intra-uterine polypus. The mechanical and sympathetic symptoms last alluded to are the most uncertain of all. For while almost all uterine diseases, however intrinsically different, give rise to similar secondary and sympathetic symptoms, we have often in other instances of the very same diseases, these same symptoms entirely wanting; just as in one woman during pregnancy we sometimes see severe, even serious, local and constitutional symptoms; and in another woman, or even in the same woman in another pregnancy, we see the same condition of the uterus unattended by any special, local, or constitutional disturbance. Again, there may be no ascertainable increased volume of the uterus, as the polypus, especially if it is vesicular, and originates in the interior of the cervix, may be far too small to lead to any appreciable augmentation in the size of the organ, although, notwithstanding, the menorrhagia may be great; for the extent of flooding does not depend on the size of the polypus, small polypi like small hemorrhoidal excrescences, often being the source of severe and repeated hemorrhages. Further, the leucorrhœal discharge which is sometimes attendant, may be entirely absent, as the polypus may not be irritating the mucous surface of the cavity in which it is enclosed. And lastly, polypi occasionally, thoughnot very frequently, are present for a long series of years without producing any degree of hemorrhage or menorrhagia. In the following case, for example, there was a state of long-standing amenorrhœa, instead of menorrhagia, coexistent with the presence of a polypus, though the two conditions, the amenorrhœa and polypus, had probably no causal relation to each other.

CASE III.—A poor woman, from East Lothian, aged about 35, and of a weak and debilitated frame, came, some three or four years ago, to ask for advice regarding the state of her health. She described her case as one of long-standing amenorrhœa. For five or six years the catamenia had been entirely absent; and she ascribed her broken health to this cause. On examining the uterus and ovaries, in order to ascertain if there was any organic change to account for the amenorrhœa, I found, with the uterine bougie, the cavity of the os and cervix uteri very small, and the latter apparently obstructed about three-quarters of an inch from the orifice. I introduced a long thin sponge tent, with the view of determining more correctly the state of the cervical cavity. On removing the sponge, two days subsequently, I found the lower part of the cervix natural, but a flattened polypus, of the size of a small cherry, attached by a short pedicle to the interior of the higher portion of the cervical cavity. The pedicle was easily seized with a pair of long slender polypus-forceps, and separated by torsion or avulsion. For some time subsequently to this little operation, menstruation recurred—the irritation of the sponge tent having probably so far roused the uterus to a restoration of its secreting functions; but a patient from the same neighborhood, about half a year ago, informed me that her health had relapsed again into its former unsatisfactory state.

The polypus, in the preceding case, was intra-uterine. During the past autumn I removed a uterine polypus, which had long passed down into the vagina, and yet had never given rise to menorrhagia.

CASE IV.—The patient, 55 years of age, had, for at least twenty-five years, been aware of the occasional protrusion, between the labia, of a portion of what she supposed a fold of thickened and insensible skin. When she first noticed it, she had called the attention of her medical attendant to it, an eminent London obstetrician, under whose kind care she was for many years placed. He examined the tumor and its relations; but advised her to let it alone. Two or three years ago a little sanious discharge began to appear, and continued to recur almost daily. On examining the projecting body, I found it an elongated polypus, of the size and figure of the fruit of the date, and depending by a long slender stalk, which passed upwards through the os uteri. I divided the stalk with a pair of blunt-pointed scissors, immediately below the os uteri, and in four days afterwards the patient set off on a long journey. The polypus was of a dense cellular structure. At one point, near its fundus, its surface was ulcerated. The ulcer was about the size of a sixpence, and, no doubt, the source of the discharge that had latterly appeared. Perhaps the removal of this polypus, when it was first discovered, would have enabled the patient to become a mother, and saved from extinction one of the highest and oldest titles in the kingdom.

Cases, however, like the above, of uterine polypi, of long duration, without attendant hemorrhage, are exceptions, and not very common exceptions, to the general rule. And certainly the existence and return of attacks of menorrhagia, draining and undermining the powers of the constitution—without the presence of any ascertainable organic disease in the vagina, or around the os uteri, to account for the floodings, and the persistence of this discharge, in despite of all constitutional care and treatment—forms always the most frequent and principal symptom that would induce the practitioner to use means to ascertain if there existed an intra-uterine polypus, or any other intra-uterine lesion, that was the probable source of the hemorrhage. He would, a *priori*, have more expectations of detecting, in his investigation, an intra-uterine polypus, provided, along with the menorrhagia, there was an occasional leucorrhœal or sanious discharge, coming—as proved by the speculum—from the cavity itself of the uterus, and not from the surface of the cervix; and provided, also, there was an increased size or misshapen state of the cervix or body of the uterus, such as might result from the enclosure and distension of a polypus.

To convert, however, the probability derivable from such symptoms into a certainty, we must endeavor to read the true value of these rational symptoms by obtaining access to the cavity of the uterus itself, and ascertaining, by a digital examination, if a polypus be present in that cavity or not, or if there be any other coexistent uterine lesion, capable of accounting for the symptoms. It is becoming every day more and more acknowledged, that we can alone discover uterine diseases, and discriminate them from each other, by appealing in this way to the evidence of physical diagnosis. And no remark could be, pathologically and practically speaking, more sound and true than that which Sir Charles Clarke many years ago made: "The true character of any disease of the internal female organs can *only* be ascertained by examination."[1] With this view, in order to enable the finger to reach and examine the cavity of the uterus, the os and cervix must be opened up by a succession of sponge-tents in the way already described. When an adequate degree of dilatation is obtained, the finger will be enabled to touch the tip of the polypus; and then the pediculated or polypous character of the tumor may be farther made out by passing either the finger or a uterine sound between its body and the containing cavity of the uterus. In making this examination, as in making most other examinations of the uterus, a rule requires to be followed which is too often forgot, namely to use both hands for the purpose. For if we are examining the uterus internally with the forefinger, or fingers of the right hand, the facility and precision of this examination will be found to be immensely promoted by placing the left hand externally over the hypogastric region, so as to enable us by it to steady, or depress, or otherwise operate upon the fundus uteri. The external hand greatly assists the operations of that which is introduced internally; and farther we can generally measure, between them, the size, relations, &c., of the included uterus.

If without, or before, using sponge-tents, we are desirous to examine at the time when the os uteri is naturally most relaxed, we shall find that time to be either immediately after a menstrual discharge, or immediately subsequent to any severe attack of intercurrent hemorrhage. Under such circumstances, we can sometimes introduce the finger partially into the os uteri, and ascertain the presence of any morbid body in the lower segment of the cervix; when in the same patient, at other times, this orifice is so completely shut as to prevent entirely such a proceeding. Sometimes, indeed, a small or elongated intra-uterine polypus will pass through the os uteri at these times, so as to be felt by the usual vaginal examination, but it will become retracted into the cavity of the cervix during the interval between the hemorrhagic discharges. In the following case[2] this occurrence was observed:

CASE V.—About eight years ago, I occasionally saw a patient who suffered much from leucorrhœa and menorrhagia. At last her health became so much broken in consequence of these discharges, and the pallor of the face and lips, and other symptoms of anæmia, so alarmed the patient, that she agreed reluctantly to submit to a vaginal examination. She had an objection, however, to me on the score of youth; and the late Dr. Beilby was so good as to make the exa-

[1] Diseases of Females, vol. i. p. 250.

[2] See notice of an analogous case, by Dr. Ramsbotham, in the Medical Gazette for 1835, p. 406.

mination, and found a polypus, of the size of an almond, projecting from the lips of the os uteri. On Dr. Beilby returning, two or three days subsequently, to put a ligature around the neck of the polypus, none could be found, and the os uteri was shut. The other symptoms, however, did not change; and, on the recurrence of a new hemorrhage, Dr. Beilby made another examination, again found the polypus protruding, ligatured, and removed it.

In this instance, as in many others, the passage of the polypus through the os uteri did not produce any appreciable degree of pain. In enumerating the symptoms of intra-uterine polypus, I have omitted to state that, like polypi which have passed through the os uteri, they very rarely are attended with feelings of pain; and too often, both by the patient and the practitioner, the absence of pain is erroneously supposed to be a proof of the absence of organic disease. Sometimes, however, as they are pressing upon the lower part of the cervix and os uteri, or distending and passing through these parts, uterine contractions and pains temporarily supervene, similar to those of miscarriage; and, if there is any difficulty in the passage of the tumor, these pains may become exceedingly severe. In a case in which a fibrous tumor of the uterus had undergone the calcareous degeneration, and part of it had assumed a semi-pediculated or polypous form, the recurrent pains, when the mass came down upon the os uteri, appeared at times as extreme as those of the last stage of labor.

CASE VI.—The patient, now sixty-nine years of age, the mother of several children, had for several years suffered from recurring slight attacks of uterine hemorrhage. In February, 1848, I saw her, with Dr. Hunter. The os uteri was drawn up so high, that it was with great difficulty I could reach and touch it; the top of the vagina stretched up in the form of an inverted funnel, the apex being placed at its upper or narrow extremity, and hence it was impossible to introduce or use a speculum. At the same time, the abdominal parietes were so thick and full, that it was impracticable to ascertain in any way the state of the uterus by an external examination. Not feeling a polypus, however, I left with the idea that the cause of menorrhagia was some form of carcinomatous disease of the uterus. Subsequently, in the month of July, all her symptoms became aggravated, and very severe bearing-down pains were superadded. These pains recurred regularly once a day, lasted in paroxysms for several long hours, and always left the patient weakened and prostrated. In consequence of their persistence, Dr. Hunter made another examination of the vagina, and found the os uteri, which was now pressed lower down, filled with an apparently irregular bony mass. I saw her again, and removed the calcareous mass, filling up the os uteri, with a portion also of fibro-calcareous tumor which we found above it, and distending the lower part of the cervix. The irregular calcareous portion protruded through the os uteri, was about the size of a hazel-nut, and the portion of fibro-calcareous tumor above it nearly four times that volume. The daily fearful pain which the patient had been lately enduring immediately ceased, and everything looked so favorable that we had every hope that the whole of the fibro-calcareous tumor, or polypus, had been removed. Last February, however, after some unusual exertion, the pains again recurred more severely, if possible, than before; and with this difference, that the attacks of them were now twice a day, instead of being only once, as on the first occasion. Opiates and sedatives had little or no effect towards their alleviation. On examining the os uteri, no new foreign body could be found anywhere within reach. As the patient's strength and spirits, however, were rapidly giving way, I dilated the os fully, by a succession of sponge-tents, and found the cavity of the cervix occupied by another fibro-calcareous mass, larger than the first. After an ineffectual attempt to break it down and remove it, by strong lithotomy and other forceps, I dilated the os still farther with tents, with the view of, if possible, getting two or three fingers up to seize the tumor, and assist in its detritus and extraction. To allow the hand to pass into the vagina for this object, I was obliged to incise its orifice; and, after no small difficulty, I was enabled to break off, by the fingers and forceps, four or five fibro-calcareous pieces from the mass in the cervix; and these pieces, when afterwards conjoined together, were found to form a roundish semi-pediculated tumor, of the size of an orange. In order to enable her to sustain the pain of these proceedings, the patient was kept, during this tedious operation, under the influence of chloroform. The pains again ceased from the date of the removal of this second intra-uterine tumor; and, under the kind care of her son, himself a physician, our patient made a good and steady recovery, and her health was restored by spending some of the autumn in the country. There still, however, remains in the uterine parietes some fibro-calcareous structure, as I lately ascertained by passing a uterine bougie into the elongated cavity of the uterus, and striking it against its hard stony surface.

The *Treatment* of intra-uterine polypi requires to be varied according to different circumstances, but particularly by the tendency or probability of the tumor passing downwards or not through the os; by the effects of the symptoms or the urgency of the case; and by the size and site of the polypus.

Two plans of procedure may be followed according to the nature and necessities of the case, viz., first, to wait till the polypus descends farther; or, secondly, to remove it immediately. It is a generally acknowledged principle in obstetric surgery, that a polypus of the uterus should be extirpated as early after its discovery as possible.[1] But when such a tumor is discovered still included within the uterine cavity, and the polypus seems gradually but certainly making its way downwards through the cervical cavity, and the hemorrhage and other symptoms are not urgent, it will assuredly be better to wait for its descent through the os; for after that its removal becomes much more easy and simple. The dilatation of the os and cervix by the sponge-tents will promote and facilitate its descent; and perhaps the internal use of the ergot of rye may aid it. But the degree of attendant hemorrhage and debility may be too great to entitle us to postpone the removal of the polypus; or the tumor may be attached by such a short pedicle as not to be capable of leaving the uterine cavity without dragging down with it, or inverting, the fundus or some part of the parietes of the uterus;[2] or it may be retained in its descent by adhesions formed between the surface of the uterus and the surface of the polypus. I once witnessed the dissection of a case of large fibrous polypus included in the cavity of the uterus, and where inflammation had been present before death; the surface of the polypus was adherent to the surface of the uterus through the medium of a recently effused false membrane.[3] Even when an intra-uterine polypus has descended so far as even partially to open up the os uteri, it may remain in that situation for such a length of time, and with such results, as to place the patient in no small degree of danger. I shall quote, in illustration of this remark, an interesting case reported by Dr. Meigs, of Philadelphia, in his work on Female Diseases. Dr. Meigs,[4] who quotes Dr. Lee, to the effect, that "it would be folly to attempt the removal" of a polypus still retained in utero, details the case referred to in the following words:

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[1] "In the treatment of this disease (uterine polypus) the first principle, undisputed, I suppose, by those who are possessed of experience in the management of these morbid growths, is, that it ought by all means to be extirpated; but unless it be removed, it will continue to grow larger and larger, till it utterly wears out life, and this especially if it be shooting from the upper part of the uterus, or even from the neck. It is, moreover, of vast importance in polypus, not only that it should be extirpated, but that this extirpation should be accomplished as early as possible. Lay this down, then, as a most important part of your practice, that polypi are not only to be taken away, but that they are to be extirpated early, as soon as they are discovered, and as soon as it is practicable."—Blundell's Observations on Diseases of Women, p. 126.

[2] Cases of intra-uterine and vaginal polypi tending thus to invert the uterus at the site of the pedicles are detailed by Denman, Introduction to Midwifery, p. 106; Davis, Obstetric Medicine, p. 618; Dr. Oldham, Guy's Hospital Reports, New Series, vol. ii. p. 105; Scoutetten, Gazette Medicale, for August, 1839; Crosse, Transactions of Provincial Medical Association for 1845, p. 321, &c.

[3] Library of Medicine, vol. iv. p. 335.

[4] Females and their Diseases, p. 255. Philadelphia, 1848.

CASE VII.—"Some months ago a lady came to me from New Jersey. She had been for some years laboring under a uterine disease, accompanied with violent and exhausting floodings. Upon arriving here, she was wholly unable to walk or sit up in her chair. I discovered a hard polypus, whose apex was lying just within the os uteri, which was a circular opening as large as a half dollar. The os uteri was pretty low down in the pelvis, it was very hard, and completely undilatable. The fundus uteri was half way up to the umbilicus, and the uterus hard and solid, so as to allow me to trace its outlines very clearly in my hypogastric palpation. I assure you I have rarely met with a more extreme case of anæmia than in this person. This anæmia was evinced not only in the pallor of her surface, and its flabbiness, and in her irregular breathing, the frequent palpitation of the heart, and the anæmiacal throb of her pulses, but in the state of all her innervations, which were most miserable indeed, except when lying profoundly still in a low recumbency.

"After a few days' refreshment from the journey, I attempted to do what I thought I should fail to do, namely, to get a ligature on the tumor. But I soon found how vain was such an attempt, for I never found the uterus a moment to relax, nor open beyond the size of a half dollar. My attempt caused an attack of hemorrhage to come on, that I was glad to suppress by cold, by rest, and by opium.

"I kept her here many months, in hopes of seeing the uterus enter into powerful contractions to throw off the morbid mass. I gave her large doses of ergot. I thought the ergotism that was produced might expel the polypus, but I was disappointed, and subsequently had reason to believe the tumor had formed strong attachments to the inside of the uterine walls, so low down, that I could reach them with my finger, but could not break them up.[1]

"During her residence here, I thought to see her bleed to death before my eyes; her life was hardly saved by the tampon, so perverse was the hemorrhage. At length I sent her home, with directions as to her health, and a request to be informed if the tumor descended into the vagina. It will never descend into the vagina, if the adhesions I supposed to exist are truly there."

But, secondly, the severity of the attendant hemorrhages, or the improbabilities of the speedy and entire descent of the intra-uterine polypus, may induce us to remove the tumor at once; and certainly this may be effected in most cases, though with greater difficulties than in cases where the polypus has passed down into the vagina. To admit at all of the removal of an intra-uterine polypus of any considerable size, the os uteri must be previously very fully dilated by sponge-tents; and perhaps it will sometimes be found necessary, at the time of operating, to gain additional freedom, by dividing any obstructing band of the os or cervix that may not have been fully dilated by the tents. Afterwards, we shall require to proceed differently in different cases, in order to destroy or remove the polypus. We may only be able to accomplish this object by contusing and crushing the tumor, as I have described in a case already detailed. See Case II. In the instance in question, I grasped the polypus, for this purpose, with strong lithotomy forceps. In another similar case, after fully dilating the os and cervix, I seized a large intra-uterine polypus between the jaws of a screw-propelled lithotripsy instrument—invented for the purpose of crushing vesical calculi—and was enabled, by it, to crush and destroy readily the structure and vitality of the included tumor. Occasionally, we may be enabled to divide the stalk of the polypus with a silver wire or ligature, acting on the principle of the chain-saw; or we may reach it with very curved blunt-pointed scissors. The two following cases may serve to illustrate these two last-mentioned methods of operating:

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[1] The use of the uterine bougie would probably have determined this point; or the mechanical dilatation of the os by tents would have enabled the finger fully to reach and break the adhesions.

CASE VIII.—A patient, æt. 36, about three years ago began to suffer under menorrhagia and dysmenorrhœa. The catamenia became both too frequent in their return, as well as much too great in quantity; but there was little or no leucorrhœal discharge. Latterly coagula of blood accompanied the menstrual periods, and the patient felt much weakened by each attack. The dysmenorrhœa generally supervened on the second day of menstruation, and confined the patient for a couple of days, the third day being usually one of much sickness as well as pain, particularly if the patient tried to assume the erect posture. I first saw this lady in July of the present year, and found the uterus somewhat enlarged, and externally irregular in form, from the presence of one or two small fibrous tumors in its body and fundus. But the os uteri was shut, and I could not ascertain if the debilitating hemorrhage was the result merely of the irritation of these tumors in the parietes of the uterus; or whether one of them, forming a polypus in the cavity of the organ, was its source. I wrote her medical attendant to dilate the os in order to determine this point; and she returned home to

England. In September she came back to Edinburgh; but, in consequence of the state of her health, I did not venture to dilate fully the os and uterine cavity till towards the end of October. On doing so, I was enabled to detect the rounded extremity of a polypus hanging down into the cervical cavity. During two or three days it descended somewhat lower, but ultimately remained fixed and stationary above and within the os. I found I could not move it further downwards by fixing a vulsellum into it, and applying some dragging force. On the 6th November, assisted by Dr. Duncan, I applied a silver wire above the body, and around the neck of the tumor, by means of an instrument presently to be described. After the instrument was fixed and adjusted, a few turns of the screw made the wire cut through the pedicle of the polypus, and without any pain or suffering on the part of the patient. The separated tumor was then pulled by the vulsellum through the os uteri. The polypus was of the size and shape of a plum, with a small portion of the pedicle attached. It was fibrous in its internal structure. The patient's recovery was slow, but uninterrupted. She has menstruated once since the operation, but without the discharge being excessive, as formerly, either in quantity or duration (it lasted only three days); and also without her former distressing dysmenorrhœal pains.

The instrument employed in the preceding case was a modification of one kindly sent to me by my friend Dr. Sabine, of New York. I am told it has been successfully used by various American practitioners for the removal of polypi in the vagina. The advantage which it possesses over the instruments of Niessens, Gooch, Davis, and others, in the removal of intra-uterine polypi, is, that the screw power with which it is furnished enables us to use it with the power of a small chain-saw, for the immediate division of the pedicles of the polypi. And it is almost superfluous to observe, that if we can finish our operation at once, it will be much safer for our patient than leaving a rough instrument within the cavity of the uterus. The instrument itself consists of two parts, viz.: two hollow canulæ, like those pertaining to the instruments of Niessens and Gooch; and of a second part, resembling the polypus instrument of Graefe, of Berlin, with this difference, that it has a ring affixed to its top, of a heart-shaped figure, and intended, first, to receive the two canulæ, with their contained ligatures, and afterwards to serve as a point of resistance during the cutting action of the ligature upon the pedicle of the tumor. The canulæ and ligatures are first applied in the same way, and according to the same rules, as those of Niessens and Gooch. After the pedicle is encircled by the ligature, the two lower extremities of the canulæ and included ligatures are passed through the ring of the second portion of the instrument. This second portion of the instrument is then run up, with its ring surrounding the included canulæ, till it reaches the pedicle of the tumor; the projecting side of the ring being turned towards the pedicle. The canulæ are then slipped off and withdrawn, leaving the wires or ligatures alone in the terminal ring of the instrument. Subsequently these wires are twisted around, and fixed upon the knob attached to the screw. Lastly, by moving the knob downwards, by the operation of the screw, the ligature is made to cut into and through the pedicle.[1]

In the following case, I was enabled to divide the pedicle of a large intra-uterine polypus with a pair of well-curved blunt-pointed scissors.

Case IX.—The patient was 48, and unmarried. About fourteen years ago she was first seized, when in service, with a severe flooding. It returned at short intervals, and reduced her strength so much that she was obliged to leave her situation, and has never been able to take another. The hemorrhage she describes as having been almost constant for many years; that is, there was always some red oozing, and fluid blood and clots often escaped when she made any exertion. She had been treated by various medical gentlemen during this period, chiefly with iron, styptics and astringents. A vaginal examination had never been made, in order to ascertain the source of the hemorrhage. During last autumn she presented herself for advice at my house. She was blanched, thin, and debilitated, and scarcely able to walk. On examination per vaginam, the uterus felt enlarged, more particularly in its cervical region. A sponge-tent was introduced, and on her returning, two days afterwards, I found a polypus descended upon the distended os. Dr. Ziegler, Mr. Carmichael, and Dr. Duncan, were present at the removal of the polypus. In order to reach, if possible, with the scissors, the pedicle of the polypus, I required to make a slight incision into the thin lips of the os. I was enabled at last, after some difficulty, and by seizing the polypus with a vulsellum on one of its sides, to turn the polypus laterally, and obtain access, with the scissors, to its pedicle, which was small and easily divided. After the polypus was completely separated, it took no inconsiderable amount of traction to drag it through the os uteri. The polypus was round, of the size of a small orange, and of a fibrous structure. The patient was rendered anæsthetic during the operation. The vagina was plugged with sponge, and the woman sent home. Next day the plug was removed. The patient has ever since kept free from any return of the flooding, and a degree of leucorrhœa, which followed, as often happens, the removal of polypi, is subsiding under the use of medicated pessaries. A month subsequent to the operation, she stated her strength to be greatly improved beyond what it had been for many years.

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[1] When this essay appeared in the Monthly Medical Journal, it was accompanied by an engraving of the instrument above described; but as Dr. Simpson has lately abandoned its use in favor of excision by the polyptome, as will be seen in a subsequent page, it has been thought unnecessary to introduce it here.—(Ed.)

The preceding remarks, relative to the treatment of intra-uterine polypi, principally refer to these tumors when they happen to be of a large size. But uterine polypi are often *too small* to be removed by the knife, scissors, or ligatures; and yet these small polypi not unfrequently lead to severe and long-continued menorrhagia. From the analogy of hemorrhoidal tumors, we know that the mere size of a polypus is not to be taken as any measure of its capability of producing hemorrhage. Small vesicular, mucous, or cellular polypi sometimes grow from the fundus uteri, giving rise to considerable and long-continued hemorrhagic discharge. I have preserved specimens of them from the dead subject, and have met with them in the living. They can hardly be properly termed polypi, as they are scarcely pediculated at their attachment, and sometimes short, but in other cases long and slender, in their body.

The following case may be cited as an illustration of this form of the disease:

CASE X.—A lady, the mother of ten children, became irregular in her menstrual discharge during her 44th year. At times it was wanting at the usual monthly periods, at others it amounted to menorrhagia. About a year after this irregularity commenced, such an amount of fluid blood and coagula escaped as at first to lead on her part to some suspicion of miscarriage; but it continued to go on profusely for two or three weeks. At the end of that time I visited her, with Mr. B. Bell and Dr. Malcolm. On examining the uterus, we found a small vesicular polypus attached to the inner surface of one of the lips of the os, and it was easily removed by avulsion. The discharge, however, was not abated in consequence, as we expected. A series of sponge-tents was then introduced, so as to open up, first, the cavity of the cervix, which was found free from additional polypi, and ultimately the cavity of the body of the uterus. When the distension of the whole uterine cavity was at last completely effected, both Dr. Malcolm and myself found that we could touch two or three small slender polypoid bodies, hanging from the very fundus of the uterus. I removed them cautiously, from the surface to which they were attached, with the nail of the first finger. After this the hemorrhage ceased, but some local treatment was required to cure the ulcerated state of the cervix. The polypi were removed in April. The patient went soon afterwards to spend the summer in the country, where she soon gained strength, and enjoyed much improved health. I saw her lately. The menorrhagia had not recurred, but she still looked anæmic, having never recovered her color since the hemorrhages in spring.

Dr. Malcolm informs me that, since meeting with the above case, he has seen another similar one, and treated it successfully in the same way. I may add, that in several cases of chronic and severe menorrhagia, in which I have been induced to open up the cavity of the uterus with sponge-tents, in order to ascertain whether there was any small intra-uterine polypus present or not, I have merely found the interior of the uterine cavity rough and granulated at particular points, which I have generally tried to remove and scratch

Fig. 7.

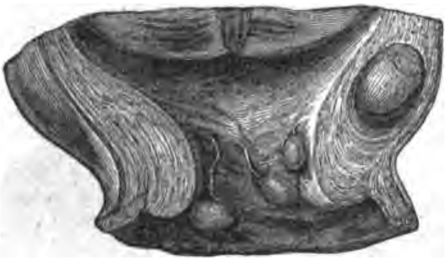


Fig. 8.

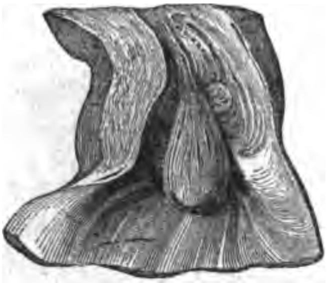


Fig. 7. From Cruveilhier's plates of Pathological Anatomy, fasc. xiii. pl. vi. Fig. i. The cut shows some small pediculated polypi of the cervix uteri. Here, as often happens, there was coexistent disease in the body and fundus of the uterus.

Fig. 8. Taken from a plate appended to a paper by Dr. Lee, in the nineteenth volume of the London Medico-Chirurgical Transactions.

off with the nail. Whether owing to their removal or not, or owing to the irritation resulting from the pressure and distension of the sponge, I know not, but certainly, in two or three cases, the menorrhagia has subsequently abated and ceased.

By far, however, the most common site for the origin of small vesicular polypi, is the interior of the cervix uteri. In fact, the small cellular or vesicular form of cervical polypus is infinitely the most common form of polypous disease of the uterus. Several specimens of them are represented in the wood-cuts. These cervical vesicular polypi are generally of a small size, like a pea, or orange pip, and vary from this size to the size of a hazel-nut. Sometimes they are sessile, and sometimes pediculated, as represented in the sketch from Cruveilhier (Fig. 7). Occasionally they are single (Fig. 8), or they form a single complex cluster; but more frequently they are gregarious, as represented in Madame Boivin's drawing of them, copied in Fig. 9. Indeed it is, I believe, the rule rather than the exception to it, that when we find one, perhaps protruding at the os uteri, as in Fig. 10, we shall find, on further

Fig. 9.

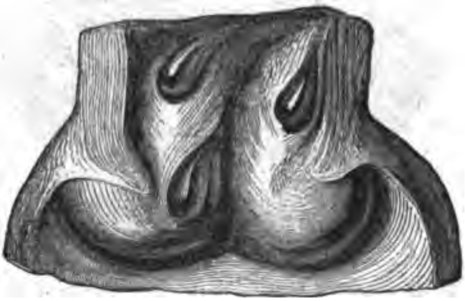
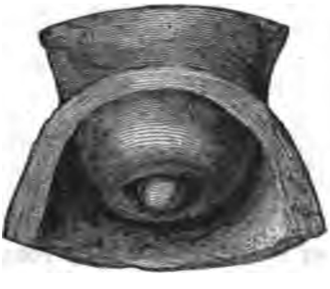


Fig. 10.



Figs. 9 and 10 are representations of a single uterus. In the latter a pisiform cystic polypus is shown projecting from the os, in the former the cervix is opened up, and three small cystic polypi seen on its walls. They are from Madame Boivin's work on Diseases of the Uterus, plate xviii. figs. 1 and 2.

search, that there are others, sometimes to the number of four, five, or six, springing from other points of the interior of the cervix, and not discoverable till the cavity of the cervix is dilated by a sponge-tent. When hanging from the os uteri, their stalk is sometimes so loose and long, and the small depending polypus is itself so small and soft, that it moves away before the finger in making a tactile examination, and one unaccustomed to this peculiarity will not feel perfectly sure of the presence of such a polypus till the speculum is used, when the polypous body will be easily seen, generally of a cherry-red or purplish color. Such polypi, though small, are often apparently the source of much menorrhagia and leucorrhœa, for they almost always coexist with, and probably produce, some degree of ulcerative inflammation of the contiguous surface of the cervix.[1]

In trying to remove these small vesicular polypi of the cervix, it is, therefore, to be held in recollection, that there are generally more than one present, and that to ascertain this point with any precision, it is necessary to dilate and expand the cavity of the cervix with a sponge-tent.

In more than one instance I have found these polypi, when their pedicles were perhaps long and easily broken, come away, imbedded in the surface and foramina of the sponge, which had torn them off during its expansion. But in twenty-nine out of thirty cases, more methodic measures are required for their removal—as scratching them off with the sharp finger-nail, seizing and tearing them off with polypus-forceps, or dividing their stalks with a pair of scissors. If we can use the speculum, these modes of removal are greatly facilitated by the sense of sight. Indeed, if we require to use the polypus-forceps or scissors, for the removal of these small polypi, and are guided by touch alone, we shall generally find the operation, though apparently simple in principle, one which is tedious and difficult to perform in practice.

In a considerable number of instances of obstinate slight menorrhagia and leucorrhœa, I have been enabled to detect the presence of vesicular polypi attached to the interior of the walls of the cervix uteri, by opening up the cavity of the os and cervix with sponge-tents, and have afterwards removed them by the methods alluded to. The following was one of the first instances in which I pursued this practice:

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[1] A small cervical polypus may even produce death by the extent of hemorrhage to which it gives rise. In an excellent practical paper on polypi, published by Dr. Locock, in the London Medico-Chirurgical Transactions for 1848, he states (p. 171), "Upwards of twenty years ago, the late Dr. Robert Hooper showed me a preparation of a uterus laid open, having a polypus not larger than a pea, with a short and narrow peduncle attached within the cervix, high up, considerably within the os uteri, and not perceptible till the cervix was slit open. All the history which he could give me was, that the uterus was removed from the body of a young woman, who had died in the Marylebone Infirmary from long-continued uterine hemorrhage."

Case XI.—A lady was confined of a premature child in early married life, and afterwards her health remained broken and wretched. She did not again conceive; and was unable to take walking exercise. There was a constant feeling of dragging and pressure about the pelvis. Betimes menorrhagia, and some degree of leucorrhœa, supervened. She was seen by many medical men in different parts of Europe. It was generally considered that there was a tumor on the back wall of the uterus; and for some years previous to my first seeing her, in 1842, she had been undergoing a course of local leeching, and other treatment, under the idea that the enlargement of the uterus was hypertrophic, and that her irregular menstruation was the result of congestion. I found the apparent tumor or hypertrophy of the uterus was formed by a complete retroversion of the organ. The cervix uteri was ulcerated, and I thought I could touch a small vesicular polypus, on pressing my finger against the os. I distended the cervical cavity with a sponge-tent; and, on removing it next day, I was easily able to trace three or four small cystic polypi attached to the interior of the cervix. I removed them, by picking each individual polypus carefully off with small forceps. An amelioration in the irregular menstruation immediately followed; and other means were subsequently adopted for the treatment of the other complications.

The small vesicular polypi of the cervix have sometimes, as we have already seen, long pedicles. Occasionally, however, we find, coexisting with these pediculated polypi, others that are non-pediculated or sessile: and, occasionally, after the cervix is dilated, we find others not raised yet above the level of the general surface of the mucous membrane of the part, but feeling imbedded like shot or peas in or beneath that membrane. In other words, we find, in some cases, these vesicular polypi in all their stages of formation, from small shut cysts, up to pediculated vesicular tumors. When such is the state of matters, we can only remove those that are more fully formed, by the nails, scissors, or forceps. To effect a complete cure, we require other means; and for this purpose the application of caustics to the mucous membrane of the cervix answers every indication. Nitrate of silver generally proves too weak for this purpose, unless repeated very often, and combined with scarification of the mucous surface. We possess a far more potent and certain caustic for the purpose, and one that is perfectly manageable, in potassa fusa. The surface of the os and cervix, when small vesicular polypi exist, are often found to be the seat of chronic inflammatory ulceration; and sometimes the submucous tissue, and the structure of the cervix, is also the seat of chronic inflammatory hypertrophy and induration. When such a combination exists, the potassa fusa is doubly useful, as its application at once destroys the polypi, and sets up a new and healthy action in the affected and morbid tissues of the cervix. I have described elsewhere, its great value and mode of application in inflammatory induration of the cervix,[1] and the power we have of immediately arresting and limiting its action by the neutralizing effects of acetic acid. I need only add here, that I have now repeatedly found this caustic of the greatest possible use in obstinate and complicated cases of vesicular polypi of the cervix, such as I have above alluded to. In illustration of its effects, I shall only cite one instance, and that because it was a case which was peculiar in several respects.

CASE XII.—On the 1st October last I was called into Roxburghshire by Dr. Anderson, of Jedburgh, to see a lady who had been losing large quantities of blood for three weeks previously, and in whom the hemorrhage had continued to go on profusely, day after day, in despite of all the means which he had tried for its suppression. The patient's strength had, in consequence, become greatly exhausted. She was between forty and fifty years of age, was the mother of a family, and for some years past had suffered under occasional menorrhagia. Three years ago a uterine polypus had been detected at Brussels, and afterwards removed in London, apparently with some difficulty, as the first physician who attempted it failed. Her present attack of hemorrhage was much more long-continued and severe than those that had occurred previously. Before being able to make a tactile examination of the uterus, I had to remove several large clots of blood lying in the vagina. I found the anterior lip of the os uteri very much enlarged, indurated, and roughened on the surface. By the speculum we saw this lip greatly enlarged and dotted over with small pediculated red-colored polypi, like red currants; and the use of the mop showed them to be the source of the flooding. About a dozen of these small red polypi were within the field of the speculum, but others could be felt on the internal aspect of the enlarged lip. As it seemed hopeless to attempt to detach them all one after another by the forceps, and as doing so would not remove the suspiciously indurated and enlarged anterior lip of the cervix, I at once had recourse to the application of potassa fusa to the diseased lip itself, and melted it down, with the polypi attached, by decomposing upon it a couple of sticks of potassa, of above an inch in length each, and followed this immediately by the free and abundant injection of vinegar to neutralize the alkali. Subsequently, under the use of astringent injections and medicated pessaries, the surface took on a healthy cicatrization, and her health greatly improved under Dr. Anderson's kind and able care. I saw the patient in Edinburgh two months afterwards, on her way home to London. There were no remains of the induration or polypi. The uterus felt natural in size, and the surface of the cervix was entirely cicatrized. There has been no recurrence of the menorrhagia. The menses have been present once, but not in unnatural quantity. (This patient has remained free from any return of the disease.—ED.)

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[1] See page 105 of this volume.

Since this essay was written (see page 125), dipping the piece of sponge for sponge-tents in gum water has been discontinued; experience having proved that simply moistening it in water before compression is sufficient, probably because sponge itself contains glutinous matter.—(ED.)

NOTE ON UTERINE HEMORRHOIDS.

Dr. Simpson has frequently pointed out to us a condition of the os uteri in many respects analogous to a hemorrhoidal state of the rectum. In this affection there are usually leucorrhœa and frequent hemorrhage, giving rise to all the consequences and effects of anæmia, as observed in patients who have lost blood in the way usually noticed in hemorrhoids. By the aid of the speculum, the os uteri is found roughened and highly injected, like the mucous surface of an internal rectal hemorrhoid, when the end of the bowel is everted. The part is apt to bleed when touched with the finger or a probe. Dilatation by sponge-tents discovers no polypus in the uterine cavity, but the roughness may be often recognized by the finger to a considerable distance within the cervical cavity. In treating this abnormal condition, we have seen the vascular portions scraped off by the instrument of Recamier, when deeply seated; or, if nearer the os uteri, and accompanied by an hypertrophied condition of the substance of the cervix, the potassa fusa answers the double purpose of destroying the hemorrhoidal mucous membrane, and reducing the induration of the general tissue.—(ED.)

OF THE EXCISION OF LARGE PEDUNCULATED UTERINE POLYPI,

AND ITS ADVANTAGES OVER DELIGATION.

(From Edinburgh Monthly Journal of Medical Science, January, 1855, p. 10.)

A VARIETY of operative means have been proposed and practised for the removal of uterine polypi, after they have once passed the os uteri and come within the reach of surgical interference. The means usually followed differ according to the size and consistence of the polypus.

In the removal of small and soft uterine polypi of the mucous or cellular type, the practitioners employ—1. Simple avulsion with proper forceps, or with the fingers; or 2. Avulsion by torsion; or 3. Compression or crushing of the neck or body of the polypoid growth; or 4. Scooping out its attachment with the nail, if the cervix is opened, or with the scraping or gouging instruments invented by Recamier and Dr. Locock; and 5. The introduction and application of a stick of *potassa fusa* inside the os, followed by the free application of vinegar, is sometimes most advantageously had recourse to, where, as is often seen in practice, several small, sessile or pediculated polypi—formed by dilatation of the Nabothian follicles—are attached to the lips of the os and along the interior of the cavity of the cervix uteri.

In the present communication, however, I wish only to speak of the larger and firmer forms of pediculated uterine polypi. These are usually fibroid or cellulo-fibroid in their structure, and vary from the size of a small walnut to that of a new-born child's head. In the removal of them one or other of the following operations is generally had recourse to, viz., either,

I. The slow and gradual division of the pedicle of the polypus by *Deligation*, or by the constriction of ligatures of silk, catgut, silver wire, etc., applied by means of various forms of canulæ; or,

II. The instantaneous division of the pedicle by *Excision* with the scissors, scalpel, or bistoury, and the consequent immediate removal of the amputated polypus.

Some practitioners combine together these two methods—applying deligation first, and then—either immediately or some days subsequently—using excision or resection in addition.

In this country the operation by deligation alone has been the plan usually adopted. "In England," as Dr. Ashwell states, "the ligature has *always* had a decided preference."[1] "British practitioners have," says Dr. Hamilton, "*universally* agreed that the safe mode of operating is by ligature."[2] "I shall," observes Dr. Ramsbotham, "consider this as a point *settled*, at least in our island."[3]

The removal, however, of large uterine polypi by excision, has long appeared to me to be in many respects a simpler and safer operation than their removal by ligature. It is, on the whole, more easily accomplished; the cure of the disease by it is infinitely quicker; it is accomplished with far less restraint and annoyance to the patient; with less risk of local irritation; and, as I believe, with less ultimate chance of actual peril to health and life.

Two objections have usually been urged by accoucheurs against the removal of large uterine polypi by excision. The first of these objections, viz., the danger of hemorrhage, has been particularly insisted on by those practitioners who have written in favor of deligation. But the fact is, that excessive hemorrhage is not common after division of the peduncle of the polypus; and however great that or any other traumatic hemorrhage from the unimpregnated uterus may be, it can always be arrested by properly filling the vagina for a few hours with pieces of sponge or other appropriate plug. Dupuytren, though using no plug or other means to prevent bleeding, only met with two cases of severe hemorrhage out of some two hundred instances in which he removed uterine polypi by excision; and Lisfranc only observed it twice out of one hundred and sixty-five similar operations practised by himself.[1] I believe, however, from what I have myself seen, that the practitioner, in employing excision, must expect considerable hemorrhage in a proportion of cases much greater than this; but at the same time he can, I repeat, always readily *arrest* it, when it does occur, with proper plugging; and it is perhaps best—as a general rule—to *prevent* and forestall its occurrence by introducing, for ten or twelve hours after every case of resection, a proper tampon of fine sponge into the vagina. Let me add, that it is well to have each piece of sponge which is used previously transfixed by a strong thread, the end of which should be left out of the vagina, in order to permit of the more easy withdrawal of the plug.

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[1] Diseases Peculiar to Women, p. 485.

[2] Practical Observations on Midwifery, p. 40.

[3] Medical Gazette for 1835, p. 433.

The second objection usually urged against excision is of greater weight, viz. that more or less injury is always liable to occur to the pelvic attachments of the uterus, if it and the polypus are forcibly dragged down by Museux's forceps, or by hooked vulsella, so as to bring the peduncle of the tumor into view before dividing it,[2] a plan followed by most operators;[3] or, on the other hand, if the peduncle is divided while the polypus and uterus are in situ, the scissors or knife employed are, it is averred, apt to injure and cut the vaginal walls and neighboring tissues, while worked within the vaginal cavity.[4]

Of late years I have used a means of excising large uterine polypi, that seems to me to obviate entirely this last class of objections, and by which, as I have been led to believe, the whole operation is much simplified, and rendered both greatly more easy to the practitioner, and more safe to the patient. By the means or instrument in question, the peduncle of the polypus is divided in situ, and without any chance of its cutting portion injuring the structures of the vagina or vulva.

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[1] Lisfranc's Clinique Chirurgicale, vol. iii. p. 210.

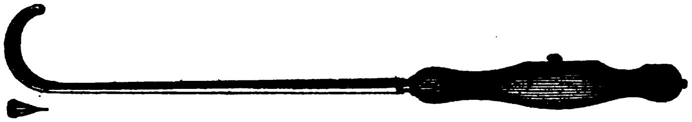
[2] Dr. Heming's Translations of Mad. Boivin's Treatise on Diseases of the Uterus, pp. 210, 211.

[3] When the texture of the polypus is too soft to allow, without tearing, of the mass being drawn downwards by vulsella, Lisfranc even advises the hooks of the forceps to be fixed in the cervix uteri itself, to get a sufficient hold for dragging down it and the polypus with it to the vulva.—Gazette Medicale, 1834, p. 149.

[4] Quia *secantia* instrumenta aegre in vaginam immitti possunt, ne partes vicinas laedent, et facile lethales hæmorrhagias gignant.—Nissen De Polypis Uteri, 1789, p. 34.

The instrument, or polypus-knife, to which I refer, is sketched in Fig. 11. It is of the form of the usual midwifery hook; with the concavity, however, of the hook not blunt, but turned into a cutting surface by the insertion of a small piece of well-tempered steel blade into it. A transverse section of the curved or cutting portion of the instrument, and of its included knife-blade, is also shown in the wood-cut. The entire length of the instrument which I have hitherto employed in my own practice, is ten inches—the length of the wooden handle being four inches, and that of the metallic shaft six inches. A shorter instrument might, perhaps, suffice equally well. The cut represents the curved portion, or hook at the extremity, as somewhat wider and larger than in the polyptome made for me in the first instance, and which I have generally employed in practice. Perhaps an increased or a diminished size and width, in the curved hook at the extremity of the instrument, would render the operation of division by it more easy, according as the stalk of the polypus is very thick, or comparatively slender. The extreme point of the instrument is blunt and rounded; and the cutting portion of the blade is so protected and concealed by it, and by the back wall of the curve, that it can be introduced into and withdrawn into the vagina, without any chance of its edge injuring or dividing the vaginal structures themselves. To be always able to discover the direction to which its hooked extremity points after it is introduced into the vagina, the front aspect of the handle is distinguished by having a slight knob or other mark upon it.[1]

Fig. 11.



In employing this polyptome, the stalk of the polypus is first to be reached by the apex of the first finger of the right hand, introduced along the short anterior or pubic surface of the vagina; the instrument is then pushed by the left hand along this finger as a guide, and passed over or above the peduncle of the tumor, in such a direction that the concavity of the hook will come down upon and embrace this peduncle, as the instrument is pulled downwards. The next step is to make the blade of the polypus-knife cut through the stalk of the tumor. For this purpose, a little simple traction, with a slight rolling or sawing motion, is all that is generally required. If the tissue of the peduncle is dense and strong, the dividing force of the instrument may be increased by the fore-finger of one hand being applied with a tractive power to the blunt extremity of the instrument, while the handle is dragged down and moved in a sawing direction, by the other hand of the operator. Sometimes when the polypus is round and loose, after the curve or hook is applied to its pedicle, the cutting portion of the polyptome will divide this stalk most readily, by merely doubling backwards with the fingers the body of the polypus upon its own stalk, and pulling the knife against the bent peduncle. In such a case, the peduncle is divided as much by pressing it against the knife, as by pulling the knife through the peduncle.

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[1] The use of the polyptome was first proposed to the Obstetric Society of Edinburgh at its meeting of June 25, 1851, at which time Dr. Simpson "showed two polypi, which he removed a few days before, by a new kind of instrument—viz., a hook, like Dr. Ramsbotham's decapitation instrument. One of these polypi was very large, and when the stalk was divided by the instrument in question, the polypus required to be removed from the vagina by a pair of forceps."—Edinburgh Monthly Journal of Medical Science, Nov. 1851, p. 492.—(Ed.)

During the last few years, I have removed a very considerable number of uterine polypi of different sizes, and some of them of large dimensions, with this curved polyptome; and I can now speak from somewhat extensive experience of the perfect facility and safety of its employment.

Sometimes soft and cellular and canaliculated polypi, usually of an elongated form, are met with in practice, which afford no sufficient resistance for a knife to divide them or their peduncles. In such cases, the peduncle of the polypus is, perhaps, most easily severed, by the careful clip of a pair of blunt-pointed curved scissors. In all other forms of the disease, where the tumor was large and pedunculated, I have of late employed the polyptome. In using it, the patient is placed in bed, in the common position on the left side; and generally the whole operation is accomplished so readily and easily, that she is often not aware that more than a common digital examination has been made. I have several times seen some difficulty attend the removal of the amputated polypus itself from the vagina, after its stalk was divided, in consequence of the great size of the tumor; and in order to effect extraction, I have occasionally been obliged to transfix it with the teeth of a large vulsellum. But hitherto I have met with no special difficulty, in at once and easily dividing the peduncles themselves of the polypi, with this polypus knife.

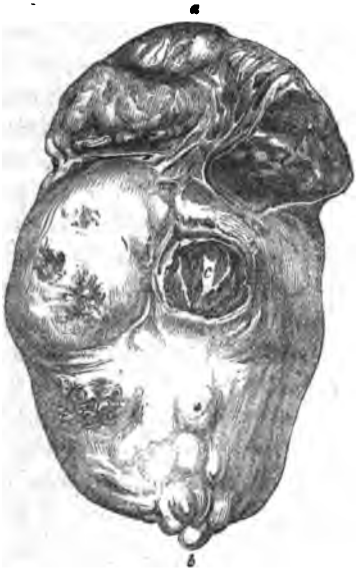
The whole operation is simple and safe, expeditious, and painless, and approaches, perhaps, more than any other in practice, to the Asclepiadean character of the object of the physician: "*ut tuto, ut celeriter, ut jucunde curet.*"

In conclusion, let us compare and contrast, in a few points, the operation of the excision of a common or a large-sized uterine polypus, by the method I have described, and its removal by the ordinary operation of deligation.

I.—*Relative Difficulty of the Excision and Deligation of Uterine Polypi.*

No practitioner can ever be perfectly certain that any large growth detected in the vagina, is a uterine polypus, until his finger touches and traces the peduncle itself of the tumor. And wherever the finger can thus be made to pass and detect the stalk of the polypus, the polyptome may certainly be guided to, and applied so as to divide that stalk. I refer here to cases of considerable difficulty, from the unusual shape or size of the polypus. In such instances, one cannot but conceive it easier to pass upwards a solid curved instrument directly around the mere stalk of the tumor, than to pass a piece of whip-cord or other ligature *behind* and *over* the whole body and mass of the polypus itself, till, in being retracted, it comes indirectly and ultimately to embrace the stalk. For example: in Fig. 12, there is a sketch of a large uterine poly-

Fig. 12.



pus, which I some time ago amputated with the polyptome. The figure represents the polypus diminished two-thirds—*a*, marking the upper, and *b*, the inferior extremity of the polypus, while *c*, denotes the site and thickness of the peduncle of the tumor, as divided by the polyptome. In this instance, the polypus is of an elongated form, its peduncle being attached to its middle, and not to its upper extremity. The tumor had evidently grown into this form after being expelled from the uterus into the vagina. It had developed upwards towards the roof of the vagina, as much, or more than downwards. In this case, the peduncle of the tumor was readily caught and divided by the polyptome; but it would evidently have been a matter of great difficulty to have passed a ligature over the back and top of such a polypus, so as to embrace with it the peduncle from above.

When, however, a polypus is smaller, round, or oblong, and its peduncle is attached to its upper part, there is not more difficulty in applying the ligature, than in applying the knife to the stalk of it. Some, however, of the practitioners who have had most experience with the ligature, confess to the occasional difficulty of its application, with even the best kind of canula. "By practice and dexterity," says Dr. Burns, "this instrument (the double fixed canula) may doubtless be adequate to the object in view, but without these requisites, the operator will be foiled—the ligature twisting, or going past the tumor; every attempt giving much uneasiness to the patient, and not unfrequently, after many trials and much irritation the patient is left exhausted with fatigue, vexation, and loss of blood." This is very likely to happen if the polypus be so large as to fill the vagina. "Dr. Hunter," adds Dr. Burns, "after repeated trials failed in a case, where the polypus filled the vagina; the pedicle in the preparation is long, and as thick as the finger."[1] The application of a ligature to a large uterine polypus is, "in many cases," Dr. Hamilton[2] testifies, "one of the most difficult and dangerous operations in surgery;" and he tells us that "he has seen some of the most eminent practical surgeons of this part of the kingdom foiled in their endeavors to apply the ligature."

I quote, in preference, such opinions from the writings of Professors Burns and Hamilton, because both of these gentlemen were strongly in favor of the operation of deligation.

II.—*Relative Duration of the Operation of Deligation and Excision.*

The process of excision is generally accomplished in the course of two or three minutes at most; sometimes in a shorter period.

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[1] Burns's Principles of Midwifery, p. 130. [2] Hamilton's Practical Observations, p. 40.

On the contrary, the deligation of a uterine polypus consists of a succession of operations rather than of one; and is usually protracted through a period, varying from two or three days to two or three weeks. The application itself of the ligature and canula, in the first instance, requires as much, or indeed more, time and pains than the act of excision. But, after its first application, the ligature requires to be tightened and adjusted from time to time. "Twice a day," as Dr. Gooch directs, "the ligature is to be untwisted from the shoulder of the canula, drawn tighter, and then fixed again round the projecting part; and this is to be done morning and night." "Every day," observes Sir Charles Clarke, another advocate, like Dr. Gooch, for this mode of treatment—"Every day the practitioner is to examine the state of the ligature, and as often as it is found to be at all slack, it is to be tightened. The mode of tightening it," he continues, "requires particular attention. If the canula should happen to be long, the practitioner should not hold the end of it whilst he tightens the ligature, lest with the force used the ligature should cut through the neck of the tumor, and the other extremity of the canula should be suddenly and forcibly pushed against the internal parts of the woman. The time," he adds, "at which the ligature will come away will depend upon the thickness and firmness of the neck of the tumor, and the tightness with which the ligature is at first applied. The neck of the tumor sometimes is cut through in four days, sometimes ten or twelve days will elapse between the application of the ligature and the removal of the tumor, and occasionally the separation of the tumor will take up three weeks; but this is an uncommon occurrence."[1] "After an interval," observes Dr. Churchill, "varying from six days to three weeks, the canula will be found loose in the vagina, and the stalk of the polypus severed."[2]

III.—*Relative Care and Management after the two Operations.*

After the operation of excision, the only special treatment in general required is the introduction of a sufficient plug, of sponge or other soft material, into the vagina, to prevent the chance of bleeding; and the withdrawal of this plug after ten or twenty hours. After, however, the application of the ligature in deligation of a uterine polypus, a considerable amount of continuous care and caution is necessary up to the time at which the pedicle is ultimately divided. "The patient is," says Sir Charles Clarke, "to be desired to remain constantly upon her side, and should not be allowed to move from one side to another unless when the practitioner is present. For want of attention to this caution, there is," he adds, "reason to believe that the canula has been inadvertently pressed against, and its extremity pushed through the uterus of the patient, so as to occasion her death."[1] "The woman," as Dr. Ramsbotham states, "will be obliged to keep her bed during the sloughing process; and she ought to be cautioned, upon attending to her natural calls, to beware of any accidental occurrence which might push the point of the instruments against the internal surface of the uterus."[2] "As the instrument," Dr. Gooch remarks, "projects out of the vagina, if the patient was, whilst turning from side to side, to sit down upon it, she might impale herself on it—an accident which I have heard once took place, and terminated fatally."[3]

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[1] Observations on the Diseases of Females, p. 263.

[2] On the Diseases of Females, p. 220.

IV.—*Relative Chance of Local Irritation of the Vagina and Uterus.*

Local lesion and irritation of the vagina and cervix uteri are not liable to follow upon the practice of excision, unless some local injury has resulted in the operation from very incautious manipulation. But in addition to this danger, there is after deligation, other sources of local disease in the sloughing and putrefaction of the polypus before its complete separation; in the presence of the very fetid and excoriating fluid with which the surface of the vagina and vulva is in consequence constantly bathed; and in the irritation by the ligature itself, as a foreign body, upon the constricted and ulcerating stalk of the tumor—not to speak of the constant application to this ulcerated surface of the foul and acrid discharges that issue from the dead and decomposing polypoid structure. The polypus usually swells after the first application of the ligature. "On account," observes Chelius, "of the increasing bulk of the polyp, it is generally necessary for the first few days after deligation, to empty the bladder with the catheter, and the rectum by clysters. The symptoms," he further states, "which may occur after the tie has been made, are violent inflammation and fever, pain, spasms, bleeding, and other symptoms, from the pressure of the swelling polyp. To prevent," he adds, "the effect of the stinking ichor, repeated injections of decoctions of aromatic herbs must be employed."[4] After the ligature is applied, "When putrefaction has commenced, the discharge from the vagina," to quote the words of Dr. Ramsbotham, "becomes fetid and *highly* offensive. . . It is, indeed, the best sign we can observe, as it proves that decay is going on, that the stem is sufficiently compressed to strangulate the vessels which nourished the diseased growth. If ever," he adds, "such a discharge did *not* take place in a day or two, I should be suspicious that the operation would not succeed."[1]

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[1] Observations, &c., p. 262.

[2] Dr. John Ramsbotham's Practical Observations, vol. ii. p. 468.

[3] On some of the most important Diseases peculiar to Women, p. 264.

[4] System of Surgery, South's Edition, vol. ii. p. 752.

V.—*Relative Danger of the two Operations to the Health and Life of the Patient.*

Those authors who have written in favor of deligation usually quote one solitary case of death from hemorrhage after excision, recorded by Zacutus Luzitanus, in the seventeenth century. It was an instance of the fact that the amount of attendant hemorrhage is not regulated by the mere size of the polypus; for in the case in question, it is stated that the amputated polypus was not larger than an almond.[2] In this instance the operation was performed by an empiric, and no plug or other means for arresting the hemorrhage appear to have been employed. The patient died, not so much from the operation, as from neglect of all proper means to restrain the hemorrhage resulting from it. At the same time let me remark, in passing, that the operation of deligation itself is not free from the risk of hemorrhage, both from the abrasion of the surface of the tumor in working with the canula and ligature, and from the division of the vessels of the stalk, as they are cut through in the process of deligation. "I think," maintains Dr. Meigs of Philadelphia, "the ligature is to be preferred to all other modes of extirpation. It is not in every case to be effected without hemorrhage. I know," he adds, "of two cases here in which the hemorrhage was *terrible.*"[3]

But the principal danger to health and life in this, as after other surgical operations, is the danger of phlebitis and surgical fever. Is such a consequence more liable to follow upon the instantaneous resection of the peduncle of a polypus, and the subsequent immediate removal of the amputated polypus itself—or is it more likely to supervene upon the slow process of disjunctive ulceration being set up in the stalk of the polypus by the ligature, while the gangrenous and putrefying polypus itself is left decomposing in the cavity of the vagina?

I believe that no physician or surgeon acquainted with modern pathology will have any difficulty in answering, that the danger of phlebitis is much greater under the latter circumstances than under the former. The recorded experience of some of those who have written in favor of the ligature, shows strongly enough the occasional liability under deligation to the occurrence of irritative fever and internal inflammations, from phlebitis and the absorption of putrid and purulent matter from the vagina. Dr. Hamilton[1] mentions three cases of death which he had seen follow the removal of uterine polypi by ligature, "On a close inquiry," observes Mr. Arnott, "I find that even those who use it (the ligature) acknowledge that occasionally cases have been met with, where the ligature, in cutting its way through, has excited irritation and fever, and even death. Two cases have been described to me by the practitioners concerned where this occurred, and in casually referring to the interesting works of Bòivin and Duges I find two similar ones."[2] In his lectures on fibrous tumors of the uterus, Depuytren[3] states, "I possess eight or ten observations of women who have perished, from veritable poisoning and absorption of pus, after the application of the ligature for uterine polypus."

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[1] London Medical Gazette for 1835, p, 435.

[2] Praxis Medica, lib. ii. Obs. 86.

[3] Meigs's Females and their Diseases, p. 255. See also Examples in Colombat de l'Isere's Traité des Maladies des Femmes, p. 817.

I have myself seen a woman die with a ligature still fixed around the partially divided neck of a uterine polypus;[4] and other cases where severe but not fatal attacks of phlegmasia dolens and phlebitis followed deligation. Twelve or thirteen years ago, on a patient of Dr. Edgar's of Berwick, I applied a silver wire ligature to the neck of a large polypus and tightened it from time to time, according to the usual rules. In the course of a few days the polypus was dead and putrefying; there was much heat and irritation in the vagina; and the patient's pulse became rapid under the irritative fever that followed. On strongly tightening the ligature to expedite as much as possible the total amputation of the polypus, the wire broke: and the canula and wire slipped off. I immediately proceeded to remove the polypus by excision instead of making any renewed attempt at deligation; and the result to me was very striking and satisfactory. Within twenty-four hours the local irritation had greatly subsided, and the constitutional disturbance entirely disappeared. From that time to this I have operated on many uterine polypi, but never again by the process of slow deligation. And the more that I have seen of the practice of removing large pediculated uterine polypi by excision, the more deeply has the conviction grown upon my mind, that this method is very superior to the usual method followed in this country, of the removal of them by canula and ligature.

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[1] Hamilton's Pract. Observ. p. 37.

[2] Arnott in Lond. Med. Gazette, 1836, p. 412. See also notices of two other cases of death from uterine phlebitis after deligation, in Cyclopedia of Practical Medicine, vol. iv. p. 393.

[3] Leçons Orales.—Brussels ed. 1826, p. 237.

[4] A short notice of this case, which he had seen in consultation with Dr. Girdwood of Falkirk, was communicated to the Obstetric Society of Edinburgh by Dr. Simpson, at its meeting of February 10, 1847, and appears in the Edinburgh Monthly Journal for April of that year, p. 797.—(Ed.)

ON AMPUTATION OF THE NECK OF THE WOMB.

No. I.

CASE FOLLOWED BY PREGNANCY, WITH REMARKS ON THE PATHOLOGY AND RADICAL TREATMENT OF THE CAULIFLOWER EXCRESCENCE FROM THE OS UTERI.[1]

(From Edinburgh Medical and Surgical Journal, January, 1841, p. 154.)

In his learned work on the Diseases of Females, Dr. Churchill remarks,[2] "I am not aware that any attempts have been made in Great Britain to excise the cervix uteri."

The following instance of this operation may therefore not be uninteresting, either as regards its details, or the hitherto flattering success that has resulted from it:

In the beginning of May last I was requested by Dr. Lewins of Leith to visit with him Mrs. Cameron, who, as he informed me, had a tumor attached to the cervix uteri.

The patient, aged 33, had been married for thirteen years. During that period she had borne five living children, and suffered from a miscarriage at the sixth month. In June, 1838, she weaned her youngest child. For about a month previously to that date she had had a red discharge from the vagina, which was constant in its occurrence, though not great in its quantity. It continued during the autumn. In October she passed, with labor pains of three or four hours' duration, a body which the midwife in attendance supposed to be an abortion of the second month. During the period of pregnancy with this alleged abortion, the vaginal discharge was still present. It increased considerably after October, and was now often mixed with coagula of blood. It had always a very offensive smell, and more or less of a red tint, but sometimes it appeared comparatively pale and watery. The discharge was as profuse, though less discolored, during the night, and when at rest, as during the day, and when taking free exercise. From the supposed period of abortion in October, up to the period that I saw her with Dr. Lewins, in May, three or four cloths were soaked regularly every twenty-four hours by it. Whenever she ventured to walk about without napkins she felt the discharge "running," to use her own expression from her. On two separate occasions the escape of pure blood became suddenly so great as to pass through all the cloths, and create great alarm. Mrs. C. was not aware of any causes which excited these attacks of hemorrhage. One of them occurred during the night. She never observed any monthly increase in the discharge answering to the catamenial periods.

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[1] Read before the Medico-Chirurgical Society of Edinburgh, November, 1840.

[2] Churchill, Diseases of Females.—Dublin, 1838, p. 249.

During the whole course of the disease Mrs. C. had not suffered, if we except the temporary expulsive uterine action in October, any pain or uneasiness whatever in the region of the uterus; but by the time that I first saw her she had become greatly weakened and reduced by the abundant discharges. Her face was pale and anæmic, and she was occasionally obliged to keep her bed, in consequence of debility and exhaustion.

Dr. Lewins was first called in to see the patient a short time previously to my visiting her along with him. On examination per vaginam I found, as Dr. Lewins had described to me, a tumor fixed to the posterior lip of the uterus. It was then about the size of a small pear, and was attached by a very broad base. The surface of the tumor felt somewhat rugged and granulated. It was firm but not hard in its consistence. The patient did not complain of any pain upon touching or pressing its surface. Its superficial vessels bled freely under every attempt at examination. On introducing the speculum, and embracing the diseased mass within the further extremity of the instrument, the surface of the tumor was seen to be irregular, and of a bright red strawberry color.

It appeared possible to grasp the basis of the tumor with a ligature; but both Dr. Lewins and I were of opinion that the free amputation of the cervix uteri, with the diseased structure attached to it, offered by far the most probable means of success. We communicated this opinion to the patient's husband, and at the same time stated, that, even under this method of treatment, the disease would probably recur. After a delay of about three weeks, Mrs. C. announced that she was ready to submit to the operation that we had proposed. In making a re-examination after that short interval, I was perfectly convinced that the excrescence had grown considerably, and was extended in its base so as to involve more of the angles of the os uteri, as well as of its posterior lip.

On the 25th May, I proceeded to excise the cervix uteri, and was assisted in the operation by Dr. Lewins and Mr. Ziegler.

The patient was laid upon her face, her body placed across the bed, and her lower extremities allowed to hang over the front of it.

The thighs were held separate from one another. My object was to pull down the diseased neck of the uterus till it protruded externally beyond the mouth of the vagina, and then freely excise it. For this purpose I introduced the two first fingers of my left hand into the vaginal canal up as far as the tumor, and used them as a guide by which I fixed the teeth of a long vulsellum into the sides of the excrescence. Its tissue, however, was so soft as to tear under slight traction, and thus afford me little purchase for pulling the mass downwards. The instrument was refixed nearer the root of the excrescence, and a second vulsellum was superadded to render the purchase the more secure. With these I was enabled to pull down the tumor gradually and cautiously until it was entirely protruded beyond the external parts. Dr. Lewins and Mr. Ziegler having satisfied themselves that the cervix uteri and whole bulk of the tumor was extruded, I cut off the protruded mass, dividing it from behind forwards, and removing the whole vaginal portion of the cervix uteri. The uterus immediately slipped up into its natural position. Very little hemorrhage followed. I stuffed, however, the vagina pretty firmly, under the fear that dangerous bleeding might supervene.

The patient bore the operation well, and complained wonderfully little during it. In the evening Dr. Lewins had to remove the vaginal plug, in order to allow her to evacuate the bladder. It was not considered necessary to replace it. No marked morbid symptoms whatever, either local or constitutional, followed. The great vaginal discharge immediately ceased. On being interrogated in relation to this point on the second day, the patient emphatically described herself to Dr. Lewins as "*quite dry*," and that for the first time for many months. The incised surface, when examined through the speculum a few days after the operation, presented a healthy granulating appearance. It was not considered advisable to allow her to sit up till the tenth day after the operation was performed, and in a few days more she began to walk about the house and perform her usual domestic duties.

She has not been one hour sick since the period of the operation, and has now regained her usual strength and spirits.

No morbid discharge from the vagina of any kind has hitherto appeared. She has never since menstruated; and about five weeks ago she fancied that she felt the symptoms of quickening. On examining the abdomen to-day, 14th November, with the stethoscope, I heard distinctly both the placental souffle and the sounds of the fœtal heart. The os uteri is closed, and on examination by the finger, gives the sensation of a firm puckered cicatrix.

The excrescence after its removal was found to measure two inches and three-quarters at its broadest part, and two inches and a quarter at its greatest depth. The thickness of it where it implicated the posterior lip of the os uteri was one and three-eighths of an inch, but on either side it stretched forward and involved the angle between the anterior and posterior lips; thus rendering this admeasurement greater on its lateral parts. The anterior lip of the os uteri (Fig. 13), which was fully removed as high as the reflection of the vagina, seemed sound except at the above angles. The posterior surface (Fig. 14) of the posterior lip was densely and completely

Fig. 13.

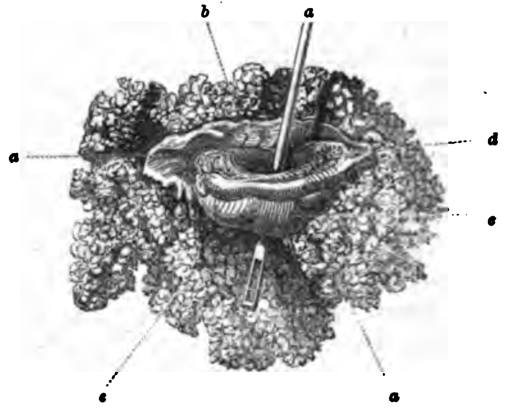


Fig. 14.

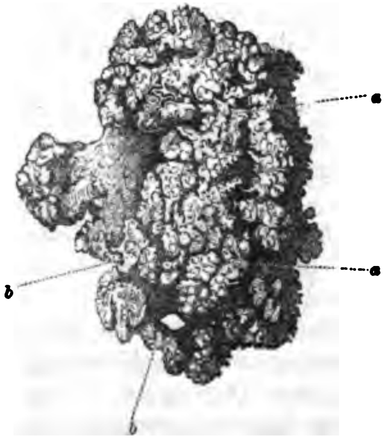
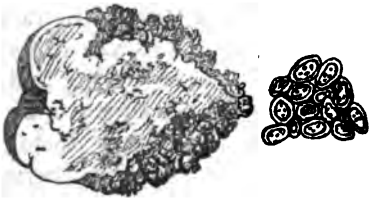


Fig. 13. *a a*, probe passed through the cavity of the os and cervix uteri; *b*, anterior lip of the uterus; *c*, posterior lip; *d d*, line of incision by which the cervix uteri was removed; *e*, rough surface of the tumor attached to the posterior lip.

Fig. 14. *a a*, Under surface of the tumor; *b b*, portions lacerated by the vulsellum.

covered by the excrescence, up to the reflection upon the vagina. In excising the diseased part, I removed it so high as to bring away all around, a small portion of the reflection itself of the mucous membrane of the vagina. The surface of this portion of membrane, as thus removed in attachment to the upper edge of the excrescence, appeared quite healthy on careful examination of the excised mass —(Fig. 14). The surface of the tumor presented a well-marked small granulated appearance, with deeper fissures crossing it, and giving it an irregular and lobulated appearance. The sides of it were considerably and deeply lacerated in various places by the teeth of the vulsellum. On rubbing down any small part of the recent tumor between the finger and thumb, a kind of vascular or cellular framework was all that was left behind. The mass, before dividing it, was steeped in a strong alcoholic solution of corrosive sublimate, in order to insure its preservation. On making a section of the tumor after it had been thus sufficiently indurated, it presented to the touch and sight an appearance greatly resembling that of the brain when hardened by the same menstruum. A number of minute cells were scattered over the surface of the section. On slightly rubbing any part of the section (Fig. 15), but particularly the more external part of it, with the handle of the scalpel, its apparently homogeneous structure at once broke up, and resolved itself into an immense number of very small, connected, grape-like granules. These same granules imparted to the external surface of the excrescence its peculiar minutely mammilated structure; while their arrangement into nodules, in consequence of the divided and lobulated arrangement of the superficies of the tumor, gave to the whole a striking resemblance to the head of the cauliflower. The accompanying wood-cuts, from drawings of the tumor by my friend Dr. Paterson, give excellent representations of its external form.

Fig. 15.



On submitting some very thin slices from the surface of the section of the tumor, to a powerful microscope in the possession of Dr. Reid, it was seen to be composed of a number of cells, arranged in some places in groups, in others in irregular lines. These cells contained each a large nucleus, and this nucleus enclosed several small nucleoli. The structure in question, of cells or cytoblasts, encasing nuclei and nucleoli, has been shown to be so common as an elementary form of natural structure, by Schleiden and Schwann, and as an elementary form of various morbid tissues, by Valentin, Gluge, and Müller, that no conclusion, in the present state of our knowledge, can be positively drawn from this microscopic structure alone. But it may be interesting to add, that none of the caudate or spindle-shaped bodies, described by Müller as often existing in morbid encephaloid structures, were seen in any section that was examined.[1] The microscopic appearance of the compound cell-globules constituting the granules, and composing the mass of the excrescence, are well represented in the wood-cut from a drawing kindly made for me by Mr. Goodsir.

Pathological Nature of Cauliflower Excrescence.—The history, symptoms, physical characters, and minute structure, of the preceding tumor appear to refer it indubitably to that species of growth which was first accurately distinguished and described by Dr. Clarke, under the quaint but expressive name of the "Cauliflower Excrescence from the os uteri."[2]

The pathological nature of this variety of morbid growth has given rise to a considerable difference of opinion among physicians. Drs. Gooch, Hooper, Davis, and Lee, regard it as truly cancerous in its character. Others, as Drs. Clarke, Burns, and Waller, consider it as a morbid tissue, not necessarily of a malignant or carcinomatous nature. A number of circumstances appear to me to show, that in reference to, at least, the first stage of cauliflower excrescence, the opinion of these latter authors is probably correct. The occurrence of the disease in some cases as early as the twentieth year of life;[3]—its occasional shrinking and almost total disappearance upon the application of a ligature, or after death;[4]—the frequent slowness of its general progress during life; the apparent absence of diseased deposits in the neighboring tissues and parts upon the dead body;[5]—and, above all, the alleged restriction and even complete removal of the tumor, in one or two instances, by the use of astringent applications and other simple means,[6] form so many circumstances strongly pointing to the opinion that in the earlier part of its progress the tumor cannot be regarded as of a carcinomatous character.

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[1] At the time this paper appeared, the term Epithelial Cancer, as applied to this and other morbid structures, was not generally in use among pathologists; but the diseased structure described is evidently of this type.—(J. Y. S.)

[2] See Transactions of a Society for the Improvement of Medical and Chirurgical Knowledge, vol. iii. p. 321.—1809.

[3] Sir C. M. Clarke on the Diseases of Females, vol. ii. p. 62.

[4] Ibid. pp. 70 and 75.

[5] Ibid. pp. 66 and 70.

[6] Ibid. pp. 105 and 108. A lady, aged 32, had a continued profuse watery discharge mixed occasionally with blood. She was greatly weakened, pale and emaciated. A cauli-

Has it any analogy in its pathological nature and origin—as it certainly has in its physical characters—with the soft warts and condylomata that sometimes form on the mucous membrane of the vulva and entrance of the vagina? These warts and condylomata have the same tendency to regeneration after their imperfect removal, and present to us a striking exception to the general pathological law of the local reproduction of a morbid growth being a sign of its malignancy.

But whatever view we may take of the primary nature of the cauliflower excrescence of the cervix uteri, we have sufficient evidence for believing either that this disease has been often confounded with carcinomatous or medullary fungus from the cervix uteri, from the want of adequate diagnostic marks to distinguish them; or that, though non-malignant in its commencement, the cauliflower excrescence may, like some other local benign growths become the seat of carcinomatous deposit and malignant action, during its progress. Thus it has been found by Gooch[1] and Madame Boivin[2] to return again in a malignant form, after its imperfect removal by the ligature or knife. In an instance mentioned by Dr. Davis,[3] its removal was followed, after the lapse of a considerable period, by its reproduction, and ultimately by carcinomatous ulceration; and in two cases that occurred to Professors D'Outrepont[4] and Siebold,[5] in which large tumors having a cauliflower form were found affixed to the cervix uteri during parturition, the neighboring uterine tissues, as well as the contiguous structures of the bladder and uterus, were found in a carcinomatous state upon the post-mortem dissection. In another case, in which Michaelis[6] excised what he termed a *fungus medullaris* with a cauliflower appearance, from the anterior lip of the uterus during labor, the posterior lip of the organ afterwards degenerated, and cancer of the stomach ultimately supervened.

If these latter cases were not merely more advanced stages of the

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flower mass projected from the surface of the os uteri. Several remedies, with cupping and local astringents, were ordered and assiduously persevered in. After two years, "*no difference* could be felt between the os uteri of the patient and that of a woman in perfect health."—Clarke, p. 107.

[1] On the most important Diseases peculiar to Women, p. 288.

[2] Heming's translation of Boivin and Duges' work, p. 300.

[3] Principles of Obstetric Medicine, vol. ii. p. 744.

[4] Abhandlungen Geburtshulflichen Inhalts, Th. i. p. 276.

[5] Dissertatio sistens casum singularem carcinomatis uteri cum graviditate conjuncti.

[6] Neue Zeitschrift fur Geburtskunde, Bd. iv. S. 176.

cauliflower excrescence, but, as appears to us not improbable, diseases originally and pathologically different from it, though resembling true cauliflower excrescence in its peculiar form and external physical characters, are there any means which might enable us to form a diagnosis between the two affections? The whole subject is one certainly demanding more careful observation and deeper investigation. The nature and characters, both physical and chemical, of the vaginal discharges in these and other maladies of the sexual parts, require to be more accurately examined and discriminated. May the degree of mobility of the cervix uteri serve in any case as a source of diagnosis? "The tendency of cancer," as observed by Muller,[1] "is to interfere with the natural structure of surrounding parts, while those formations which are of a benignant nature leave the neighboring healthy tissues unaltered." In carcinoma of the cervix uteri, we thus generally find, at even a pretty early stage of the disease, that the organ has become more *fixed* and immovable than natural, in consequence of the morbid deposit affecting both the structure of the neck of the organ and the contiguous surrounding tissues. Does the reverse of this hold good with regard to cauliflower excrescence of the cervix uteri?

Radical Treatment of Cauliflower Excrescence.—Different measures have been proposed for the radical removal of cauliflower excrescence from the cervix uteri. The caustic, ligature, and knife have each been employed. With regard to the two former, it seems superfluous to hope that the good results following upon their use can be more than temporary. The base of the diseased structure will in all probability be left. Occasionally both the caustic and the ligature appeared to have produced injury rather than good, by the irritation and increased action that they have excited in the diseased parts.

If any radical operation and cure for cauliflower excrescence be attempted, the excision of the tumor with the whole of the vaginal portion of the cervix uteri, to which it is attached as a base, appears to me to be the only measure which can at all be hoped to insure ultimate success. The disease has no doubt recurred in repeated instances even after this operation. In some of these cases it probably had advanced too far onwards to a carcinomatous character. In others, the failure might be attributable, as confessed by Boivin and Duges, in regard to the cases which they themselves report, to "the tumor being alone removed," and not the cervix uteri also, which forms its seat, and "is always more or less affected."[2] In a few authenticated cases on record, in which complete amputation of the cervix uteri with the attached tumor was performed, the patient was known to have remained free from any symptoms of the disease for several years afterwards. A search through the medical literature of the last twenty years would, in all probability, enable me to adduce several such instances; but it may be sufficient for my present purpose to adduce three cases, of which I have the notes lying before me, and that appear to me, as far as I can judge from the details and expressions of the reporters, to have been probably instances of the same species of tumor that Dr. Clarke originally described.

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[1] On the Nature and Structural Characteristics of Cancer.—West's transl. p. 66.

[2] Heming's Translation, p. 301.

CASE I.—In an instance of what is termed fungous cancer (*cancer fongeux*) by Colombat,[1] that surgeon amputated the cervix uteri on the 2d June, 1830. The wound completely cicatrized, and the patient's health was re-established. She died in April, 1832, of epidemic cholera.

The fungous cancer, Colombat observes in another part of his work,[2] is one of the forms of cancer which is the least liable to return after excision of the parts.

CASE II.—Boivin and Duges mention a case of cauliflower excrescence of more than two inches in diameter, which was attached to the anterior lip of the cervix uteri. It was removed, along with more than six lines of the cervix uteri, in November, 1828. The patient was alive in October, 1832, and is then reported by the above authors as only laboring under some symptom of menorrhagia and dysmenorrhœa at the menstrual periods.[3]

CASE III.—An instance is reported by Duparcque, under the head of "Exuberance de l'Uterus," in which Hervez de Chegoin excised the two lips of the uterus, affected with what the operator terms "a granular strawberry inflammation," and which he alleges has often been confounded with cancer. The discharge and other symptoms of the disease had been present two years previously to the operation. At the date of the report, four years after the excision of the diseased part, the patient remained perfectly well.

In the case of Mrs. C., which I have above reported, I undertook the amputation of the diseased part, with, as has been already said, strong doubts as to its ultimate success. The patient's peace of mind was broken and her constitution so rapidly breaking down under the constant, profuse, and weakening discharges which afflicted her, that she would in all probability have soon sunk under them. Immediately after the operation was performed, these discharges completely ceased, and have never since returned. Her health and strength have been in the meantime restored to her; and she is at the present moment, as I have already shown, advanced beyond the middle period of pregnancy. The morbid characters of the diseased structure that I removed are such, certainly, as to render its future regeneration not at all improbable, but as yet there are no local appearances of its return; and, taking the very worst view of the case, there seems to be no reasonable doubt but that the operation has restored the bodily comfort, and prolonged the life of the patient, if it has not entirely freed her from the risk of a future return of the disease.

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[1] Colombat de l'Isere, Traité des Maladies des Femmes, tom. ii. p. 701.

[2] Ibid. p. 711.

[3] See Heming's Translation of Boivin and Duges' Treatise on Diseases of the Uterus, pp. 300–301, and drawings of the excrescence in the Atlas, pl. xxiv. figs. 3 and 4.

EXCISION OF THE CERVIX UTERI.[1]

No. II.

WITH REMARKS UPON THE OPERATION IN CARCINOMATOUS DISEASE.

(From Dublin Quarterly Journal, November, 1846, p. 352.)

In eight instances I have had occasion to perform excision of the cervix uteri. In three of these eight cases the operation was had recourse to for the removal of excrescences or morbid structures, possessed of a carcinomatous tendency and character. In the following remarks I propose to describe the more leading facts connected with these three cases, and the results of the operation adopted for their treatment. I shall append a few observations on the method I have followed in performing the operation, and on the cases of uterine disease that appear to be most adapted for its employment.

Case I.—Between four and five years ago, I laid before the Medico-Chirurgical Society of Edinburgh some details of this case, and had an opportunity of showing them the characters of the excised structures, while still in a recent state.[2]

The patient, aged 33, weaned her fifth child in June, 1839. For about a month previous to that date, and during several months subsequently, there was a constant slight menorrhagic discharge present. She aborted in October, and afterwards the reddish vaginal discharge increased, was often mixed with coagula of blood, and had an offensive smell. At times it lost so much of its red tint as to appear comparatively pale and watery. For some months before I saw the patient, the discharge was so profuse as to require the daily use of several napkins. Twice there occurred alarming hemorrhage without any obvious exciting cause. There was no local uterine pain or uneasiness. By the time I first visited the patient with Dr. Lewins in May, 1840, eleven months after the discharge first appeared, she had become greatly weakened and reduced. Her face was pale and anæmic, and she was occasionally obliged to keep her bed, in consequence of debility and exhaustion.

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[1] Read before the Edinburgh Medico-Chirurgical Society, December 3, 1845.

[2] See ante, p. 157.

On making a vaginal examination, Dr. Lewins and I found a tumor, the size of a small pear, attached to the whole posterior lip of the os uteri; its basis of attachment was very broad; its surface was of a strawberry color, rough, granulated, and fissured; it was insensible to touch; but the superficial vessels upon it bled freely under slight pressure or abrasion with the finger or speculum.

On the 25th May, 1840, I excised the whole vaginal portion of the cervix uteri, with the tumor attached to it. In order to secure its complete removal, and insure that my incision was, if practicable, through healthy tissue, and above the seat of the morbid degeneration, I divided the cervix as high up as the reflection of the vagina would permit, and even removed at one point a line or two of the reflected mucous membrane of that part. The excrescence measured 2¾ inches at its broadest part, and 2¼ in its greatest depth (Figs. 13 and 14). After the tumor was steeped in a strong alcoholic solution of corrosive sublimate, its section presented to the touch and sight an appearance greatly resembling that of brain hardened by the same menstruum. It was microscopically examined by Mr. Goodsir, and found to present a nucleated cellular structure, but no condyloid or spindle-shaped bodies were observed in it.

The patient recovered rapidly from the operation. The morbid discharge, from which she suffered so much, ceased from the date of the removal of the tumor; and, when I last reported the case, in November, 1840, she was advanced several months in pregnancy.

My most sanguine expectations regarding the case have been more than realized by its subsequent history. Since the date of the operation, May 25, 1840, the patient has been three times pregnant, and has given birth to three children, all of them now alive and well, viz., the first, born on the 14th of February, 1841,[1] the second on the 18th of May, 1843, and the third on the 19th of April, 1845.

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[1] See an account of the first labor after the excision of the cervix uteri, with some interesting remarks upon the operation, by Dr. Lewins, Jun., in the Edinburgh Medical and Surgical Journal, Jan. 1841. The os uteri was very rigid, and dilated with difficulty. Her two subsequent labors have been much easier.

When I lately saw her, 10th August, 1846, still nursing this third child, now nearly fifteen months old, she declared to me that she never had enjoyed, in all respects, better health than at present.[1]

On examination, I found the os uteri presenting an indentation or fossa resembling the fossa of the umbilicus, instead of the usual nipple-like projection of the cervix.

CASE II.—In the autumn of 1843, I was called to Kincardine, Clackmannanshire, to see this case. It was under the care of Dr. Wilson, now of Edinburgh, and who was then practising at Kincardine. The patient, aged 40, had been eight years a widow. She had been previously married eleven years, and born five living children. After suffering for some time under considerable leucorrhœa and pain in the back, the symptoms increased much in intensity about the beginning of August, 1843, six or seven weeks before I first saw her. The discharge then assumed a more watery character, and imparted an excoriating, burning feeling to the parts as it passed. It was augmented very greatly in quantity, especially in the erect posture, and was generally mixed with blood under the slight straining efforts required to empty the bladder and rectum. After this watery "boiling discharge," as the patient herself termed it, had lasted profusely for about a fortnight, severe hemorrhage came on. It was kept under imperfect restraint by the supine posture, &c., for the next few days, when, at the return of the normal catamenial period, great flooding supervened, and in despite of the use of cold astringents, the plug, and other active and appropriate measures, much blood was lost. The patient, though naturally a very strong and robust woman, was, in consequence, soon reduced to such an extreme state of anæmic weakness and exhaustion, that she required to be lifted with sheets when they ventured, from time to time, to get her bed made dry, and she became sick and faint whenever her head was attempted to be raised. I saw her at this time, with Dr. Wilson and other practitioners of the neighborhood. On cautiously removing the vaginal plug, I found the posterior lip of the os uteri enlarged, indurated, and roughened, and the surface of it and of the anterior lip the seat of ulceration of an apparently malignant kind. The base, however, of the cervix appeared so far free from disease as to allow the possibility of the excision of the parts above the line of the morbid structure. Two days afterwards, viz., on the 13th of September, I accordingly excised the whole cervix uteri. Dr. Wilson, Dr. Girdwood of London, Dr. Adamson, and Mr. Crawford, were present.

The excised mass measured about an inch and a half in diameter at its base, and the incision through the cervix uteri passed, apparently at all points, through a healthy structure. In other words, the line of incision was above the seat of disease, and, consequently, the whole diseased tissue seemed removed. The posterior lip of the cervix was enlarged, in the form of a tumor, to the size of a pigeon's egg, and roughish and tuberculated upon its surface, as is seen in the wood-cut. The base of the tumor found upon the posterior lip, and some parts of the unenlarged anterior lip, were the seat of ulceration, marked by an acute sharp edge. The diseased structure of the posterior lip slightly passed the angle or commissure of the left side, and partially invaded the anterior lip. Dr. Anderson, Professor of Medicine in the Andersonian University, Glasgow, and an excellent morphological anatomist, kindly examined for me a section taken from the enlarged posterior lip. Its structure was found by Dr. Anderson to present, in a well-marked degree, all the usual microscopic and anatomical characters of Miller's *carcinomata fasciculatum*.

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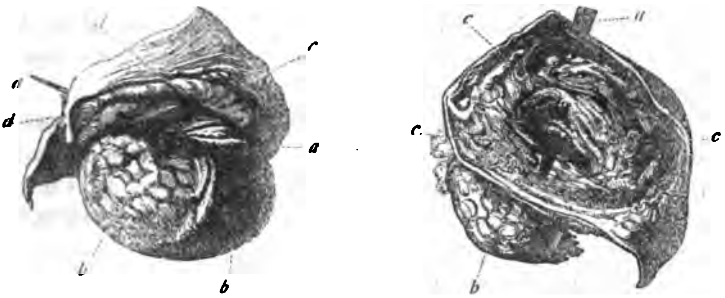
[1] This patient still survives, and has borne more children.—(ED.)

Figs. 16 and 17.



After the excision of the diseased structures, the lumbar pains and local watery and hemorrhagic discharges entirely ceased. Pus was secreted for some days from the surface of the wound. The patient rallied so speedily in health and strength, that within a fortnight she was able to be taken into the garden. For two months subsequently she did not menstruate, but afterwards the catamenial discharge occurred regularly, accompanied with dysmenorrhœal symptoms. A year after the operation I visited my patient, when I happened to be in the neighborhood, and found her in excellent health, busily employed in active and fatiguing duties, and again ruddy and florid in complexion. On examination, the os uteri felt firm and puckered, like the cicatrix of a common stump, upon a small scale.

Figs. 16 and 17. *a a*, Piece of whalebone passed through the cavity of the os and cervix; *b b*, tumor attached to the posterior lip; *c c*, line of incision by which the cervix uteri was removed.

I had a letter from this patient a few months ago, in which she says:

"I am happy to state to you that I enjoy the best of good health. The monthly period is regular and right. I have no pain or uneasiness in the stomach."

In a communication of a later date, 15th August, 1846, the same favorable report is, in all respects, confirmed.

CASE III.—The subject of this case was a patient of Dr. Paterson of Leith. I am principally indebted for the notes of it to Dr. Jackson, who, along with Dr. Paterson, watched over the poor woman with great care and kindness.

The patient, aged 36, had been twice married, and had borne two children to her first husband. She had been married a second time for six years without having any family. During her first marriage she had contracted syphilis from her husband, and it was nearly two years before she entirely recovered from it.

For a long time previously to her seeking medical advice, she had labored under menorrhagia and leucorrhœa. Four months before I first saw her, there was a great increase of the leucorrhœa, and the discharge assumed an offensive smell. After being under treatment for some weeks for this aggravated leucorrhœa, menorrhagia supervened about the middle of June, 1844, recurring to a considerable amount every three or four days. The discharge sometimes changed from a red and clotted appearance to a dark-brown color, and more fluid consistence.

I now quote Dr. Jackson's notes of the case:

"In the beginning of August, on examination per vaginam, I discovered a tumor or excrescence, about the size of a large walnut, attached to the anterior lip of the os uteri. It had a rough, warty, softish feel, and bled on being touched.

"Soon after this the discharge increased, amounting to nearly what we see in severe flooding after delivery. The plug was introduced, and had the effect of arresting it. On the withdrawal of the plug a few days afterwards, a discharge of clear watery fluid commenced, and continued for some days in large quantity. The patient now began to lose color and strength; by and by the pulse sank, and long and protracted faintings took place. At this time, 13th August, 1844, Professor Simpson saw the case, and it was agreed to excise the cervix uteri and attached tumor as soon as arrangements could be made. This was accordingly done on the 16th.

"The patient bore the operation well; simple laxative medicines were given, the vagina frequently washed out with tepid water, and an occasional anodyne exhibited. The only unfavorable symptoms that followed the operation were thirst and vomiting. For a time these were very distressing, but soon disappeared; and subsequently, under the use of tonics, the patient regained her usual health and strength, menstruating regularly, and during the intervals had only a slight whitish discharge at times.

"In the commencement of April in the following year, 1845, eight months after the operation, I again," Dr. Jackson continues, "saw the patient, who informed me she still was well, and continued as when last described.

"This happy state did not last long, for on the end of the same month there was a sudden and violent discharge of blood from the uterus, demanding the immediate use of the plug. This kind of discharge took place occasionally during the next few months; and in the intervals there supervened a discharge of greenish and clear-colored fluid, having an offensive smell. This discharge increased and continued in very great quantity till the day of her death, 24th October, 1845, fourteen months after the operation.

"The amount," Dr. Jackson adds, "of this clear-colored discharge may be fancied, when I state that her linen and lower bed-clothes used to be soaked three or four times a day. Many remedies were tried in vain, and examination per vaginam discovered the cut surface of the neck of the uterus amidst much softening of the neighboring parts, all of which felt covered with a thick mucus.

"During the continuance of the discharge, and about three months before her death, she had a severe attack of anasarca, with a great loathing of food, and vomiting, and complained of dull and obscure pain in the lower part of the abdomen. The œdema, however, yielded to treatment, but again returned a fortnight previous to her death; and she apparently sank ultimately under pure exhaustion from the excess of the discharge."

The excrescence which I removed in the preceding case was of an oval form, and nearly the size of a small peach. It was slightly irregular and lobulated on its inferior or free surface, and on more narrow examination this surface presented a small granulated appearance. The line of incision by which it was removed seemed to pass through healthy tissue. On microscopic examination, Dr. Anderson found the structure of the tumor to present all the characters of Muller's *carcinoma reticulare.*

On inspecting the body of the patient after death, Drs. Paterson and Jackson could not detect any traces of disease in any part except the pelvis. The body and fundus of the uterus were slightly enlarged; and in several places its peritoneal coat was strongly united to the Fallopian tubes, and the neighboring serous surfaces, by old false membranes. The site of the excised cervix uteri, the upper portion of the vagina, and the cellular substance intervening between these points, and the bladder and rectum, were the seat of pultaceous softening and ulceration, but there was little or no thickening or actual morbid deposit in these parts. Dr. Anderson examined microscopically the structure of the uterus at the seat of excision and ulceration, but could discover no decided marks of *carcinoma reticulare*, or other specific form of malignant structure in the tissues of the part. From the softened condition of the affected textures, it was found impossible to ascertain, at the time of the autopsy, whether the degeneration and ulceration extended, or not, through the recto-vaginal septum into the rectum itself.

METHOD OF OPERATING; COMPLICATIONS AND CONSEQUENCES.

In performing the excision of the cervix uteri, in the three instances I have detailed, and in the other cases in which I have operated, I have proceeded on the following plan: I have fixed one or two vulsella into the outer or vaginal side of the diseased cervix, as high as it was possible to insert them, and, by the purchase which they afforded, have gradually and cautiously dragged this part down in the lines respectively of the axis of the pelvic brim, cavity, and outlet, till it appeared so far beyond the vulva as to allow me to cut through the base of the protruding cervix. In one or two cases I used a knife in making the incisions. But in consequence of the powerful retraction under which the cervix uteri is placed during the operation, it is difficult, or indeed impossible, to make the incision in this way so equable and perfect as to remove with certainty all the diseased part. After a partial cut or two the uterus is strongly retracted at the points of incision, and the remainder of the operation requires to be finished with the line of incision thus rendered irregular and confused. A pair of large, curved, blunt-pointed scissors, such as were used in this operation by Osiander and Dupuytren, is in this respect preferable. We are enabled by them to surround and embrace the whole cervix at once; and having cautiously and carefully adjusted their edges to the very points which we wish to divide, and thus calculated, by this preliminary step, the exact limits of the incision, we may then immediately complete the amputation of the part, by one or two strong and rapid strokes of the instrument. The blades must be placed around the cervix, *above* the line of the teeth of the vulsellum; and then our object is, as it were, to cut out the vulsellum along with the whole inferior and diseased parts of the cervix, in which it is fixed. The operation is much facilitated by the labia being strongly pressed aside by broad copper spatulæ.

I have always placed my patients upon the face, the body being situated across the bed, and the lower extremities hanging over it, as in the operation for hemorrhoids. We are thus enabled to make our incision through the cervix uteri from behind forwards, instead of from before backwards—a matter, in my opinion, of no small moment. For if we cut in this latter direction, viz., from *before backwards*, we would sometimes run a greater danger of opening into the peritoneum, which stretches downwards so much more behind than in front of the cervix uteri, and offers a very thin wall of partition between the cavity of the vagina and the cavity of the abdomen. Latterly, I have found the first portion of the operation, namely, the seizure and traction of the cervix uteri, much facilitated by using a very large and strong vulsellum, made with the common loose joint of the obstetric forceps, instead of the usual fixed pivot or scissors joint. With the common scissors-jointed vulsellum, whilst we are intent on fixing the teeth of one blade in a proper situation, the teeth of the other blade are always apt to become entangled in the tumor or walls of the vagina itself, and thus impede and embarrass the operator. But with the modification of the vulsellum that I have alluded to, this difficulty is avoided, for the individual blades are introduced, adjusted, and fixed, separately and successively; and then, afterwards, they are easily united together for further use. Besides, in this way we far more readily effect what are, I believe, the two principal secrets in the operation, viz.: 1st. We fix both blades of the instrument, and more especially that corresponding to the diseased lip, as high upon the cervix, and as near its line of reflection upon the roof of the vagina, as possible; and 2dly, by making our line of incision immediately above the hold of the vulsellum, as if our object were to cut out that instrument and the part which it embraces, we secure this important point, that the incision which we make is more likely, than if we followed any other plan, to pass through a stratum of healthy tissue, as we thus inevitably remove the whole vaginal portion of the cervix uteri, and the diseased structure of which it is the seat. In thus attempting to insert the vulsellum as high as possible in the cervix, we will succeed far better by guiding it directly to the point required by the finger and the sense of touch, than by attempting to direct it by the speculum and the sense of sight. In fact, if the cervix is, as generally happens, at all much increased in size, it is, of necessity, utterly impossible to see, with any speculum, the part in which the teeth of the vulsellum should be fixed—that part lying much higher than the sphere of vision.

Several forms of danger have been found to attend upon excision of the cervix uteri. A fearful variety of nervous depression is alleged by some authors to supervene occasionally upon the operation. I have seen no instance of it. Severe hemorrhage sometimes occurs, but it is much rarer than we might *à priori* expect. In only one case have I met with it in any considerable amount, and in that instance it was readily and effectually restrained by the plug. Out of nineteen private patients operated on by Lisfranc, Paley avers that four died within twenty-four hours. Subsequently there is further danger to the patient from inflammation kindling in some of the uterine structures, or in the peritoneum itself. Out of the eight cases in which I have operated, seven recovered perfectly. The remaining eighth patient recovered so far as to leave her bedroom, but was then, from an unfortunate domestic quarrel, subjected to great mental excitement, after which she relapsed, and died under symptoms resembling those of phlebitis. In his Operative Medicine, Velpeau, after stating that he had himself only operated in two cases, in one of which death occurred in three days, and in the other six weeks after the excision of the cervix, proceeds to remark: "A patient operated upon by M. Blandin died of uterine phlebitis; one of those that Lisfranc lost was carried off by peritonitis; others have sunk under a nervous state, the gravity of which it is not easy to explain. Up to the present time," he continues, "scarcely any one has been seen to die directly of hemorrhage. Rust and Graefe of Berlin, Roux and Dupuytren, who have all seen their patients perish from the immediate results of the operation, do not ascribe the fatal result to this complication (hemorrhage). Excision of the neck of the uterus, although easy and by no means severe, is nevertheless sometimes extremely dangerous and speedily fatal. However, Osiander has practised it twenty-eight times, Dupuytren fifteen to twenty times, Lisfranc forty to fifty times, without its having caused death more than once out of every six or seven operated upon."[1]

CASES ADAPTED FOR THE OPERATION.

Since excision of the cervix uteri is an operation attended with so many chances of danger, as Velpeau exposes in the passage which I have quoted, it evidently follows that it should only be adopted in cases, and under circumstances, in which milder and safer means of cure are insufficient. The forms of disease in which, upon this principle, it seems justifiable to avail ourselves of the aid of this operation, supposing no contra-indication to be present, are, in my opinion, principally—

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[1] Velpeau, Op. Med. vol. ii. p. 268.

1st. Great morbid hypertrophy, by elongation, of the vaginal portion of the cervix uteri. I have operated successfully in two such cases.

2d. Corroding ulcer, when limited to the lips of the cervix, and pathologically identical with the form of lupus or malignant ulcer so well known on the face; and

3d. Circumscribed and local forms of carcinomatous disease or excrescence, of the lips and lower segment of the cervix uteri.

Some continental surgeons, and more especially Lisfranc, advocate the propriety and necessity of excision, in various other cases besides those I have just enumerated. I have myself twice excised the part when affected by chronic induration and thickening, without carcinomatous degeneration; but I would now, most assuredly, by no means resort to it again under the same condition, as I believe that morbid state of the cervix to be quite removable by milder measures. In the case that I have alluded to as having terminated fatally, the structure of the excised cervix presented a great degree of condensation and induration, with two small cystic tumors enclosed in the morbid tissue. The symptoms attendant on this lesion had been of very long standing, and had previously broken down the health of the patient.

Certainly, however, the set of cases in which, of all others, the operation is likely to prove an occasional and important addition to our previous means of treatment, is that in which there exists local carcinoma of the cervix uteri.

Every practitioner knows that, of all uterine diseases, cancer is the one which the female mind most constantly and most justly dreads. A patient scarcely ever suffers for a length of time under any severe affection of the uterus, without her own anxieties and fears magnifying it into an instance of cancer, and investing it with all the horrors pertaining to this most fearful and loathsome malady. And certainly it is a disease which does occur sufficiently often. The female constitution seems much more disposed than the male, to assume the carcinomatous diathesis; and the absolute number of deaths from cancer, as recorded in our own and other mortality bills, is in the proportion of two or three females for every one male that sinks under this specific form of morbid action.[1] Some pathologists besides, as Tanchou, Stern, and others, believe that cancer of the uterus is becoming much more frequent at the present time than it was formerly. Be this the fact or not, there can be no doubt of this other circumstance, that in consequence of the more advanced knowledge of the diagnosis and pathology of uterine diseases, the practitioner has now ample means of separating and distinguishing at once from carcinoma, various forms of uterine affection that were not long ago, generally looked upon as doubtful or decided instances of uterine cancer, and consequently regarded with all the utter hopelessness of despair.

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[1] As a statistical illustration of the truth of this remark, I may adduce the proportion of fatal cases of cancer in the two sexes, reported by the Registrar-General as having occurred

Fibrous tumors of the uterus, for instance, one very common form of structural disease in this organ, were formerly very frequently confounded with cancer, but it is now well known that they have a different anatomical seat, a different pathological course, and a very different morbid termination.

I have known a patient supposed to be laboring under irremediable cancer of the uterus, when the disease was only a severe but remediable form of polypus. The accompanying symptoms are often very much alike in the two diseases even to the character of the discharges. "The fœtor of the discharges in polypus," observes Sir C. Clarke, "induces in the mind of the patient, and sometimes of the practitioner, a belief that the disease is cancer, and this is confirmed by the sickness which generally attends the disease." Sanson and Roche tell us that they have repeatedly, to quote their own words, "seen Dupuytren perform the removal of polypi which various of the most able surgeons of the French capital had mistaken for uterine cancer, and thus render back to life patients condemned to a most certain death by those mistakes of which they had been the subject."

Various other forms of uterine disease are liable to be mistaken for cancer of this organ. In one of the first cases in which I recognized by the uterine bougie the existence of retroflexion of the unimpregnated uterus, the patient had some years previously been doomed by the highest obstetric and pathological authorities in England, as suffering under the first stage of scirrhus uteri—the displaced fundus of the retroverted organ having been mistaken for a

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in England, excluding the metropolis, during the first five full years in which the Registration Act has been in operation.

Mortality from Cancer in England as Regulated by Sex.

Year of Report.	Total fatal Cases of Cancer.	In the Female Sex.	In the Male Sex.
1838,	2304	1717	587
1839,	2549	1924	625
1840,	2238	1656	582
1841,	2215	1692	523
1842,	2356	1757	599
Total,	11.662	8746	2916

carcinomatous tumor. The uterine displacement was easily rectified by the use of a wire pessary worn for some months in the uterine cavity, and the patient is now in the enjoyment of the best of health. I have seen other cases of the same mistake, with this same curious but common form of uterine displacement.

Two varieties of an inflammatory, and hence of a curable form of disease, are certainly not unfrequently confounded with cancer uteri. I have now seen four cases in which patients had been condemned, as afflicted with cancer uteri, and who each had indurated, irregular tumors of the pelvis, formed by effusion of coagulable lymph, or pus, or both, into the cellular tissue situated around the cervix uteri, and forming hard, firm swellings in that locality, in consequence of the inflammatory deposit of which they consisted lying above the dense pelvic fascia of this part. All of these cases have perfectly recovered, and two of the patients have borne children since the attack. The second form of inflammatory disease, which is liable to be mistaken for cancer, consists of inflammatory induration, and often ulceration, of the proper tissues of the cervix uteri. I have now had occasion to see no small number of instances, in which this chronic inflammatory affection of the cervix uteri has been confounded with cancer of the part, and where the non-carcinomatous character of the disease has ultimately been established by the ulceration and induration totally disappearing under appropriate local treatment.[1]

In any case, then, of suspected cancer uteri, and where, as often happens, the local and constitutional symptoms are such as are usually described as accompanying that affection, as local pains, sanguineous and morbid discharges, general cachexia, &c., we may always hope that a careful physical diagnosis will disclose the local disease to be one or other of those more safe and more curable forms of morbid action that I have above adverted to. And let me here add, at the same time, that as a general rule, I believe it utterly impossible to make, with any certainty, such a diagnosis by the mere rational or external symptoms only, such as the nature of the discharges, the degree and character of the attendant pains, &c.; for in practice we constantly meet with uterine cases having all these symptoms well developed, without the local disease being carcinomatous; and, on the other hand, we occasionally meet with cancer of the uterus without these symptoms being present in any very marked or appreciable degree. In this, as in most other uterine affections, "the true character of the disease can *only*," as was many years ago most justly observed by Sir Charles Clarke, "be ascertained by an examination."

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[1] Dr. Valentine Mott, the distinguished American surgeon, in the account which he published at New York, in 1842, of his "Travels in Europe and the East," inadvertently states a fact which to every one conversant with uterine pathology, must abundantly prove, that in most cases in which Lisfranc of Paris performed excision of the cervix uteri, the operation was undertaken for simple *inflammatory* induration and ulceration of the amputated part. For after mentioning Lisfranc in very laudatory terms, Dr. Mott observes, "I am delighted to have it in my power to say, that in one of my visits, by express invitation, to examine a great many cases of a peculiar and distressing malady of the female sex, for which he had performed in previous years more than sixty operations—exsection of the neck of the uterus—he now stated to me that he readily effected a cure by a much more simple and less painful process; a fact highly honorable, I consider, to his humanity, and denoting clearly the advancing march of surgical science. *His remedy*," Dr. Mott simply adds, "*is merely the application of lunar caustic to the part affected.*"—Travels, &c., p. 38.

But the question again recurs to us, supposing that we do discover, by a proper physical diagnosis, that true carcinoma of the uterus is present, is the case consequently to be looked upon as always utterly hopeless?

I believe that in forty-nine instances out of every fifty in which we find the uterus or any part of it the seat of true carcinomatous deposit, the disease inevitably leads, sooner or later, to a fatal termination. The rapidity of its march is various, and may deceive an incautious observer by its duration. I have known death occur a few weeks after the disease first attracted the special attention of the patient; and I have had occasion to watch the course of a case, where the patient dragged on a miserable existence for seven years after the first discovery of the malady by the late Dr. Hamilton. But whilst thus fatal in almost every instance, there are still, as we have already seen, some rare varieties or forms of carcinoma uteri, that are apparently within the just range of surgical treatment. And *one* condition favoring this is the generally admitted fact, that the disease almost always begins in, and for a time is limited to, the structure of the lips and cervix of the uterus. Professor Rokitansky of Vienna, perhaps the most profound and experienced morbid anatomist of the present day, in speaking of the first locality and origin of cancer of the uterus, observes—"Carcinomatous induration generally limits itself to the vaginal portion and cervix, and very often in a refined and sharp manner (*mit einer sehr bezeichnenden Weise und schärfe*)." In another paragraph he remarks—"The primitive seat of cancer is always the cervix uteri, and first of all and particularly the vaginal portion. The primary appearance of cancer in the fundus uteri is limited to so extremely rare cases, that what we have just said remains one of the most fixed rules (*eine der ausgemachtesten Regeln.*") "It forms," he adds, "in this respect a contrast with fibrous and tuberculous tumors of the uterus, a contrast which also holds with regard to its involving the parts around, and its ulcerative destruction."[1]

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[1] Handbuch der Pathologischen Anatomie, vol. iii. pp. 551, 552.

In relation to the same question, we must bear in recollection *another* circumstance in the natural history of carcinoma uteri, granted by most of the best pathologists who have written upon this disease. "Uterine cancer," observes Professor Walshe, one of the latest and most learned writers on the subject, "is commonly primary, and possessed of comparatively slight tendency to contaminate the system generally." And again—"There can be no question that the womb ranks among those organs less prone than certain others, as, for instance, the mammæ and testes, to contaminate distant viscera. Among thirty-seven females, cut off by uterine cancer, and examined by M. Ferrus, seven only exhibited secondary formations elsewhere."[1]

In these two important respects, therefore, uterine carcinoma presents conditions favorable for surgical interference. Still, however, in order that a case may offer any chance of operative success, several conditions seem requisite:

1st. The disease must be in an early stage.

2d. The morbid structure must be strictly limited to the lip or lips of the cervix, or at all events be *distinctly* situated below the line of reflection of the vagina upon the cervix uteri.

In actual practice, however, it rarely indeed happens that the above combination of circumstances is met with, because, in truth, the medical attendant is very seldom called in till the disease is so far advanced as to have passed the limits in question. In fact cancer uteri generally proceeds throughout the first stage of deposition and induration with such slow and stealthy steps, that the attention of the patient is not awakened to its presence by any particular local symptoms; and it is commonly not till the malady is advancing or has advanced towards its higher states of morbid development and disintegration, that a sudden and unaccountable loss of blood, or the unexpected appearance of some purulent or sanious discharge, or the supervention of uterine pain, first rouses the lurking suspicions of the sufferer to the nature of the fearful fate that is impending over her.

3d. Future inquiry will in all probability prove that there are some varieties, types, or *species* of carcinoma of the cervix uteri which are much more within the pale of surgical treatment than others. I believe this last to be a most important subject of inquiry; but it is confessedly a department of uterine pathology to which, valuable as its results may be, no laborer has as yet directed his attention.

The three different cases which I have detailed present three different species or forms of carcinoma of this part. Though the materials which these three cases furnish afford a very meagre and imperfect nucleus for such an inquiry as I suggest, still they are neither without interest nor importance; and in this point of view I will here venture, in conclusion, to recapitulate the principal pathological and practical data which they seem to supply.

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[1] On the Nature and Treatment of Cancer.—London, 1846, p. 443.

In the first case which I have described, the excised morbid mass had all the usual characters of cauliflower excrescence, a disease which, in its ultimate course, always takes on malignant action, whatever difference of opinion may exist as to its pathological nature in the incipient stages. The tumor removed from the second patient was an example, as I have already stated, of Muller's *carcinoma fasciculatum;* and that from the third was an equally characteristic specimen of the *carcinoma reticulare* of the same author.

Every pathologist will, I believe, readily grant that these are forms of malignant structure regarding the truly carcinomatous nature of which there can be no rational doubt. All the three patients were extremely sunk and prostrated by the attendant discharges, before I had recourse to the excision of the cervix uteri and the morbid excrescences attached to it. All the three were so far benefited by the operation as to recover their usual health and strength, and be again able for the duties of life. But in the last case the disease recurred after eight months of comparative health, and after fourteen months, it terminated fatally. The other two patients still remain in the enjoyment of perfect health, although in one instance upwards of six, and in the other about three years have elapsed since the period of the operation. And, as I have already stated, the first of the patients has now conceived, borne, and nursed three children since the date of the excision of the diseased parts—a sufficient proof both of the completeness of her own recovery, and of the safety of the operation, so far as regards the primary and most important of the physiological actions of the organ operated upon.

It will be evident that excision of the cervix uteri may be resorted to, with the greatest hope of ultimate success in those cases where the morbid hypertrophy or elongation approaches least towards a malignant type. We are acquainted with at least three instances where the operation was, during the last two or three years, successfully performed by Dr. Simpson, on account of such non-malignant hypertrophy. In the Royal Infirmary, just now, is a strong healthy-looking woman, on whom Dr. Simpson excised the cervix uteri some two years ago. The mass amputated, consisted of the elongated and hypertrophied cervix, covered with warty-looking excrescences to such an extent as to protrude like a large tumor, the size of the closed fist, through the vulva. The patient has returned lately to Edinburgh, and is now under treatment for retention of the menstrual secretion, from occlusion of the os uteri, consequent on adhesion of the opposed lips after amputation. Dr. Simpson has made an incision into the cavity of the uterus through the cicatrized cervix, and allowed a quantity of thick retained menstrual fluid to escape.—(ED.)

OCCASIONAL LATENCY OF THE SYMPTOMS IN ADVANCED CARCINOMA UTERI.

(From Edinburgh Monthly Journal of Medical Science, January, 1852, p. 39.)

In the earlier stages of cancer of the uterus, the disease is, as a general rule, accompanied by few, or indeed no, well-marked dynamic symptoms. Patients themselves, and sometimes also the members of the profession, seem to expect that the advent and presence of this fatal malady should be very constantly accompanied with local pain and suffering. The reverse, however, of all this seems to be the general rule. In fact, it rarely happens that a patient affected with uterine cancer applies at all for medical advice till the disease has advanced beyond the stage of deposit, and has already made more or less progress in the stage of ulceration. Even then the local symptoms which excite the patient's attention are usually not the expected pathognomonic pain, but occasional attacks of hemorrhage, attended with leucorrhœal discharge. Or, if the pain is present, it often as yet only amounts to a sensation of discomfort and uneasiness, and not to a feeling of actual suffering. Nay, sometimes any feeling of pain in the uterus or uterine region itself never supervenes at all, or not till the very last period of the affection. In the course of practice, I have happened to see a number of cases to which this remark applies. Instances also occasionally occur where the patient suffers more or less severely from pain; but that symptom is in the form of a sympathetic or reflex pain, situated not in the uterus, but in the limbs, loins, or some other distant part. Several years ago, I had occasion to examine a case in which the cervix uteri was entirely eaten away by extensive cancerous ulceration; but without any marked local pain. The patient, however, had complained so much of pain in the mamma, that local anodyne and other applications had been applied to that part of the body. Dr. Davidson told me the particulars of a case, in which the patient complained to her medical attendant of nothing during life, except a series of severe urinary symptoms, for which she had ineffectually undergone a variety of treatment. On opening her body after death, the coats of the bladder were found deeply implicated in a mass of ulcerated uterine carcinoma. The following case, which I saw within the last few weeks with Dr. Cowan, in a patient who came from a distance in the country, is one of the most striking illustrations which I have met with of the occasional latency of the local symptoms of cancer of the uterus, even in a very advanced and ulcerated stage, and of the transference, as it were, of the principal suffering and symptoms to another organ:

Case.—A lady, æt. 43, married at a very early age, and the mother of six children, had enjoyed the most robust health until twelve months ago. About that period, she first observed a white discharge from the vagina, which she believed to be common leucorrhœa. There likewise occurred repeated discharges of blood; sometimes in large coagulated masses and shreds. At the same time, the catamenia recurred with regularity, and without pain. About three months since, she first complained of such prostration as prevented her taking her usual amount of exercise. Difficulty and pain also in passing water, and latterly incontinence of urine supervened. During all this period, she experienced no feeling of uneasiness referable to the uterus itself; nor were the leucorrhœa or menorrhagia of a nature or extent calculated to excite in the mind of the patient any feelings of alarm. In fact, the principal, and, according to her own account, her almost sole symptoms were the debility already mentioned, and the painful dysuria, which had, however, been relieved by alkalies.

On making a vaginal examination, I found the cervix uteri, with the upper and anterior part of the vagina, the seat of extensive carcinomatous induration and ulceration. The disease in its ulcerative process had in fact proceeded so far at one point that it had implicated and *already perforated* the neck of the bladder—thus leading first to the dysuria, and subsequently to the incontinence of urine, of which the patient so much complained.

In a note from Dr. Cowan, dated December 20th, he states:—"At present our patient's appetite is good; bowels regular. She sleeps well, and the general appearance is improved, rather than otherwise, since you saw her. All she complains of is, general debility, incontinence of urine, with a thin white non-acrid discharge, and occasionally, but not constantly, heat in the region of the uterus, unaccompanied with pain. All other symptoms of extensive uterine disease are absent."

Dr. Cowan has further stated to me, in a recent communication:—"Mrs. C. died ten months after you and I visited her together in Edinburgh. She gradually sank under the extension of the cancerous disease. Some time after you and I saw her, the symptoms were still so slight and trivial, that another medical man whom she saw declared that it was impossible she could have such a serious malady as cancer. She died in Paris, and the French physicians whom she consulted, for a time doubted the correctness of your diagnosis and prognosis, her symptoms looking still so slight. But the subsequent course of the affection showed them the true cancerous nature of her disease."

ON CARCINOMATOUS DISEASE OF THE CAVITY, BODY, AND FUNDUS OF THE UTERUS,

THE CERVIX BEING UNAFFECTED.

MOST pathologists and practitioners have laid it down that the cervix uteri is always, or almost always, the portion of the uterus that is first and principally affected in cases of cancer.

In a preceding page, we have seen Rokitansky stating that cancer of the uterus "always attacks the cervix in the first instance."[1] "Cancer of the uterus," observes Dr. Walshe,[2] "almost invariably originates in the cervix." In describing carcinoma uteri in his work on the Diseases of Females, Sir Charles Clarke remarks, "This disease attacks only in the first instance the cervix of the uterus, and the author," he adds, "lays great stress on this observation."[3] "The cancerous action," according to Dr. Francis Ramsbotham, "first assails the tissues of the os and cervix uteri. I believe," he continues, "this is invariably the case."[4]

Such strong statements are liable to mislead the practitioner, and to cause ever and anon, errors in diagnosis and prognosis. No doubt, the cervix of the uterus is much more frequently the seat of carcinomatous disease than the cavity of the organ, or the tissues of the body and fundus. But I have been myself deceived, and have seen others deceived by the common belief that cancerous affections never originate in the cavity, body, or fundus of the uterus, and without the cervix being primarily or cotemporaneously attacked. In the course of practice, I have seen, on the contrary, a very considerable number of instances in which carcinomatous disease, when affecting the uterus, has primarily sprung up in the cavity of the organ, or in the walls of the fundus or body, and in which the tissues of the cervix have remained sound to the last, or at most been only affected secondarily.

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[1] Manual of Pathological Anatomy, vol. ii. p. 300; or ante, p. 178.

[2] The Nature and Treatment of Cancer, p. 96.

[3] Observations on the Diseases of Females, p. 207.

[4] London Medical Gazette for 1835, p. 466.

Carcinoma, when it attacks the cavity, body, or fundus of the uterus, may appear under different types or forms. The principal varieties of it which I have had occasion to observe in practice are the following:

1. When carcinomatous disease attacks the cavity of the uterus, it sometimes presents the form of an irregular, flat, or roundish fungoid *excrescence*, attached by a broad basis to a greater or less extent of the interior of the organ.

Case I.—The first decided instance of this kind which I had an opportunity of seeing was in an unmarried lady, forty years of age, the sister of a distinguished English physician. For many months she had suffered under a constant and copious discharge of watery fluid from the genital canals, with occasional slight hemorrhage, and gradual emaciation; but there was no local pelvic pain or suffering. She was for some time under the care of an esteemed obstetric practitioner here—a friend of her brother's—and a great variety of applications were employed by him to arrest the profuse serous discharge. These applications had been all made to the surfaces of the vagina and cervix uteri: but without any effect on the copious morbid secretion. When I saw the patient with her physician and relative—as there was apparently no diseased state of the vaginal canal or cervix uteri, I suggested the introduction of a sponge-tent into the os uteri, with a view of shutting up that aperture for a time, and thus ascertaining if the abundant watery secretion did not proceed from the cavity of the uterus itself. As long as the tent remained in the os uteri the discharge was arrested —a phenomenon not observed for many months before; and on withdrawing it there was a copious rush of the characteristic clear fluid. The morbid source of it was thus proved to be some point or points in the interior of the uterine cavity. On opening up the os and cavity of the cervix more fully with sponge-tents, we were able to reach the edge of a rough tuberose excrescence, attached by a broad basis to the interior of apparently a great part of the cavity of the uterus. Small granular portions of it were easily detached by the finger or nail. It seemed to all of us a sessile carcinomatous or cauliflower growth growing in the cavity of the uterus. The discharge continued and increased; and the patient ultimately sank under the usual course of cancer about eighteen months subsequently.

Case II.—A short time afterwards I was sent for to the Highlands to see, a week or two before her death, a patient who had long suffered under the same kind of profuse watery discharge; but in her the local disease was much farther advanced. The os uteri was dilated to the size of a two-shilling piece; the orifice was round, and its lips thin and healthy—exactly like the uterine orifice in the first stage of natural labor. There projected, however, through this opening, not any part of an ovum or fœtus, but a rough irregular mass, granular on its surface, and very friable and lacerable in its structure. It bled profusely when touched. On passing the finger over the edge of the dilated os, and within its interior, the fungating structure protruding from the uterine opening was found springing, as it were, from the interior of the cervix uteri all around at a height varying from a few lines to about an inch. In the centre of the protruding excrescence there was an opening leading up, as shown by the uterine sound, into the cavity of the uterus. The uterus itself was enlarged, and could be felt midway between the pubes and umbilicus. Its whole interior seemed to be filled by an accumulation of this epithelial cancer. This patient was about 33 years of age. She had been long married without having any children.

Since the above period I have seen several cases of the same nature as the above at different stages of their progress. Occasionally, as in the last case I have described, the os uteri was so open as to allow the carcinomatous structure springing up from the interior of the cavity, to be felt at once by the finger. In one or two instances, I have seen the carcinomatous fungus protruding through the os, sloughing and gangrenous from the stricture and compression of the circle of the os upon it. But in most instances the disease has been in an earlier stage; the patient complaining of watery and bloody discharge from the cavity of the uterus; and the true nature of the malady was not ascertained till the canal of the cervix was artificially dilated for the purpose of a more accurate diagnosis by the finger.

In some of these instances of carcinoma affecting the interior of the uterus, the whole bulk of the organ is little, if at all, increased beyond its natural dimensions, and the discharge is bloody rather than serous; but towards the termination of the disease, it begins to present the odor peculiar to cancer, with shreds and fragments of the cancerous tissue passing along with it.

CASE III.—In a patient, aged 50, I saw the disease of the uterus followed by cancerous disease of the mamma. A few years after the regular catamenia had ceased, this lady observed from time to time a slight bloody or sero-sanguineous discharge from the vagina. On examination, the cervix uteri felt normal and healthy, and the whole uterus was of the natural size. After suffering in this way for a couple of years, carcinomatous disease appeared in the right mamma, and she died in the course of a twelvemonth, without, however, the affected mamma ulcerating or fungating. A few weeks before her death, portions of cancerous excrescence protruded from the os uteri; but up to the last the cervix uteri remained unaffected.

2. Occasionally, cancer affects the cavity of the body and fundus of the uterus, in the form of carcinomatous *ulceration*, and without any appearance of excrescence and fungation.

I have seen this form of uterine cancer destroy life without any other complication. But more frequently, I have seen it result apparently as the effect of the long-continued irritation of a pediculated fibroid polypus upon the interior of the uterus.

Case IV.—In a case of long-standing menorrhagia, after dilating the uterine canals with sponge-tents, I removed, in the presence of Dr. Arneth of Vienna, a small hard intra-uterine polypus, attached by a short pedicle to the fundus uteri.[1] The menorrhagia, however, shortly afterwards returned, and the patient died with the usual symptoms of uterine cancer about eight months afterwards, the cervix remaining, however, still unaffected.

Case V.—Three years ago, I saw repeatedly, during the last twelvemonth of her life, a patient of Dr. Brotherston of Alloa, who complained of bloody and fetid discharges from the vagina, with the os and cervix uteri, however, apparently quite healthy. She was above sixty years of age, had borne a large family, and menstruation had ceased thirteen years before the morbid discharge appeared. The discharge was from the first offensive in smell, and of a greenish color. There was a feeling of weight and uneasiness which never amounted to pain. At last, a fibroid polypus, which was in the interior of the uterus, sloughed and separated, and was removed artificially in a succession of pieces. For some time the patient's health seemed to be comparatively restored, and the discharges diminished; but again they became greater in quantity, very offensive, and occasionally mixed with blood. After one severe attack of hemorrhage ten days before death, all discharge whatever ceased, as if the canal into the cavity of the uterus had become occluded. Shortly afterwards, great collapse supervened; the ulcerated uterine walls having become lacerated or perforated by the retained and distending secretion. On dissection, the cervix of the uterus, from the os externum to the os internum, presented a perfectly healthy appearance. The body of the organ was distended into a large cavity, about five inches long, containing a semi-putrid fluid, and with its surface in several parts excavated by cancerous ulcerations, one of which had completely perforated or broken through the back wall of the uterus. The *os internum uteri* seemed to form a complete line of demarcation between the diseased and healthy parts. There was an abundance of pus and lymph in the peritoneal cavity.

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[1] Dr. Arneth has recorded the case in his Geburtshülfe und Gynækologie in Frankreich, Grossbritannien, und Irland.

CASE VI.—Last year I showed to the Medico-Chirurgical Society,[1] the drawing and preparation of a case of cancerous ulceration and perforation of the fundus uteri, where there still existed, attached by a short pedicle to the interior of the uterus, the remains of a fibroid polypus. The cervix uteri was unaffected with the cancerous disease. The patient had long suffered from menorrhagia and fetid discharges from the genital passages. But as in most other cases of carcinoma of the cavity of the uterus, she complained of little or no local pain during the course of the malady.

Let me observe in passing, that I have seen two or three well-marked instances of carcinoma of the cervix uteri follow apparently the irritation of a polypus when allowed to remain long without removal, even after the body of it had passed from the cavity of the uterus to the cavity of the vagina.

3. The soft or encephaloid variety of cancer sometimes affects the structures of the *fundus and body* of the uterus, without implicating the tissues of the cervix. In this variety of the disease the cancerous structure is seated in the walls of the fundus and body, and not in the cavity of the uterus. Sometimes the tumor reaches rapidly the size of a uterus at the fourth or fifth month of pregnancy. In this form there is not usually any menorrhagia or any peculiar discharge from the vagina. In the following instance, while the patient was under my care, hemorrhage accompanied it, but the hemorrhage was from the urinary, and not from the genital canals.

CASE VII.—An unmarried lady, 40 years of age, suffered for some time from pains in the back and lower extremities, particularly after exertion. When at last an examination was instituted by her physician, in Dublin, in 1851, a large tumor was found in the uterine region. Early in 1854 the tumor increased much and rapidly in size, and when I saw her soon after, it already reached half-way between the pubis and umbilicus. But still there was no appearance of general cachexia observable. After a few weeks the morbid structure again suddenly assumed a rapid growth; a dark, sanious, and bloody discharge was passed from the bladder; and the patient sank exhausted in the course of a few days.

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[1] See Edinburgh Monthly Journal of Medical Science, March, 1854, p. 285.

On laying open the abdominal cavity, the omentum was found adherent to a morbid mass, reaching from the pelvis to a point higher than the umbilicus. This mass or tumor was covered by large tortuous vessels; had a soft general consistence; and near the summit it was broken down and pulpy, and had evidently all but burst into the cavity of the peritoneum. The whole fundus and anterior wall of the uterus was implicated in the structure; but the cervix and tissues immediately surrounding it were free from morbid deposit, except in the form of two or three minute nodules. The bladder, however, was perforated posteriorly, and a portion of dark fungus from the uterine tumor projected into its cavity. The tumor, which was carefully examined, had all the characteristics of the Encephaloid or Hæmatoid variety of cancer. The cavity of the uterus presented no appearance of the disease, while the structure of the anterior wall and fundus of the organ were lost and merged in the encephaloid mass itself.

ON RETROVERSION OF THE UNIMPREGNATED UTERUS.

(From Dublin Quarterly Journal of Medical Science, May, 1848, p. 371.)

PRELIMINARY REMARKS AND DEFINITION.

DISEASES are sometimes regarded as rare, merely in consequence of a deficiency on our part of a proper and easy means of detecting them during life, or from our overlooking their existence in the dead body. Not many years ago, for instance, emphysema of the lung and granular degeneration of the kidney were supposed to be affections that were very seldom met with in practice. After, however, Laennec and Bright pointed out simple and ready modes of diagnosticating these diseases, they were speedily found to be morbid affections that were extremely common, instead of extremely rare, in their occurrence; and every physician at the present day is now ready to acknowledge their great frequency.

With some of the displacements of the unimpregnated uterus practitioners have long been familiar. In particular, the displacements of the organ downwards, in the form of prolapsus and procidentia, are recognized and acknowledged by all, and elaborately described in every work on female diseases. But displacements of the unimpregnated uterus, in the form of versions or flexions, either of the whole or of the upper part of the uterus, posteriorly, anteriorly, or laterally, have hitherto been looked upon as rare; and this, far more however from our past want of power of diagnosticating them, than from their own infrequency.

In the present communication, it is my object to describe briefly some new and simple means that I have practised for the last four or five years for detecting and treating these displacements.

Let me premise that, in the normal and healthy state, the long axis of the uterus is situated in a line parallel with the line of the axis of the brim of the pelvis, or in the relative direction represented in the accompanying diagram (Fig. 18, *b*). But the fundus of the uterus, instead of looking upwards, may be turned downwards and forwards, or downwards and backwards. In *e* it is represented as directed downwards and forwards, constituting *anteversion.* In *a*, *d*, *e*, it is directed downwards and backwards, constituting *retroversion.* These three figures of retroversion represent different forms or degrees of this displacement. The diagram *a*, represents an aggravated degree of retroversion, taken from a drawing of a case of this displacement by Frank. The mode in which the rectum is

Fig. 18.

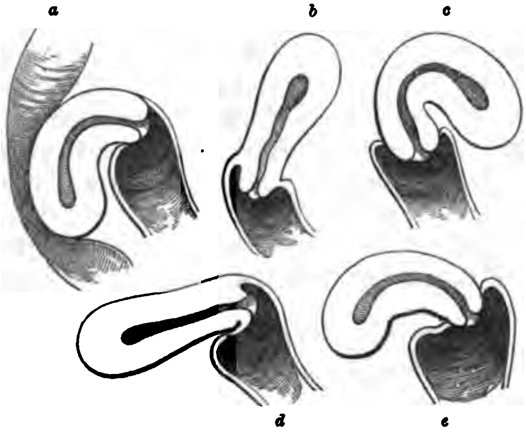

Fig. 18. These diagrams are intended to represent vertical or antero-posterior sections of the uterus, and upper part of the vagina. In *b* the uterus is supposed to be placed in its normal position, and the other four figures represent different deviations of the organ from this position.

impressed by the retroverted uterus is here shown. He found this instance of displacement in the body of a patient who had died of chest disease, but he does not give her previous history.[1]

Some authors have attempted to draw a specific line of distinction between the forms of posterior displacement of the uterus portrayed in *d* and *e*, and have described the form given in *e* as *retroflexion*, and that given in *d* as *retroversion*. In other words, by retroversion, properly so called, they would understand a displacement backwards of the entire organ, *d*—the flexion taking place in the upper part of the vagina, and the uterus itself not being necessarily changed in form. On the other hand, retroflexion *e*, is a term proposed to designate the displacement backwards of the fundus only, along with more or less of the body of the uterus; the lower part of the cervix uteri retaining in some degree, its natural position, and the flexion taking place in the substance of the body, or upper part of the cervix of the organ. But in reality, in the living subject, we meet with all possible intermediate shades and degrees of these posterior displacements; and I believe it to be an incorrect and unnecessary refinement to draw such theoretical nosological distinctions between them. Practically and pathologically, there is no true difference between these modifications or degrees of this morbid position of the uterus; and I shall in my subsequent remarks include them, and all other varieties of posterior diplacement, under the generic term of Retroversion. Farther, in order to avoid repetition, I shall in the present communication treat only of retroversion of the uterus. It will be found that the same principles of diagnosis and treatment apply, *mutatis mutandis*, to the almost equally common displacement of the uterus, which I have defined above as Anteversion.

ALLEGED RARITY OF RETROVERSION OF THE UNIMPREGNATED UTERUS.

In all our English systematic books on midwifery and female diseases, down to the very latest works, retroversion of the *unimpregnated* uterus is described as an exceedingly rare disease.

In his work on the Diseases peculiar to Women (1846), Dr. Ashwell tells us that he has "been long in the habit of observing uterine organic disease;" but he states "the published cases of retroversion are nearly silent on any other cause than pregnancy;" and he speaks of this as the result also of his own observations.[2]

Dr. Burns (1844) says: "Mr. Pearson relates a case where the uterus was retroverted in consequence of being scirrhous. Dr. Marcet gives an instance where the uterus was retroverted without pregnancy, producing constipation and vomiting. Dr. Alken of Bergheim relates a case where a woman, after suffering from difficulty of passing the urine and stools, had in fourteen days complete retention of both. The bladder reached to the umbilicus; the extremities were cold, the pulse small, vomiting, &c.: the urine was drawn off. After bleeding and the warm bath, force was employed in opposite directions, both from the rectum and vagina, and in an hour the uterus was replaced. It was, however, displaced again next day, but was reduced, and the retroversion did not return. The uterus was unimpregnated." Dr. Burns himself quotes these cases in illustration of his own opinion, that retroversion, besides occurring during pregnancy, "*may* also be produced when the womb is *enlarged* to a certain degree by disease."[1]

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[1] Opuscula Posthuma, p. 78. [2] Practical Treatise on the Diseases peculiar to Women, p. 598.

Writing in 1844, Dr. Churchill observes: "I have known retroversion to happen the first day of a menstrual period, when the weight of the uterus was increased by afflux of blood. Mr. Pearson and Dr. Blundell met with cases of retroversion caused by scirrhus. Callisen and Blundell mention cases where this accident followed delivery, *but such must be exceedingly rare.*"[2]

The experience of the few last years has amply convinced me that these opinions regarding the supposed rarity of retroversion of the unimpregnated uterus are entirely wrong. Since discovering an easy method of detecting its existence, I have found it one of the most common and frequent displacements and affections of the unimpregnated uterus.[3] My observations in this respect have been fully confirmed by several of my professional brethren in Edinburgh. Three or four years ago I pointed out its frequency, mode of diagnosis, and treatment, to my friends, Drs. Rigby and Protheroe Smith, of London; and I have much pleasure in adding, that their extensive opportunities at the London Hospital for Uterine Diseases have enabled them and the pupils of that useful institution to confirm amply the justness of my previous deductions, with regard to the great frequency of retroversion, and the advantages of my proposed methods of detecting and treating it.

SYMPTOMS AND DIAGNOSIS OF RETROVERSION.

General Remarks.—I have before stated[4] that the morbid conditions of the uterus are recognized in practice by two classes of symptoms, viz., the *functional and physical*. The evidence derived from these two different classes is different in its nature and value.

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[1] Principles of Midwifery, p. 288.

[2] On the Principal Diseases of Females, p. 267.

[3] In April, 1843, I stated these results in a communication to the Medico-Chirurgical Society of Edinburgh; showed the frequency of retroversion of the unimpregnated uterus, and its means of detection and cure. See Monthly Journal of Medical Science for July, 1843, p. 660.

[4] See p. 52 of this volume. To avoid repetition we have here omitted a page which

All accoucheurs will, I believe, readily admit that the two following observations hold good with regard to the symptoms and diagnosis of utero-gestation, viz.—1. That the state of the uterus in pregnancy, one and identical as it is, is liable to be accompanied, in different women, or in the same woman in different pregnancies, with very different local, sympathetic, and general effects or functional symptoms; and, 2. That the usual concourse and succession of functional phenomena, to which pregnancy generally gives rise, may be induced by other states and irritations of the organ than utero-gestation. Hence, then, I attach far less importance to the functional symptoms of retroversion which I have first to enumerate, than to the physical signs, which I shall afterwards consider.

FUNCTIONAL SYMPTOMS OR DERANGEMENTS.

In some cases of retroversion of the unimpregnated uterus, more especially when the displacement is chronic and the pelvis large, as in some other forms of serious uterine disease and of pregnancy, few or no marked functional or sympathetic symptoms, either local or general, are present; while in other instances the attendant functional derangements and irritations are excessively severe and distressing. And in this, as in other uterine affections, between these two extremes we may meet with every shade of difference.

In retroversion, as in other morbid conditions and diseases of the unimpregnated uterus, the accompanying sympathetic derangements or symptoms are, when they are well and highly marked, more or less perfect imitations of the secondary phenomena of pregnancy. Dyspeptic and hysterical symptoms are sometimes present, with local neuralgic pains in the mammæ; in some portions of the vertebral column; or, what is still more frequent, in the parietes of the abdomen or chest, and more especially in a limited spot beneath the left mamma. The displaced position of the uterus often gives rise to mechanical irritations and symptoms of the same kind as if the organ were actually morbidly enlarged. Constipation and impeded defecation are frequent results, partly from the fundus of the displaced uterus physically compressing the calibre of the rectum, and partly from its producing a functional inability to expel the feculent contents of the bowel through the lowest part of the canal. Occasionally the bowel is irritated, and there are discharged from it,

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formed part of the original memoir, and which, in its detached form, was then necessary to complete the monograph.—(Ed.)

from time to time, quantities of mucous or fibrinous-like effusions. The bladder frequently suffers from dysuria or retention; and, much more rarely, I have seen a degree of incontinence, especially where the urine has become phosphatic, from the want of power in some cases of completely emptying the bladder. Symptoms of weight, tension, and bearing down in the regions of the uterus and rectum, with dragging at the loins and in the regions of the uterine ligaments, are very common. Pains often stretch down one or both of the lower extremities. Occasionally there is an inability to bear carriage exercise, and walking and standing speedily produce fatigue. In a few cases I have known the patients to find themselves forced to remain almost constantly in the horizontal position, from the intense and overpowering feeling of pressure and malaise which the erect posture always brought on, and the power of standing and progression restored by the spontaneous or artificial reposition of the uterus. In general, all the symptoms, local and constitutional, which I have alluded to, are aggravated more or less by exercise in the erect position; and they are more particularly liable to be increased in their intensity when the uterus becomes periodically congested and heavier, at the recurrence of each menstrual period.

In some cases of retroversion the menstrual function is not morbidly altered. In other cases, however, I have seen the catamenial discharge affected most oppositely and variously—occasionally in the way of amenorrhœa, sometimes of dysmenorrhœa, and not unfrequently of menorrhagia, particularly after miscarriage.

The mucous secretion of the uterus is not altered by retroversion, unless congestion or inflammation supervene; it may then change into leucorrhœa. Occasionally there is a sudden temporary increase of discharge, once or oftener during the intervals between the catamenial periods, as if it had collected in the cavity of the retroverted organ, and escaped, or become expelled, only from time to time.

When a patient with a retroverted uterus becomes pregnant, abortion is apt to take place. But I have seen various instances in which the uterus became spontaneously rectified in position as it became larger, and utero-gestation went on to the full time. Usually the existence of retroversion interferes with the function of conception. Often it is a cause of sterility, as shown by impregnation taking place after the displacement is rectified. In women who have borne children at distances of several years between each, I have several times found the uterus permanently retroverted in the unimpregnated state.

The functional symptoms that I have enumerated may make us suspect the existence of retroversion of the uterus. But retroversion may be present without most, or almost any of them; and they may be present with other diseases besides retroversion. Hence the necessity here, as elsewhere—in this, as in other uterine affections—of having recourse to the physical examination of the uterus, in order to decide and determine its actual morbid state.

PHYSICAL SIGNS OF RETROVERSION.

The usual physical means of diagnosis of uterine disease are reducible to the observation of phenomena by the senses of sight and touch.[1]

Speculum.—The employment of sight, by means of the speculum, assists us in no respect in the diagnosis of retroversion.

Tactile Examination.—On an accurate vaginal examination we feel an apparent projection of a solid tumor between the uterus and rectum, when applying our finger, or fingers, behind the cervix uteri to the *posterior* part of the upper reflection, or roof of the vagina. (At *a*, Fig. 19.) The same firm mass is felt through the anterior wall of the bowel in making an anal examination. The tumor or mass feels smooth and roundish on its surface; is often sensitive on pressure, more especially if the retroversion is recent, or when the posterior wall is, as often happens, congested and engorged; is generally capable of being moved more or less easily by the finger; and varies in size according to the degree of displacement, and the morbid or healthy state of the uterine walls.

The os and cervix uteri may be displaced forwards, or they may maintain their usual position. The whole body of the uterus is often prolapsed and lower than its natural situation; but occasionally it is quite normal in these respects.

How are we to determine that the solid tumor lying upon the back part of the roof of the vagina, and between the rectum and uterus, is the displaced fundus and body of the uterus?

If the patient be unusually thin and emaciated, and we examine simultaneously with one or two fingers of the right hand placed in the vagina, and those of the left hand placed above the pubis, we can almost feel the uterus between the two hands, and ascertain the whole position and relations of the organ; but this, however, can rarely be accomplished.

Generally, we have by tactile examination no other means of knowing the probability of the apparent tumor being formed by the displaced fundus and body of the uterus, than by tracing along with the finger, between the tumor and cervix uteri, a direct *continuity* of structure, and this may be done either per vaginam or per rectum. But this physical sign is in itself apt to lead into error, if alone depended on. If the uterus is retroflected more than retroverted, the continuity cannot be traced at the point or angle of flexion; while on the other hand, a fibrous or other tumor attached to the back wall of the uterus sometimes may be distinctly traced to be *in continuity* with the uterine structure, and moves with all motions imparted to the cervix. Nor is the position of the os uteri any more certain guide, for sometimes it is not displaced when the fundus is so; and it may be thrown forwards by a tumor, when the fundus retains its normal position.

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[1] In a few cases the sense of hearing is also had recourse to. I have repeatedly heard with the stethoscope a sound like the placental souffle in large fibrous tumors of the uterus. The uterine walls around these tumors are sometimes hypertrophied and thickened, exactly like the walls of the uterus in pregnancy, and their vascular structure undergoes a similar increase and mutation. Hence, probably, the origin of the sound in question. When present, I believe it to serve, among others, as a diagnostic mark between fibrous tumors of the uterus and ovarian tumors.

Other and additional means, therefore, of diagnosis become necessary. M. Pereyra, of Bordeaux, suggested, a few years ago, to attempt to obtain a more correct physical diagnosis for retroversion, by conducting the examination by the rectum or bladder. "The difficulty," he observes, "of distinguishing diseased growths from retroversion of the uterus is greater than might at first be supposed; the only way of attaining a correct diagnosis is to ascertain if the uterus be or not in its normal situation. Two methods are proposed for this end; either the forefinger is introduced into the rectum, or a male catheter into the bladder, by the extremity of which instrument an exploratory process is conducted. The latter mode, treated of by Malgaigne,[1] requires, of course, some tact, but to the experienced surgeon it will give the more certain indication."[2]

The determination of the case by an examination per rectum is impossible; and I have elsewhere shown that Malgaigne's method of diagnosis is attended with difficulties and uncertainties that render it quite useless in practice.[3]

A far more simple and certain method is to determine the precise situation of the fundus and body of the uterus, not through either the intestinal canal behind, or the urinary canal in front, but through the intermediate genital canal itself. The proper canal of the uterus is, of course, too narrow to allow us to introduce our finger into it; but by passing into it a slender metallic finger, if we may so speak, we can easily by it ascertain, amongst other matters, any change in the *direction* of its cavity, and consequently in the direction of the body and fundus of the uterus itself. The employment of the uterine bougie readily enables us to do this.

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[1] These du Concours, 1833.

[2] American Journal of Medical Sciences, April, 1843, p. 483.

[3] Seé ante, p. 95.

Examination by the Uterine Sound or Bougie.—The form, &c., of the uterine bougie is represented in Fig. 1, and consequently requires little or no description. Some years ago I gave a full account of the instrument, and the mode of using it.[1] It has the configuration of a slender male catheter; tapering in form; knobbed at the extremity; divided into sections, so as to measure, when required, the length of the uterine cavity; and provided with a handle, smooth on its posterior surface and roughened on its anterior, that surface represented in the wood-cut, in order to make the operator constantly aware of the position and direction of the point and concavity of the instrument, when it is passed into and hid in the uterine cavity.

This instrument can be easily and readily passed into the uterine cavity, so as to enable us to measure its depth; to examine, more distinctly than we have otherwise the power of doing, its fundus, body, and cervix; to ascertain the presence of strictures in the canal; diseased states of the cavity, and walls of the organ, &c. I have used it daily for five or six years past, and have never, in any instance, seen any serious irritation, or any bad result to the uterus, follow its employment; whilst it has enabled myself and others to detect and discriminate morbid conditions of this organ, that were, by any other means, entirely beyond the reach of correct diagnosis.

Its power of detecting retroversion of the unimpregnated uterus depends, as I have already stated, upon its directly and easily enabling us to ascertain the *direction of the uterine cavity*, and hence of the body and fundus of the uterus, which form the walls of that cavity.

When the uterus is in its normal position, and is placed with the long axis of the organ, and consequently the long axis of its cavity, in a line parallel with the axis of the pelvic brim, the point of the bougie, when introduced into the uterus, passes upwards and forwards in the direction of the umbilicus; and the concavity of the instrument, or the rough side of its handle, is directed towards the symphysis pubis. When, however, the uterus is retroverted, the point of the instrument, instead of passing up vertically and forwards, is resisted in that direction, and can only be passed horizontally and backwards towards the hollow of the sacrum; its concavity and the rough side of its handle thus looking towards the sacrum instead of towards the pubis, and at once showing the altered position of the cavity, body, and fundus of the uterus.

But the diagnosis may be made out still more completely and accurately by the further use of the bougie. For—

1. Besides showing in the manner stated, the direction of the

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[1] See ante, p. 58.

uterine cavity, and hence of the body and fundus of the uterus, by the direction in which the instrument itself passes,—

2. We can ascertain by a vaginal or anal examination of the supposed tumor, that the extremity of the uterine bougie is lodged in *its* centre, showing the swelling to be produced merely by the displaced fundus of the uterus; and,—

3. After this, by turning the bougie gently round so as to bring the concavity, or the rough side of its handle, to look to the pubis, instead of looking, as at first, to the sacrum, we can replace the uterus and feel it upon the bougie if required, through the abdominal parietes in front. We can thus certify to our own minds that we have nothing on the point of the instrument except the fundus uteri. And again, if necessary, by introducing a finger into the rectum or vagina, and then retroverting or replacing the uterus at will, we can as it were make and unmake, as often as required, the apparent tumor lying between the uterus and rectum, and thus further prove this tumor to be nothing whatever but the retroverted fundus uteri.

DIFFERENTIAL DIAGNOSIS OF RETROVERSION.

I have seen retroversion of the unimpregnated uterus not only very frequently and entirely overlooked, but also very often mistaken for other morbid states and lesions of the uterus. I shall point out the principal morbid conditions with which I have known it to be confounded, and the modes of distinguishing retroversion from them.

1. *From Pregnancy.*—In a considerable number of instances I have had occasion to see the feeling of fulness and apparently increased size of the uterus in retroversion mistaken for the earlier periods of utero-gestation. A few weeks ago I was called to a case at a distance from Edinburgh, where this error of diagnosis had led to much distress: from an unmarried lady, suffering severely from dysmenorrhœa and menorrhagia, being supposed by her medical attendant to be pregnant and aborting, by his mistaking the retroverted fundus and body of the uterus for general enlargement of the organ. This error is still more liable to be committed when the retroversion is accompanied, as it sometimes is, with occasional amenorrhœa. Two or three years ago, I had under my care a patient with retroversion of the uterus and temporary amenorrhœa, who had been pronounced as undoubtedly pregnant by her usual medical attendant, an excellent practitioner and lecturer on midwifery in another medical school. About the same

time I had a patient under my own care with retroversion, who passed three successive menstrual periods; but I was certain, from no corresponding increase in the size of the retroverted uterus, that she was not pregnant. In this case the difficulty of the diagnosis was rendered the greater in consequence of the areolæ becoming darker and their follicles enlarged as in pregnancy. A drawing was made of the areolæ at the time; and afterwards, when the uterus was replaced, and the patient at last became pregnant, the areolæ were most certainly not deeper marked at the same period in the true, than they had been in the spurious pregnancy.

2. *From Fibrous and other Tumors in the posterior Wall, &c., of the Uterus.*—This is one of the most frequent errors of diagnosis which I have met with, and one into which, in former times, I myself frequently fell.

The attendant functional symptoms are in all respects the same; and on examination there is the same continuity of structure felt between the cervix uteri and the body lying between it and the rectum. In this way retroversion of the uterus has very often been mistaken for a morbid growth upon the back part of the uterus, and even described as such.[1] But the introduction and direction of the uterine bougie at once enables us to solve the difficulty. The bougie passes backwards into the very body and centre of the apparent tumor, at once showing it to be the retroverted fundus uteri. It may be proper, however, to add, that instances are by no means rare in practice of the presence of small fibrous tumors attached to the posterior wall of the uterus being conjoined with retroversion.

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[1] Dr. Hamilton, in his Outlines of Midwifery, describes retroversion as an "unequal projection" on the posterior part of the uterus. I have seen one or two patients whom he had pronounced in writing to be laboring under these "projections," and where the apparent tumor was the retroverted fundus uteri. In the work quoted he observes: "An unequal projection of different sizes is occasionally discovered on the posterior part of the uterus, resembling in shape the tubera which form upon the surface of the liver, but differing from tubera in being of a more resisting texture, and in being pained on pressure. From the cases which have fallen under the author's notice, it appears to him that the following is the progress of this fortunately rare disease. At first there is a slight enlargement of the uterus, with a little thickening and tenderness of its posterior surface, occasioning a sense of bearing down on making any unusual exertion, and an obscure gnawing pain towards the back part of the pelvis. In the progress of the disease the posterior surface of the uterus becomes more and more unequal, till at last a distinct projection like a walnut, or even larger, can be felt on examination per anum. At this stage of the disease the patient can neither stand nor sit upright, such is the continued uneasiness in the back part of the pelvis. It is remarkable that in this, as well as in several other of the local diseases of the uterus, the catamenia continued to flow as usual. In the early stages of this disease the progress has been generally checked by the means employed in cases of chronic enlargement of the uterus; but in the latter stages, that is, after the circumscribed projection has taken place, no other means of treatment have hitherto proved successful."—Outlines of Midwifery, p. 134.

I have known the retroverted fundus uteri to be pronounced a carcinomatous tumor, local thickening of the back walls, &c. The differential diagnosis is readily made, in the way I have just mentioned. When it has occurred after delivery I have seen it mistaken for the common puerperal hypertrophy of the uterus. The means of differential diagnosis are the same. And cases, in which the apparent swelling formed by the back wall of the deflected uterus have been, out of the pregnant and puerperal states, mistaken for simple general hypertrophy of the uterus, and assiduously treated by mercury, iodine, &c., have frequently come under my notice.

3. *From Ovarian Tumors in their earlier Stages.*—When the ovary enlarges from multilocular degeneration, or other causes, it almost always first grows downwards into the space lying between the back wall of the uterus and the anterior part of the rectum, resting thus upon the roof of the vagina behind. In its enlargement it almost invariably pushes the uterus anteriorly, and *before* it; and this relative position of the uterus to ovarian tumors is often an important matter in the uterine diagnosis of ovarian disease in its latter and more advanced stages. At first the body of the enlarged ovary may be mistaken for the retroverted fundus uteri, more especially as the os uteri is generally displaced forwards. But the introduction of the bougie shows the uterus to be in its normal situation, and at the same time generally enables us to draw the uterus so far forwards as to make us certain that it is not attached to the existing tumor, and does not form one continuous structure with it. So far the evidence is merely negative. If further evidence of a positive kind, of the nature of the tumor, is required, we may obtain it by the use of a fine exploring needle, a means of diagnosis of great value in this as in other complications.

4. *From Pelvic Cellulitis.*—Inflammation of the cellular tissue of the pelvis, limited or more diffuse, is certainly a frequent disease, both after delivery and in the unimpregnated state. I have seen it now at many different periods of life, from six years up to sixty. It is generally spoken of as "pelvic abscess," but improperly so, for it does not always necessarily terminate in empyema. When the inflammatory effusion seems limited, as it sometimes is, to the space between the uterus and rectum, the firm tumor, or swelling formed by it, may be mistaken for retroversion. The direction of the bougie, when introduced into the uterus is *not* retroverted; and the accompanying symptoms, and if necessary, the use of the exploring needle, will enable us to complete the diagnosis.

5. *From Extra-uterine Conceptions lodged between the Uterus and Rectum.*—Nauche[1] mentions a case in which an extra-uterine concep-

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[1] Maladies des Femmes, p. 108.

tion was mistaken for a retroverted uterus. I lately met with an instance where it was a matter at first of great doubt and difficulty, whether a tumor lying on the anterior wall of the rectum, and accompanied with sudden symptoms of rupture into the peritoneum, was an enlarged and retroverted uterus or an extra-uterine conception. Examinations with the bougie at once showed the uterus to be both normal in its size and in its position.

6. *From Organic Disease in the Anterior Wall of the Rectum.*—In a case of Dr. Marnoch's the tactile examination of a tumor lying between the uterus and rectum gave me the idea that it was a retroverted uterus. The employment of the bougie, however, showed the uterus to be normally placed. On more minutely examining per anum, the anterior wall of the rectum was found much thickened and indurated; the patient died some months subsequently of carcinomatous stricture and disease of the rectum.

7. *From Stricture of the Rectum.*—The diagram of retroversion in Fig. 19 shows how readily this disease may be mistaken for stricture in the rectum; the deflected fundus uteri pressing in upon, and sometimes diminishing greatly, the calibre of the bowel. But the use of the bougie always readily dispels the difficulty, by showing first the direction of the fundus, and secondly, when the instrument is turned round, by at once removing the fundus and the supposed stricture. But I know that the mistake of confounding a retroverted uterus with stricture of the rectum is by no means infrequent in practice. Sir Charles Bell states that he had found a surgeon employing rectum bougies for years, on account of an obstruction from

Fig. 19.

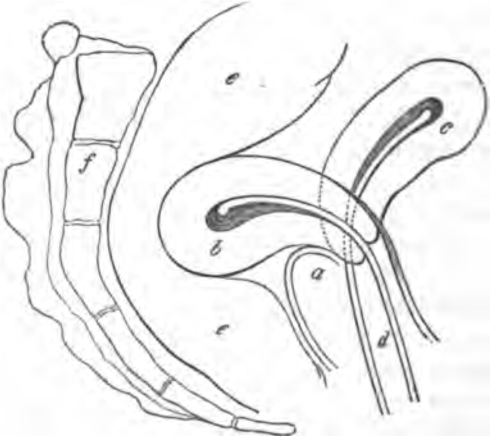

Fig. 19 is a diagram of the position of the retroverted uterus *b*; and its replacement to *c*, by the sound, *d*; *f* being the sacrum; *e*, the rectum; and *a*, the vagina.

displaced uterus.[1] A case of retroversion of the unimpregnated uterus some time ago came under the care of a medical friend of mine in Edinburgh. He discovered the displacement, introduced the wire pessary (Fig. 22), which I shall afterwards describe, and at once rendered his patient comfortable, and capable of taking exercise. She returned to her own distant home, with a line to her physician, who declared that he knew the instrument well, but thought it necessary to take it out at the menstrual period, and could not again replace it. Another distinguished obstetrician was called in his place. He said the uterus was enlarged, and not displaced; used leeches, &c. Not finding the desired benefit from this treatment, the lady placed herself under the care of an eminent surgeon, who pronounced all the previous opinions wrong, and that the real disease was stricture of the rectum. The last time I heard of the patient she was submitting to the frequent use of bougies for the cure of this imaginary affection.

RETROVERSION LIABLE TO ESCAPE NOTICE EVEN IN POST-MORTEM EXAMINATIONS.

In the preceding pages I have spoken of retroversion of the unimpregnated uterus being with extreme frequency entirely mistaken in practice, and overlooked during life. But even after death the same error is liable to occur. Dr. Robertson details[2] a case most illustrative of this remark. A woman died of tenesmus, constipation, and symptoms of obstructed bowel, ending in enteritis. Before death the rectum was examined for the obstruction, and the gut was found "encroached on by a tumor which, per vaginam, was discovered to be the uterus." "But," adds Dr. R., "on the post-mortem examination, to our surprise, no uterine *tumor* was found to encroach on the rectum." In this, as in other cases in which retroversion of the uterus has existed, the morbid displacement has, on the inspection of the body after death, escaped notice, from the form and structure, and not the mere *position*, of the uterus being looked to. In our common post-mortem method of examining the pelvic contents from above, the attention of the morbid anatomist is rarely or never directed to the observation of any mere deflection of the fundus and body of the uterus ;. and when once the parts are dragged out of their situation, it is impossible to ascertain the amount and degree of retroversion.

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[1] Institutes of Surgery, vol. ii. p. 216.

[2] Edinburgh Medical and Surgical Journal for 1822, p. 525.

ORGANIC STATE OF THE UTERUS IN RETROVERSION.

This is very variable. I have seen it several times coexisting with the presence of fibrous tumors in the uterine parietes. More frequently the uterus is hypertrophied merely from chronic metritis, and the enlargement more especially marked in the posterior walls. The organ is at the same time elongated as well as hypertrophied; and its cavity, instead of measuring two and a half inches in length, will measure three or three and a half inches. Many authors seem to think that enlargement of the uterus, under some form or another, is a necessary preliminary to retroversion taking place, and that we never meet with the displacement, without finding it combined with some increase in the size of the organ. Such, however, is assuredly not the fact. In a large proportion of cases the retroverted uterus is in no degree enlarged or increased in volume, but natural in size. Nay, I have on more than one occasion seen the uterus retroverted, when it was less than normal in its length and dimensions. Latterly I have happened to be consulted in several instances of amenorrhœa in women advanced beyond twenty or thirty years of life, in whom the menstrual secretion had never appeared, and where, on examination, the uterus was found to be imperfectly developed, and the length of its cavity, as measured by the uterine bougie, was not above one, one and a half, or at most two inches. In one of these instances, in which the cavity of the uterus was only one and a half inches long, the fundus of the preternaturally small organ was, at the same time, distinctly retroverted, and felt like a small roundish tumor through the vagina and rectum. Some time ago I saw, with my friend Dr. Girdwood of Paddington, a case still more rare. The uterus was retroverted, as he had ascertained by examination and the use of the bougie. There was a second orifice in the cervix uteri. On introducing a second bougie into this additional orifice, I found it pass into a second uterine cavity, quite separated and distinct from the first, and with the fundus of each diverging from the other. In fact, the uterus was not only retroverted, but double. It is, as far as I am aware, the first case in which a double uterus has been distinctly diagnosticated upon the living subject.

TREATMENT OF RETROVERSION OF THE UNIMPREGNATED UTERUS.

When recent, and occurring after some straining effort, or from congestion and inflammation of the uterus, or subsequent to delivery, mere replacement of the organ by the bougie or finger will sometimes suffice, provided, along with it, we enforce for a length of time the horizontal position, or rather lying on the side or face, prevent over distension of the rectum and bladder, reduce any local congestive or inflammatory state that may be present, and restore the local tone of the relaxed soft structures of the pelvis by astringent vaginal injections, or by the use of medicated pessaries made with ointment containing extract of oak-bark, or tannin, or iodide of lead, &c.

But such simple treatment rarely succeeds, even when the retroversion is recent; and still more seldom when, as is almost always the case in practice, the displacement is already chronic and confirmed. Under these circumstances we have three principal indications to perform:—1. To remove, if necessary and possible, any morbid action in the uterine structures that may exist along with the displacement. 2. To restore the uterus to its normal situation. 3. To use means to retain it in its replaced and natural position. I shall speak separately of these several indications.

1. *Removal of any Morbid State of the Uterus that may coexist with the Retroversion.*—Not unfrequently, along with retroversion, the uterus is congested and hypertrophied, and the uterine bougie shows it to be elongated half an inch or more. Sometimes chronic inflammation of the body of the organ is present, and more especially in its posterior wall, which often feels tumefied and tender to the touch; or the cervix is enlarged, condensed, and ulcerated, especially in its posterior lip. The os uteri, or the uterine canal, an inch or so above the os, is not unfrequently contracted and strictured, and may have been giving rise to retention of the menses and congestion. Occasionally one or both ovaries can be felt through the roof of the vagina, enlarged and painful from congestive or inflammatory irritation. As a general rule, all these morbid states should, when possible, be reduced and removed, or at least moderated by their appropriate means of treatment, before engaging with the other indications to be fulfilled, particularly when they are apparently in any respect the *cause* of the retroversion. More frequently, however, they are the *effects* of the retroversion; and in this latter case our means of combating them will usually fail, or only partially succeed, till we have first restored the organ to its natural form and situation, by our fulfilling the second and third indications which I have laid down, before we fulfil the first. When the retroversion is combined with and produced by the presence of fibrous tumors in the posterior wall or fundus of the uterus, the first indication cannot, of course, be accomplished, as we as yet possess no certain power of removing and discussing these tumors.

2. *Restoration of the Uterus to its Normal Situation.*—Most authors who have treated of retroversion of the unimpregnated uterus have

spoken as if its replacement could generally be fully effected by the fingers alone. In practice, however, its complete replacement by this means is almost always found to be impossible. By pushing up the retroverted fundus, or by pulling down the cervix, or by combining simultaneously both measures, the replacement can sometimes be effected by the fingers alone; but rarely. For it is scarcely ever possible, by any pressure which we can make with the fingers upon the posterior surface of the uterus, either through the vagina or through the rectum, to push the fundus uteri upwards and forwards to a sufficient degree. Some authors finding their fingers to fail, from their shortness, &c., have proposed instruments for the purpose. Richter and Evrat suggested a species of vectis or probang, to be used through the rectum. Bellanger, Lallemand, and Duges, have spoken of introducing a strong sound into the urethra and bladder, to act as a lever upon the os uteri. Siebold and Drejer attempted to replace the uterus by instruments made of whalebone, introduced into the vagina, and made to press by their upper and blunted extremities upon the fundus uteri through the upper and back portion of the vaginal parietes.

The simplest and most easy method of temporarily replacing the retroverted uterus, is by introducing an instrument into the cavity of the organ itself, and using it as a mechanical means or lever, for the purpose. In treating of the physical diagnosis of retroversion, I have shown how the uterine bougie can be readily used for this purpose. The very means of diagnosis are thus, at the same time, the very means of replacement.

3. *Retention of the Replaced Uterus in its Normal Situation.*—This is necessary to effect a cure. Some, as Schmitt and Schweighauser, have supposed that it could be accomplished by the mere position of the patient, and that lying on the side or face, with the pelvis somewhat elevated, would be sufficient. I have already stated that in few—exceedingly few cases, indeed—will it be found to succeed, and these only of recent origin. Instruments for the purpose of supporting and retaining the uterus in situ are therefore necessary. They have been proposed to be worn in the rectum,[1] and in the vagina.[2] Various forms and modifications of vaginal pessaries have been invented by Hervez, Drejer, Sander, &c. They are all intended either to press principally, by guarded stems or otherwise, through the roof of the vagina, upon the tumor formed by the fundus uteri; or to keep the cervix uteri pushed back, with the view of throwing the fundus forwards. I have used sponges for this last purpose; and Siebold and Kilian state, that they believe they form the best pessa-

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[1] Ætius, Verandois, Desault.

[2] Colombat, Duges, &c.

ries for retroversion, modified according to the necessities of each case. Moreau conceives that the principle of treatment should be to fill up the space between the uterus and rectum, so as to take away any room for the retroversion to recur: he uses a kind of caoutchouc pessary for this purpose. Two years ago, I removed from a patient one which had been worn for some months, and which Moreau himself had introduced. It had not in any degree benefited the retroversion.

After detecting the ease and certainty with which the uterine bougie could be used for the discovery of retroversion, and for the temporary restitution of the organ, it appeared to me that the most direct and perfect method of retaining the replaced uterus in situ would consist in some means of retaining and fixing, as it were, the end of the bougie for a time in the uterine cavity. I soon found that the restoration of the uterus temporarily, from day to day, with the bougie, was insufficient, and that some more permanent means of keeping the organ replaced and retained were necessary. But one primary and important point required to be solved:—Would the uterus bear with impunity the presence of such a body for a length of time in its own cavity? My first experiment on the subject I made with extreme anxiety, and great misgivings as to the result. I watched the case from hour to hour, and from day to day, and was delighted to find that the presence of the instrument was borne by the uterus without irritation or annoyance. The patient was almost entirely incapacitated from walking, by retroversion of the uterus, before it was used. After thus wearing for some months a wire pessary in the cavity of the uterus, she so far recovered as to bear two children, one in this country, and subsequently another in India. I soon found in other cases, that, when the instrument was once properly fitted and adjusted, it could be borne with perfect safety, and without any pain or inconvenience. Occasionally I have since met with cases in which the uterine pessary has created so much irritation as to render its withdrawal necessary in a few days after it was introduced. But these cases have been exceptions, to the general rule; and I have allowed the instrument sometimes to remain in the uterus for two, four, six months, or longer. Within the last few weeks I have removed two uterine pessaries, one of which I had introduced eight, and the other nearly ten months previously. They are not to be interfered with at the menstrual periods. Sometimes, though by no means always, a slight menorrhagia follows their use, particularly for the first period or two after their introduction.

It is unnecessary to detail the changes of form which were tried, and the difficulties I met with, in first attempting to construct a proper uterine pessary. The three forms which I have been in the habit of using for the last three or four years are those depicted in the wood-cuts.[1]

First Form of Uterine Pessary, Fig. 20, *a*, *b*, *d*.—The stem of the instrument, *d*, 2⅛ inches long, is introduced into the uterine cavity; the lips of the cervix uteri rest upon the flattened ovoid bulb or ball; *a*, shows the lower surface of the bulb or ball, with an orifice in it to allow of the end of a staff, *c*, about 8 inches in length, to be fixed in it, for facilitating its introduction into the uterus.[2] This form of pessary generally answers much better for anteversion than for retroversion. Occasionally I have had the bulb made of lead, that it might serve as a counterpoise to hold the os uteri in situ. The instrument, however, is imperfect, from the impossibility usually of retaining it in the uterus above a few days, the canal relaxing and dilating, and allowing of its escape. In fact it is exactly the form of bougie which I generally use to dilate the os and canal of the uterus in cases of stricture, in any cases in which I do not employ the knife or hysterotome for that purpose.

Fig. 20.

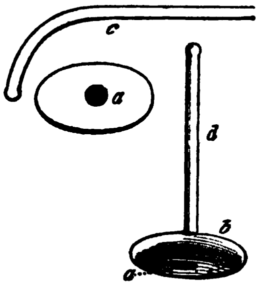
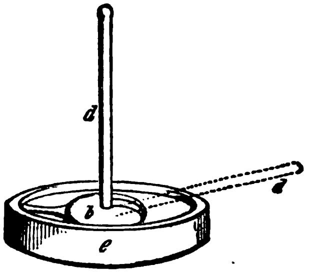


Fig. 21.



Second Form of Pessary, Fig. 21, has a similar uterine stem and bulb, and in addition a large ovoid disk, 2⅝ inches in length, 1¼ inches broad, and ½ an inch in depth, to retain the instrument in situ. The bulb, *b*, for the cervix uteri to rest upon, is fixed in the middle of the disk, and the uterine stem, *d*, is movable to a certain extent upon it. This is necessary in consequence of the size of the retaining disk, and the impossibility of introducing the instrument into the uterus and genital passages, with the stem fixed upright, and at right angles to the plane of the disk. The dotted line represents the stem laid down, as is required in the introduction and withdrawal of the instrument. On the lower surface of the instrument, which is not seen in the figure, there is a spring-catch to hold the stem fixed and upright after the instrument is introduced, and capable of being unlocked by the nail when the instrument requires to be again withdrawn. The expanded disk, by pressing on the sides of the vagina, enables the uterine portion of the pessary to keep its situation. In some cases this instrument answers well in retaining the replaced uterus. But occasionally, when the *tendency* to displacement is *great*, this form of pessary is altogether inadequate, and will be moved about by the changes of position which the uterus itself undergoes. The remaining form which I most frequently employ is free from these disadvantages. The first two forms are, when used, altogether concealed within the genital passages. In this third form a portion of the instrument is placed externally, and another internally.

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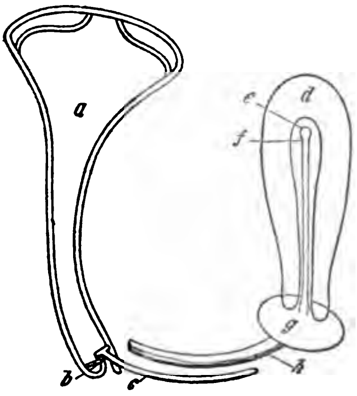
[1] Mr. Young, our cutler in Edinburgh, makes them of German silver; sometimes he has electrotyped them for me, but this addition is unnecessary.

[2] Dr. Simpson has found in practice that generally the experienced hand does not need the assistance of the staff. This we have also observed ourselves.—(Ed.)

Third Form of Pessary, Fig. 22.[1]—It is made up of two parts: 1. An *internal* part, provided, like the two preceding pessaries, with a stem, *f*, to pass into the uterine cavity; a bulb or ball, *g*, for the cervix to rest upon; and in addition, a vaginal portion, or curvilinear tube, *h*. 2. An *external* part, consisting of a wire framework, *a*, *b*, *c*, to maintain and hold the internal portion in situ. This external part, or wire frame, is about 5 inches long; at its lowest or vaginal portion it is about ½ an inch broad; and towards its upper or pubic portion, it suddenly swells out to 3 inches in breadth. From the vaginal extremity of this frame projects, at nearly right angles to it, a flat tubular portion, *c*, 2½ inches long, closed at its further extremity, like the point of a female catheter, and fitted to slide

Fig. 22.



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[1] The different parts of the pessaries are represented in the wood-cuts as somewhat below their actual size. The bulb or ball is, in general, made an inch and a half long, one inch broad, and about half an inch thick. I have sometimes used a larger bulb. When smaller, it is apt to produce dilatation of the os uteri, and even to pass partially into it.

into and fix in the corresponding open tube, *h*, attached to the bulb of the internal half of the instrument, *g*.[1]

In the wood-cut the uterine stem of the instrument is represented as placed in the cavity, *e*, of the uterus, a section of the organ *d*, being outlined around it. In introducing the instrument, the internal portion, *f*, is first passed into the uterus and vagina, in the same way as the uterine bougie is passed for the purpose of diagnosis. The retroverted uterus is then replaced by raising it up upon this portion of the instrument, which is turned round for that purpose. After the organ is replaced, the solid vaginal portion attached to the external frame of the pessary is slid into the corresponding vaginal tube, *h*, of the internal half of the instrument, locking into it on the same principle as the stilet locks into the canula of a curved trocar. Thus the instrument is, as it were, rendered at once completely solid and fixed. In order to have room to lock the two portions together, it is necessary, in general, first to bend back the pubic portion of the external framework to a very considerable extent, for the purpose of avoiding its being caught and impeded by the anterior part of the pelvis or pubis. After the locking is accomplished, this pubic portion of the instrument is bent, and moulded upon the anterior portion of the pelvis, so as to fit it as exactly as possible. Formerly I was in the habit of attempting to keep the instrument more permanently fixed, by having elastic tapes fixed posteriorly into the apertures, *b*, and anteriorly into those still represented in the upper part of the pubic portion of the instrument; and both attached to a band encircling the trunk of the body. This arrangement I always now dispense with as unnecessary. If the pubic portion of the pessary is properly bent in over the pubis, it generally takes a sufficient hold; and if aught more is found necessary, all that is required, in addition, is, that the patient wear a common napkin, or understrap, to pass between the limbs and over the instrument. Latterly I have had the instrument made without the part, *b*, projecting posteriorly.

It is, perhaps, almost unnecessary to add that, if the instrument is found to press disagreeably on any part, it may be easily bent without removing it, so as to take off that pressure; and it may thus require to be remodelled and adjusted again and again in various parts, in order that it may be ultimately worn without annoyance and inconvenience.

When required to be withdrawn, the pubic portion is first bent back, so as to be clear of the pubis; then the vaginal pieces are unlocked; and, lastly, the internal part is extracted.

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[1] I have a patient at present wearing one of these pessaries with a stem three and a half inches long. The retroverted uterus is enlarged and elongated by fibrous tumors in its walls, and one of the ordinary pessaries did not suffice to hold it replaced.

The time during which it requires to be worn, in order to effect a cure, varies in different cases from one to two weeks to many months. The recent or chronic character of the case, and particularly the degree of tendency to the recurrence of the retroversion, are our principal guides on this head.

The employment of these uterine pessaries will cure many, but by no means all cases of retroversion of the unimpregnated uterus. And even when not sufficient to cure, they will generally relieve the patient, and palliate her annoyances and sufferings.

If the retroversion were causing no unpleasant symptoms nor interfering with the functions of the uterus, I have always recommended the avoidance instead of the adoption of local measures and remedies, either mechanical or others. When the reverse was the case, I have employed the means I have mentioned, with the effect of curing many and of relieving others. In the few exceptional cases in which no pessary could be borne, in consequence of the irritable state of the uterus, or where they fail to produce a cure, I have contented myself with reducing this morbid state of the parts by leeching, external counter-irritation, the application of belladonna ointment pessaries, &c., and rest, in the first instance, followed afterwards by the employment of local and general tonic measures. The patient often derives much relief from wearing an abdominal bandage, as those of Hamilton and Hull, with a supporting perinæal pad.

I have not entered into the consideration of the *Causes* of retroversion of the unimpregnated uterus. An explanation of these causes, and, indeed, of uterine displacement in general, is only to be found, I believe, in a complete study of the forms, relations, and functions of the different parts and layers of the pelvic fascia. In retroversion, those portions of this fascia which unite the back part of the uterus to the rectum and pelvic cavity behind, partially yield and give way. To restrengthen this support, and allow of its renovation, retention of the uterus for a length of time in the position and mode I have described often suffices. The simultaneous use of local injections and baths aid further the same view. But I allude here to the subject principally to observe that we may yet find further means of strengthening the weakened tissues, by indurating and contracting the upper and posterior portion of the vagina, as by the use of caustics, thus taking advantage of that contracting and strengthening power of the new tissue of cicatrices which burned surfaces particularly have. I think I have seen the application of nitric acid and potassa to the posterior lip of the uterus produce replacement.

BIBLIOGRAPHICAL NOTE.

In the fifth century Ætius treated definitely and at length, of retroversion of the unimpregnated uterus (Tetrabiblos, sermo iv. cap. 77); Moschion has left us some notices of it (Spachius' Gynæcia, p. 24); and in his work on female diseases, Roderic a Castro has some observations upon it (De Universâ Muliebrium Morborum Medicinâ, p. 274). In modern medical literature, the first individual case of retroversion of the unimpregnated uterus that was put upon record was reported by Saxtorph, in 1775 (Collectania Hauvniensia, vol. ii. p. 129). In 1779, Willich described a second case (Richter's Bibliothek, vol. v.) In 1786, in a woman who died of pulmonary disease, Peter Frank found the uterus greatly retroverted, and published a notice and drawing of the displacement (Opuscula Posthuma, p. 78). In 1787, Jahn noticed its occasional occurrence in the unimpregnated uterus, but considered it "too obscure and too unimportant to deserve the name of a disease" (Dissertatio de Utero Retroflexo, in Schlegel's Sylloge Operum Minorum Præstantiorum ad Artem Obstetricam, &c., p. 612). A few years subsequently, Schneider (Richter's Bibliothek, vol. xi.) and Kirschner (Stark's Archives, vol. iv.) published each a case of this affection. In 1817, Schweighauser of Strasburg pointed out that retroversion of the unimpregnated uterus was far from being rare, and he asserted it to occur much more frequently than in the gravid uterus (Aufsätze über einige Physiologische und Praktische Gegenstande der Geburtshülfe). In 1820, Schmitt published at Vienna an essay taking the same view (Ueber die Zurückbeugung der Gebärmutter bei Nichtschwangeren, &c.) Subsequently individual cases of retroversion of the unimpregnated uterus, or more general remarks regarding it, have been published by D'Outrepont (Zeitschrift für Geburtskunde, 1827), Denman (Introduction to the Practice of Midwifery, p. 138), Cooper (Anatomy and Surgical Treatment of Abdominal Hernia, part ii. p. 59, 1827), Robertson (Edinburgh Medical and Surgical Journal, 1822, vol. xviii. p. 520), Velpeau (Traité de l'Art des Accouchemens, vol. ii. p. 622), Bazin de Basseneville (Memoire sur la Retroversion de l'Uterus, 1837), Moreau (Traité Pratique des Accouchemens, vol. i. 1838), &c. &c. I have referred in a preceding page to the first observations which I published on the matter, in the Abstract of the Proceedings of the Medico-Chirurgical Society of Edinburgh. I shall extract these remarks from the Monthly Journal of Medical Science for July, 1843, p. 660: "As one of the most important of all these applications of the uterine bougie, Dr. Simpson demonstrated that, by showing the direction of the uterine cavity, and hence of the uterus itself, and by its enabling us, when it is introduced, to change at will the position of the organ, it afforded a simple means of detecting those displacements of the unimpregnated uterus known by the names of retroversion and retroflexion and anteversion and anteflexion—states, that Dr. Simpson further showed were very common, and which, from the want of proper means of diagnosis, had been almost constantly mistaken for fibrous, carcinomatous, and other tumors situated between the uterus and rectum, or between the uterus and bladder. In cases of retroflexion and anteflexion of the unimpregnated uterus, the organ can with facility be temporarily restored to its normal position and relations, by turning the uterine bougie, when used as a means of diagnosis. Dr. Simpson proposed to maintain and fix the bougie in the uterine cavity for a length of time as a means of cure. He laid before the Society a number of utero-vaginal pessaries, which had been used by him for this purpose. They were constructed of nickel and silver-wire, and had each attached to them a part which passed up to the fundus uteri. Some of them had been worn for weeks and months in the uterine cavity, and had produced little or no irritation." In 1846, Dr. Rigby published some excellent and accurate observations on the disease (Medical Times, p. 292, &c.); and more lately Dr. Protheroe Smith has written on the subject (Obstetric Record, p. 35, &c.); see, also, Mr. Hensley (Provincial Medical and Surgical Journal for January, 1848); and Dr. Beatty (Dublin Journal for November, 1847); &c.

Although the treatment of uterine displacements by intra-uterine pessaries seems to be generally known to the profession, it has by no means become universally adopted—great, and as experience proves, unnecessary fears being entertained as to the safety of introducing instruments into the uterine cavity. Still further obstacles seem to have been thrown in the way by the recent report of M. Depaul to the French Academy. For ourselves, we can only say, that though we have witnessed a great number of cases treated by these means in the practice of Dr. Simpson, as also in the Hôpital de la Pitié, under the care of M. Valleix, and have sometimes seen such irritation produced as to necessitate the withdrawal of the instrument, we have never observed a fatal result follow the introduction of an intra-uterine pessary. That such may follow, we are not prepared to deny; all we wish to insist upon is, that deleterious results in any form are the exception and not the rule; and we can testify that many patients, who for years had been wretched invalids, are now, from their employment, enjoying comparative health and comfort.—(Ed.)

ASCENT OF THE UNIMPREGNATED UTERUS
(ELEVATIO UTERI.)

(From Edinburgh Monthly Journal of Medical Science, October, 1852, p. 366.)

No subject in uterine pathology is better known to the practitioner than the displacement of the uterus downwards, or the so-called *Prolapsus Uteri.* But the occurrence of the ascent, or displacement of the uterus upwards, has been left comparatively unnoticed. In my observations on paracentesis, I have alluded to its elevation and position upwards in front of an enlarged ovarian tumor, as occasionally rendering it liable to be injured by the trocar of the surgeon. Voison has published a case in which the elevated uterus was fatally wounded from this cause, in the operation of tapping an ovarian dropsy.[1] I have seen several instances of multilocular dropsy of the ovary, in which the uterus was drawn up and elevated more or less in front of the ovarian tumor, but only very rarely so as to be beyond the reach of the finger. The same upward displacement, or *Elevatio Uteri*, occasionally happens in connection with the presence of fibrous uterine tumors. In cases of fibrous tumors, I have known this displacement so great that the os and cervix uteri could not be readily or at all reached by a vaginal examination with the first finger or fingers of the right hand, till the mass of the tumor was pressed downward, and, at the same time, somewhat rotated forwards upon its transverse axis by the left hand placed externally upon the abdomen. Within the last two weeks, I have met with an instance of this elevation of the uterus to a higher degree than I remember to have previously observed. The displacement in this instance has occurred in connection with the presence of several large fibrous tumors in the uterine parietes.

Case I.—The patient, aged forty, had been married twenty years, had borne one child about a year after marriage, but had never been again pregnant. The abdominal cavity is now distended by a mass of dense, firm, fibrous uterine tumors, which stretch upwards to nearly the scrobiculus cordis, and are altogether as large as the uterus in the eighth month of pregnancy. The mass consists of one great tumor stretching upwards above the umbilicus, and superadded to this are several comparatively smaller masses above and laterally, forming tuberose elevations and projections upon the sides of the larger central tumor. One of these smaller tumors, situated towards the left side, of a flattish form, and about two and a half inches broad, is pediculated and mobile, like a peritoneal polypus. On examination per vaginam, a rounded elongated portion of the largest tumor is felt pressing low down into the pelvis, and filling up the space in front of the rectum. The vagina, from the lowest point of the tumor upward, is narrowed and flattened between the tumor and the pubis; but the finger, when passed along this contracted canal, cannot reach the os or cervix uteri. In fact, a sound passed into this canal runs a considerable distance upward from the vulva before it touches the upper extremity of the vagina, and consequently before it reaches the cervical portion of the elevated uterus; and the end of the sound arrested at the junction of the vagina and uterus can be felt through the abdominal parietes *as high as two and a half inches above the edge of the symphysis pubis.* The body of the uterus can be felt as a flattened projecting mass above this part.

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[1] Recueil Periodique de Médecine, vol. vii. p. 362.

This patient, who has come for medical advice from Australia, was there considered to be laboring under ovarian dropsy; and it was supposed to be a case admitting of removal by operation. But that it is a fibrous tumor, and not a multilocular cyst of the ovary, is certain from its slow growth, from the density of the tissue of the tumor, from the arrangement alluded to of superadded external tumors, and from one of these superadded tumors having become pediculated like an external polypus—a morbid arrangement which we never see in ovarian disease. Besides, there is this common, and, as I believe, pathognomonic sign present, that in various parts of the larger tumor, and particularly on its right side, a loud souffle is heard synchronous with the pulse, when the stethoscope is used—a phenomenon very common in fibrous tumors imbedded in the substance of the parietes of the uterus, but which I have never met with in any instance of dropsical disease of the ovary. The affirmative evidence of the enlargement being a fibrous uterine tumor, which can be usually derived from the simultaneous movement and union of the mass of the tumor with the body and cervix uteri, under a combined abdominal and vaginal examination, is here of course wanting; in consequence of it being impossible to reach the elevated uterus. Nor, for the same reason, can we take advantage of those other means of diagnosis between fibrous tumors of the uterus and cystic degeneration of the ovary, which depend upon the respective measurements of the length of the cavity of the uterus in these two diseases, as ascertained by the employment of the uterine sound.

At present I have under my care, from Berwickshire, a case of very large fibrous tumor of the uterus, in which the os uteri is elevated above the pubis; though not to the great height mentioned in the preceding instance.

Case II.—The patient, æt. 38, has been married seventeen years, but has had no family. Menstruation has been regular; and only lately somewhat menorrhagic. Her general health is good. About years ago she first noticed an abdominal enlargement, which has gradually and slowly increased. The tumor is now of as great size as the uterus at the ninth month of pregnancy. It touches the lower edge of the ribs upon the right side. Its external form, however, is somewhat irregular, particularly from a large projection upon it towards the right iliac region. The tumor is not so firm in consistence as fibrous tumors generally are; and towards its centre, and near the umbilicus, it feels comparatively so soft as to give a deceitful idea of fluctuation, like that sometimes imparted by subcutaneous adipose tumors. Three months ago a trocar was passed in this situation, without drawing off any fluid whatever. There is a deep musical souffle, synchronous with the pulse, to be heard on the sides of the tumor, particularly on the left side. On making a vaginal examination, the os uteri cannot be reached, but a decreasing, conical-shaped cavity may be felt, passing up in front of the tumor, and somewhat towards the left side. When the patient is placed upon her face, and the finger deeply passed along and behind the symphysis pubis, the os uteri can with considerable difficulty be touched, lying above the upper edge of the pubis. The uterine bougie, when introduced into the os, passes readily for several inches, showing the uterus to be elongated and hypertrophied upon the side of the tumor. In this instance, there is no projection downwards of the tumor, as in the preceding case, into the pelvic excavation. The pelvic cavity is, in fact, quite free, and the roof of the vagina is altogether higher than usual.

It does not seem difficult to understand the mechanism by which the uterus becomes morbidly elevated in such cases of ovarian and uterine disease as I have alluded to in the preceding remarks. If an ovarian or fibrous tumor, attached organically to the back wall of the uterus, grow downward upon the roof of the vagina, or, in other words, into the reflection of peritoneum between the rectum and uterus, and develope itself steadily in this its *lower* segment, the extension of the tumor in this downward direction, upon the resisting roof of the vagina, forces the tumor to lift the uterus, which is attached to the anterior surface or body of the tumor, higher and higher with it during the longitudinal development of the mass to which it is united. The tumor, in its downward longitudinal development, necessarily carries upward more and more the uterus affixed to its anterior part; in the same way as the uterus, in its own enlargement during pregnancy carries and elevates upwards the Fallopian tubes and ovaries attached to its two sides. Or the enlarging uterine or ovarian tumor may, as in the second case detailed above, obtain a similar elevating influence upon the uterus, by resting its lower and growing segment upon the pubis or sides of the brim of the pelvis, instead of upon the roof of the vagina; thus ultimately displacing the uterus upwards by somewhat the same kind of mechanism, as the os and cervix uteri are often, in common pregnancy, raised upwards and backwards above their usual level for some time after the uterus expands into the cavity of the abdomen, and rests, during its enlargement, upon the anterior circle of the pelvis.

Morbid ascent or elevation of the uterus sometimes occurs in connection with other morbid states besides the two mentioned—ovarian dropsy and fibrous uterine tumors. Occasionally, indeed, the cervix uteri, and, consequently, the whole organ, is found placed at an unusual height from the vaginal orifice as a natural conformation; and in advanced life, when the uterus becomes atrophied, it sometimes is situated higher than natural, with the vagina drawn upwards in a funnel shape. I have seen it displaced upwards from diseased action in connection with, and as a result of, pelvic cellulitis or pelvic abscess; as well as from the effects of simple pelvic peritoneal inflammation and adhesions (*Perimetritis*).

GOUT OF THE UTERUS.

(From Edinburgh Medical and Surgical Journal, January, 1854, p. 16.)

I HAVE seen several cases of inflammation of the uterus, or rather of the uterine region, of the nature of simple gout, or rheumatic gout; or, at all events, I believed these inflammatory attacks to be of this special pathological nature, in consequence of their co-existing with, or following immediately upon, the presence of undoubted gout in other parts of the system.

In one instance, which I had the pleasure of seeing repeatedly with Dr. Begbie last summer, the attack of uterine gout came on during the existence of a marked prolonged fit of the same disease in the fingers, &c. The principal organic effect, as ascertained by vaginal examination, was effusion into the cellular tissue of the broad ligaments (pelvic cellulitis), giving rise to tumefactions of considerable size around the sides of the uterus. This pelvic effusion was gradually absorbed under some local antiphlogistic means, and the use internally of colchicum and other medicines.

About the same period I visited repeatedly, at some distance from town, a lady, hereditarily predisposed to gout, who, after some prolonged constitutional derangement, was seized with excruciating pain followed by redness and swelling, in and around the right ankle. The agony attendant on this attack was so great, that she could not move the limb in the least for two or three weeks. There were large quantities of oxalates in the urine. After recovery, however, did begin, convalescence was very rapid for two or three weeks; when she travelled home to her own residence, a journey of nearly a hundred miles. Shortly after arriving, she was seized with severe pelvic pain; and two days subsequently there was a large inflammatory swelling in the right broad ligament, and at the same time much effusion of serum into the cellular tissue of the upper portion of the posterior wall of the vagina. This last formed a tumor like an orange, or larger, projecting backwards into the rectum. Under leeches and mercury these swellings subsided, but with a rapidity seldom or never seen in common cases of similar inflammatory swellings attendant upon common pelvic cellulitis.

In the first case of uterine gout which I ever saw, now some years ago, the lady suffered repeatedly and metastatically in the uterus, simultaneously with, and sometimes after, the appearance of the disease in the extremities. The uterus was very large; was fixed as if there had been perimetritis; and in the attacks which I watched, it appeared to me that the wall of the organ and the peritoneal surface were usually the seats of the morbid action. In her there was no effusion into the neighboring cellular tissue.

In none of the instances of gout of the uterus which I have watched, have I seen the inflammatory action attack the neck of the uterus, or the mucous membrane of the organ. Rheumatic inflammation seems to do so.

We have extracted the preceding remarks from a paper by Dr. Begbie upon Gout and the Gouty Diathesis, to which they had been contributed by Dr. Simpson.—(Ed).

ON VESICO-UTERINE, VESICO-INTESTINAL, AND UTERO-INTESTINAL FISTULÆ,

AS RESULTS OF PELVIC ABSCESS.

(From Edinburgh Monthly Journal of Medical Science, December, 1852, p. 532.)

Cellular tissue exists abundantly within the pelvis, along the lining of the walls of the pelvic cavity, within the layers of the broad ligament, and between the intestinal, genital, and urinary canals, at all those points in which they come into organic coherence. Inflammation of this tissue, or Pelvic Cellulitis, is a common affection, particularly as a consequence of parturition, &c. Pelvic Cellulitis, after giving rise to great swelling and induration, by the effusion of serum, coagulable lymph, &c., into the inflamed portion of the cellular tissue, very often terminates sooner or later in resolution;—the disease not unfrequently assuming a subacute or chronic type. In other instances, however, the inflammatory action runs on towards suppuration, and forms a so-called Pelvic Abscess. When this termination occurs, the collected purulent matter is found to obtain egress by different outlets. The abscess sometimes bursts into the intestinal canal, or into the genital canal, or into the urinary bladder. Occasionally it discharges externally upon the cutaneous surface; and in a few rare instances it opens into the cavity of the peritoneum. Sometimes the collected pus is found to make its escape simultaneously, or rather consecutively, by *two* different exits. Thus we may have the cavity of the same abscess opening into two different pelvic mucous canals. Where such *double* perforations, originating in the escape of matter from a pelvic abscess, become chronic in their character, they lead to the formation of several species of deep pelvic fistulæ, which have not, so far as I am aware, been hitherto described by obstetric pathologists.

Very few instances of the existence of fistulæ of any kind between the bladder and uterus have hitherto been put upon record. Indeed, the number of *vesico-uterine* fistulæ hitherto recorded seems to be limited to three instances, reported severally by Mad. Lachapelle, Professor Stoltz, and M. Jobert; and in all of these three cases, the perforation which existed between the bladder and cavity of the neck of the uterus was the result of injury during parturition.

In the following case this rare form of lesion was produced as a consequence of pelvic cellulitis; or, to speak more definitely, it was produced by a purulent collection formed in the cellular tissue lying between the bladder and the neck of the uterus, and which ultimately ruptured—on one side into the bladder, and on the other side into the cavity of the uterus, or rather the cavity of the cervix uteri.

CASE I.—The patient, aged 22, and the mother of two children, was admitted into the female ward of the Royal Infirmary in June last. Her youngest child was then eleven months old; and she had made a perfect recovery after her confinement with it. About six months, however, subsequently to her delivery, she was seized with local pelvic pain, dysuria, and the usual symptoms of pelvic cellular inflammation. Three or four weeks after the commencement of this attack she had shiverings and perspirations, and other symptoms of hectic fever. These symptoms were shortly followed by evidence of the escape of purulent matter; and subsequently complete incontinence of urine came on. After this the urine continued to be discharged per vaginam up to the date of her admission into the hospital, four months after the commencement of the inflammatory symptoms. The urine contained a considerable quantity of pus. On examination, the urethra was found perfectly patent, although the urinary secretion was not discharged through it. There was still a considerable mass of fixed inflammatory deposit in front of the cervix uteri, or in the cellular tissue between it and the posterior wall of the bladder. The cervix uteri itself was considerably hypertrophied, particularly its anterior lip. That the urine passed from the bladder through the os and cervix uteri, was ascertained by the simple experiment of filling up the os uteri for a day with a small sponge tent. During the time the cavity of the os uteri was stopped up with this plug, the urinary discharge was evacuated through the urethra; but immediately began again, and continued, to pass through the artificial vesico-uterine opening as soon as the sponge-plug was withdrawn. After withdrawing the plug, the cervical cavity, which had been dilated by its presence, was examined by the finger, and two apertures were found passing into it, or rather leading from it—one, the normal aperture leading upwards into the cavity of the uterus, as ascertained by the uterine sound—the other, tending obliquely forwards towards the cavity of the bladder. This latter artificial opening was freely cauterized by solid nitrate of silver. Subsequently, local and general measures were employed, as external counter-irritation, iodine, &c., to promote the absorption of the inflammatory deposit. In the course of a few weeks the swelling from the deposit between the bladder and the cervix uteri diminished, the incontinence of urine became gradually lessened, and was ultimately totally arrested; the cure being, as I believe, the result of the natural contraction of the parts, following upon the absorption of the original inflammatory deposit. Subsequently this poor patient was attacked with symptoms of acute pulmonary phthisis, and died a short time ago at a distance in the country, but without any return whatever of the incontinence of urine.

I have seen other cases of pelvic cellulitis which had run on to suppuration, leave other forms of fistulous perforation, perhaps still more strange and singular in their anatomical relations.

Two years ago, I had under my care, a case in which there was produced as the result of this disease, a *utero-intestinal* fistula.

CASE II.—A lady, a few days after her first confinement, was attacked with symptoms of fever, and local pelvic inflammation. These terminated in a very tedious pelvic abscess. About a year subsequent to her accouchement, she was brought to Edinburgh, and placed for some time under my care. She still had considerable thickening and pain on pressure in the left side of the pelvis, which had been the seat of the pelvic cellulitis. The cervix uteri, and indeed the whole uterus, was elevated upwards, and drawn considerably to the same side. On examining simultaneously, with the fingers of the right hand on the roof of the vagina, and with those of the left hand placed externally over the left iliac region, much thickening and agglutination of the uterus and intestines could be readily ascertained in the left pelvic and iliac regions. Discharges of slight accumulations of pus recurred from time to time through the os uteri; and, occasionally, after these discharges, small quantities of feculent matter were found in the vagina—showing a communication to exist between the intestinal canal, at some part, perhaps the sigmoid flexure, and the cavity of the uterus. When the canal of the cervix uteri was gently examined by a slender probe or sound, a fistulous communication could be traced, passing up from the cavity of the cervix laterally towards the left iliac region; but this sinus could not be followed for any great length.

In this case there was a constant tendency to the recurrence of inflammatory attacks in the original seat of the pelvic inflammation, some of which were extremely severe. The patient subsequently removed to the south of England, where she died under, I believe, one of these renewed inflammatory attacks. My friend, Professor Lawrie of Glasgow, saw this case repeatedly.

Some time since, I was consulted by a patient, in whom there existed a still more rare and curious form of fistula, the result of a pelvic abscess under which she had suffered. Professor Macfarlane of Glasgow, and Dr. Miller of London, also saw the case. Its peculiarity consisted in this—that, as a result of pelvic abscess, a fistula was formed between the bladder and rectum—a *recto-vesical* fistula—and yet the intermediate uterine and vaginal canals were not implicated in it. The principal particulars regarding the case are as follow:

CASE III.—The patient, when about twenty-three years of age, and unmarried, was attacked with fever and severe local inflammatory pain in the pelvis and left groin. After these symptoms had continued for some weeks, she was at length relieved by the discharge of a large quantity of purulent matter from the rectum.

About twelve months after the occurrence of this pelvic abscess, she was considered so well as to be allowed to be married. But from that time she suffered from repeated attacks of pelvic irritation and inflammation, with leucorrhœa, irregular menstruation, &c. She never became pregnant. Several years subsequent to marriage, during one of these recurrent pelvic attacks, the bladder became greatly irritated; and after this painful dysuria had lasted for some time, purulent matter was discharged along with the urine. Subsequently to this period, and on to the time of her death, four years afterwards, small portions of feculent matter and flatus passed from time to time by the urethra, along with the urine—showing a communication to exist between the intestinal and urinary canals. As high up the rectum as could be reached with the finger, a fistulous opening was traceable in the anterior and lateral part of the bowel, and a probe could be passed forwards to some extent through it. There was much thickening and agglutination of the pelvic tissues at that part. No treatment was of any avail in relieving the patient from her distressing symptoms. She died ultimately of a short illness, from, as reported to me by Dr. Miller, some affection of the brain.

In the preceding and in other cases of pelvic abscess, the different openings through which the matter becomes discharged do not, as I have already remarked, always occur simultaneously, but generally consecutively. After the purulent matter has escaped, apparently with sufficient freedom, by one opening, it will occasionally, in consequence either of its temporary obstruction and retention in the sac of the abscess, or in consequence of the walls of the abscess themselves ulcerating, again open at a subsequent period into another canal. The following case, which I had occasion to see often under the kind and able care of Dr. Johnson of Stirling, will serve to illustrate this remark:

Case IV.—A patient in the country was seized with acute symptoms of pelvic cellulitis; and a large inflammatory tumor speedily formed in the cellular tissue of the left broad ligament, and in the left iliac fossa. In despite of the active antiphlogistic treatment that was adopted the disease ran on towards suppuration, and dangerous symptoms of irritative fever and exhaustion supervened, with great local tenderness in the affected part. An exploring needle, introduced by the spine of the ilium into the inflammatory swelling, lying in the iliac fossa, showed the presence of a deep collection of pus. I evacuated it twice by a trocar, with great relief to the patient. Pus continued to be freely discharged for a considerable time through this artificial opening on the edge of the left iliac fossa. After this discharge had gone on for some weeks, the opening from time to time offering to become nearly closed, a new and distressing symptom supervened. For, along with a discharge of matter through the artificial opening, there passed from time to time some flatus, and occasionally a slight appearance of feculent matter—showing that the abscess had ulcerated into the bowel, probably the sigmoid flexure of the colon.

In this instance, the spontaneous opening of the abscess into the intestinal canal did not, as I have said, occur till weeks subsequent to the discharge of the matter through the external opening. After this occurrence, the external fistula was attempted to be shut up by various means, but for some time without avail. At last, by altering the position of the external orifice, by a new incision, the whole external aperture was happily obliterated, and the patient has made a perfect recovery.

In the preceding cases, the fistulous communications resulting from the pelvic abscess were probably valvular in their form, inasmuch as apparently the fluids and air escaped through the track of the fistula only occasionally, and not constantly. Perhaps, in some, the transit of these matters was only effected when the canal of the fistula was preternaturally distended by an accumulation within and behind it of the morbid matters that passed; the walls of the canal at other times being so much in contact as not to allow of the passage of any foreign body.

In the case of vesico-intestinal fistula which I have mentioned—(see Case III.) feculent matter and flatus appeared to pass from the bowel into the bladder; but the patient was never aware of the urine passing from the bladder backward into the bowel. It is quite possible, however, that it may occasionally have done so in small quantity, without her being able to recognize it distinctly. If it were not so, then, in that case, the fistulous communication was of such a valvular structure, as to allow of the transit of matters in one direction, and not in another—a point in its anatomy, which it is not difficult to conceive.

Since the preceding notes were written, I have received a communication from an old pupil, Dr. Heslop, directing my attention to a case published by him,[1] in which the left ovary was found after death of the size of an orange, adhering intimately to the rectum and bladder. The cavity of the enlarged ovary contained "a soft, pultaceous, half-fæcal, half-caseous-looking matter;" and communicated posteriorly with the lower portion of the sigmoid flexure of the colon, and anteriorly with the urinary bladder. In this case a fistulous communication seemed to be formed between the intestinal and urinary canals, through the cavity of this diseased ovary; and air, and probably feculent matter, are said to have passed occasionally from the bladder for twelve months before death. The patient had, in addition, a calculus in the left kidney, enlargement of the liver, &c.

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[1] Dublin Quarterly Journal for 1850, p. 220.

Latterly Dr. Simpson has treated several of these cases of pelvic fistulæ successfully, by repeated and frequent injections into their tracts of strong tincture of iodine. We have seen one patient who had a complex fistula opening into the uterine cavity, attended with very fetid discharges, quite cured in this way. Dr. Simpson has used injections of tincture of iodine also as a means of *diagnosis* in such cases. In a patient who had come over from Holland two years ago to be under his care, the iodine injected into an external fistulous opening in the upper part of the thigh passed directly into the rectum—showing at once the course of the fistula. We have seen him also occasionally make counter-openings below, and thus effect a cure of the fistula. In one case of pelvic fistula of twelve years' standing, the patient has perfectly recovered under this treatment.—(Ed.)

ON THE POSITION OF THE PATIENT FOR PARACENTESIS IN OVARIAN DROPSY,

AND ON THE PLACE OF PUNCTURE.[1]

(From Edinburgh Monthly Journal of Medical Science, October, 1852, p. 366.)

When paracentesis is resorted to for the relief of ovarian dropsy, the method in which the operation is performed among us is generally, if not invariably, the following:—The patient is seated more or less upright upon the edge of the bed, or upon a chair—a broad bandage is placed around the lower part of the trunk, and the two ends of it, crossed behind, are each intrusted to an assistant, with the view of pulling and tightening it as an abdominal compress, in proportion as the fluid is evacuated—and a small opening is cut in the anterior part of the bandage, as an aperture by which a trocar of sufficient size is introduced through the abdominal parietes into the ovarian cyst.

The bandage or compress encircling the trunk is generally alleged to be used with two objects: *first*, To aid by abdominal pressure in the evacuation of the ovarian fluid; and *secondly*, To secure the patient against the chances of faintness and syncope upon the sudden withdrawal of such a large quantity of fluid from a person seated in the upright position. Different kinds of compressing bandages, as linen, flannel, caoutchouc cloth, &c. &c., have been recommended to be employed; and the ends of them are sometimes slit or perforated, with the view both of making the compression exerted by them more complete, and of allowing the bandage to be run and applied more easily and accurately in proportion as the abdomen diminishes in size, during the evacuation of the contained fluid through the trocar. I have seen the operation of paracentesis in ovarian dropsy performed often, and by many different hands; but I have rarely seen the bandage run correctly, even when in the first instance it was most carefully applied, and afterwards as carefully pulled by the two assistants in charge of the two ends of it. As the fluid is gradually evacuated, the bandage, in fact, is ever liable to lose its proper adaptation to the diminishing shape of the abdomen; or it becomes oblique in its position, and, pressing upon the canula of the trocar, threatens to displace it; or, as sometimes happens, it rolls up, and strongly compresses one circle or portion of the abdomen, leaving the other parts of the abdominal tumor comparatively uncompressed and unsupported. Besides, any one who has acted as an assistant in a case of tedious tapping, knows that the mere pulling at the end of the bandage is a task sufficiently irksome and fatiguing.

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[1] Read before Edinburgh Obstetric Society, Session XII. 1853.

Latterly, when performing the operation of tapping in ovarian dropsy, I have placed the patient in the horizontal position, and dispensed entirely with the use of the compressing bandage during the operation, with I think, no small advantage and relief to the patient, to myself, and to my assistants. If the patient be placed in the supine position, there need be no dread of the tendency to fainting and syncope, for the prevention of which the bandage is specially recommended; and at the same time, the contents of the dropsical cyst or cysts, are, it appears to me, even more easily, and certainly more completely, evacuated, than when the operation is performed with the patient placed in the upright position.

In tapping an ovarian cyst in the horizontal position, the patient requires to lie as near the front of her bed or couch as possible, with the face of course turned towards it, and indeed, with the distended ovarian sac projecting, if possible, over the edge of the bed. If it is previously and accurately known that it is the right or left ovary which is affected, and that there is a prospect of the tumor being very completely evacuated of its contents by the operation, the patient should lie upon the right side if it be the right ovary, and upon the left side if it be the left ovary that is diseased. The trocar is then introduced into the distended cyst at the usual part in the abdominal parietes, and with the usual precautions. Towards the termination of the operation, the most complete evacuation of the cyst may be secured when necessary, by turning the patient somewhat upon her breast, so as to make the puncture as dependent as possible—and, when necessary by compressing the abdominal parietes with the hands—the latter a proceeding which requires to be followed in most cases of paracentesis in the upright position. One great danger attendant upon tapping an ovarian cyst consists in the liability of air to go backwards into the emptied cavity towards the latter part of the operation, when the stream through the trocar becomes imperfect, or actually intermits, in consequence of the irregularity of the abdominal compression. I believe that this accident, and its consequences, inflammation of the walls of the cyst from decomposition of its remaining contents, will be found to be much less liable to occur when the patient is tapped in the horizontal position, and the parietes of the abdomen are allowed to compress the cyst merely by their own elasticity, and by the external pressure of the atmosphere, than when the operation is performed with the patient sitting upright, and with the compression in a special degree intrusted to the proper adaptation and action of a bandage, the mechanism of which is not easily regulated. Let me further add, that the preparations for the operation are far less formidable to the patient when the tapping is performed in a horizontal position, and without bandages; and, besides, the necessity of assistants to manage the bandage is dispensed with. The compression itself of the bandage amounts to a feeling of distress and suffering with some patients; and I have been strongly assured by those who have been tapped at different times in both ways, that the absence of the bandage, combined with the horizontal position, were great advantages to them, as far as their feelings and comfort were concerned. It is almost unnecessary to add, that the edge of the bed requires to be protected by several plies of sheeting or towels; and, if deemed necessary, a bandage may be placed round the abdomen, when the patient lies back in bed, subsequent to the operation, particularly if the abdominal parietes are greatly relaxed, and the remaining mass of ovarian tumor feels very loose and mobile in the cavity of the abdomen. Sometimes, however, I have dispensed even with this secondary bandage.

In performing paracentesis for the evacuation of an ovarian cyst, the trocar is usually introduced in one of two situations, viz., either centrally, in the course of the *linea alba;* or laterally, in the course of the *linea semilunaris.* It perhaps is of little moment which of these two places is adopted, provided only the trocar is introduced in a situation in which the fluctuation is very distinct, and the parietes of the cyst thin and equal, and in which, therefore, the instrument easily reaches the cavity of the cyst.

In selecting the place of the puncture, it must be held in remembrance, that various causes may render the introduction of the trocar at particular sites difficult and dangerous; and that the presence of one or other of these causes should induce us to select another situation. For example:—*First*, The chance of wounding the urinary bladder must be avoided. The evacuation of the organ immediately before the operation, is our best security against this. *Secondly*, The uterus is sometimes elevated and drawn upwards in front of an ovarian tumor, and has been fatally wounded by the trocar in the operation of paracentesis. This ascent and displacement of the uterus can be ascertained before the operation; and all chance of injuring it would be avoided, if, as I have already stated, a point in the cyst sufficiently fluctuating and thin in its parietes, be selected as the site of the puncture. *Thirdly*, Ovarian cysts have been occasionally found so turned upon their axes, that the elongated Fallopian tube has stretched across the diseased ovary, and interfered with the introduction of the trocar; and a dense, fibrous state of the cyst at particular parts has led to the same mischance—the cyst thus becoming merely displaced and not perforated by the pressure of the point of the instrument. A case of obstruction to tapping from this cause is detailed by Dr. Bright, in the Guy's Hospital Reports. The puncture, in consequence, must not be made over a point which feels unequal and condensed in its structure. *Fourthly*, In the later stages of ovarian dropsy, the tumor often so compresses the contents and parietes of the abdomen, that the circulation of the blood through the abdominal portion of the vena cava is much interfered with. I have twice, in dissection of ovarian dropsy, seen the cavity of the vena cava obstructed from this cause. In consequence, a *vicarious* venous circulation is set up in many cases through the superficial veins of the abdominal parietes which hence become greatly enlarged. These veins are often seen of the size of goose-quills or larger, and running immediately beneath the skin. In paracentesis, the wounding of one of these large veins with the trocar must be carefully avoided. *Lastly*, The epigastric artery has been opened by the instrument in ovarian paracentesis. It is on this account that some authors have advised us to select the linea alba in preference to the linea semilunaris, as the site of puncture. Mr. South relates a case, however, in which this artery was fatally wounded when the tapping was made in the first of these sites—the linea alba. Common care, and a little examination beforehand for the seat of a pulsating artery in the thin and distended abdominal parietes, should enable us to avoid this source of danger. And if we avoid this difficulty, and at the same time select as our proper site for the operation, the part where the fluctuation is most distinct, and the parietes of the sac most thin and equable, it matters not whether that be in the course of the linea alba or of the linea semilunaris. The latter is perhaps the best, because the most dependent site, if we have our patient lying, during the operation, in the horizontal position.

It has been suggested by Callisen, Macarn, Delpech, Recamier, Arnott, and others, that dropsical ovaries should be tapped through the roof of the vagina in preference to the abdominal walls. If the so-called ovarian cyst is unilocular, its contents may certainly be evacuated by this means, as well as by tapping through the parietes of the abdomen; and I have more than once evacuated the contents of a dropsy of the Fallopian tube, by introducing the small trocar, which forms the usual exploring needle, in this position. In one of these cases the elongated sac formed by the distended Fallopian tube inflamed after its evacuation, and, in consequence, seemed, to become entirely obliterated. The patient, a lady from New York, had previously been almost incapacitated from taking exercise, and had been in bad health for several years. Since her return home she has been pregnant, and borne a dead child. But it is excessively rare that a true ovarian dropsy, so distended as to require tapping, is unilocular. In forty-nine cases out of fifty, or perhaps in a larger proportion, the enlarged ovarian dropsy requiring the operation of paracentesis, consists of the multilocular form of degeneration of the organ. And in this compound or multilocular cystic dropsy of the ovary, paracentesis by the vagina can very seldom readily or safely evacuate the contents of the diseased mass. For, in the operation of paracentesis in the common multilocular form of ovarian dropsy, we evacuate merely the contents of the *largest* cell or cells in the mass; and we reach this largest cell or cells easily through the abdomen, but cannot usually reach them readily through the vagina. This important circumstance depends upon a simple pathological law, which has not been adverted to, as far as I know, in any of the numerous essays or observations, which have been published on ovarian disease. Cystic tumors of the ovary, like other morbid tumors and collections, increase always most readily and rapidly towards that direction in which there exists the least physical resistance to their growth; and on the other hand, most slowly and imperfectly towards that side or sides on which they meet with most opposition to their mechanical development and increase of size. Ovarian tumors do not usually grow readily, or largely, towards their inferior or pelvic sides, because the resistance of the floor of the abdomen and pelvic parietes offers in that direction sufficient impediment to their development. But they increase and develope readily in an upward direction towards the abdomen, because in that direction they meet with comparatively little resistance or opposition to their growth. And while the cells in the pelvic portion of the remaining multilocular tumor are, as far as I have examined them in a considerable number of preparations and dissections, usually very small in size, however great in number—we have on the contrary, in consequence of the above pathological law, the largest cyst or cysts in the mass generally, if not always, placed, first, at the upper or abdominal extremity of the tumor—and, secondly, on the anterior part of the abdominal tumor, rather than on its lateral or posterior parts—the cyst or cysts in front growing more readily, because they are less resisted in their growth by the abdominal parietes in front, than the cyst or cysts placed towards the sides or back of the tumor, inasmuch as these latter are repressed by the denser fabric of the lateral and posterior walls of the abdominal cavity of the patient. It is, I repeat in consequence of this pathological arrangement, that, fortunately, by the operation of abdominal paracentesis, we are usually able to evacuate the largest cyst or cysts in the mass; and in consonance also with the same law, the contents of such more prominent cyst or cysts are usually far more fluid, and hence more easily capable of being evacuated through the trocar, than are the contents of the more condensed and undeveloped cysts of the tumor. In fact, the larger and more anterior cyst or cysts have often their contents sufficiently fluid for evacuation by paracentesis, at the very time that the more undeveloped and more compressed cysts still contain a thick gelatiniform matter, quite incapable of being evacuated through any trocar.

It is almost unnecessary to add, that the horizontal position of the patient answers as well for paracentesis in ascites as for paracentesis in ovarian dropsy. It sometimes happens that the two are combined in a greater or less degree. A few days ago I saw, with Mr. Goodsir, a case of this combination, viz., a very large multilocular ovarian tumor floating in, and surrounded by a quantity of ascitic fluid. On placing the patient in a horizontal position, the ovarian cyst was first evacuated of its contents, which were of a very dark brown color, and mixed with old-effused blood. After the ovarian cyst was completely emptied, a second puncture of the abdominal parietes was necessary, while the patient was still lying in the horizontal position, to remove the collection of ascitic fluid.

PLASTER-BELT IN ABDOMINAL TUMORS, ETC.[1]

(From Edinburgh Monthly Journal of Medical Science, June, 1848, p. 887.

Dr. Simpson stated, that patients affected with pediculated ovarian tumors, large fibrous tumors of the uterus, &c., often suffered from the morbid masses being loose and mobile, and impinging on the bladder, &c., in different positions of the body. Patients sometimes instinctively applied their hands to the tumors, under such circumstances, to steady and fix them. In these cases different means had been tried, with the view of preventing the tumors rolling and moving—such as various bandages, air pads of Mackintosh cloth, &c. The best and simplest means, however, consisted in surrounding the whole trunk with a continuous belt of lambskin or chamois leather, eight or ten inches deep, and shaped and sewed so as carefully and exactly to fit the loins and lower part of the abdomen of the patient, like a common abdominal bandage, and embossed in front so as to contain and include, as in a bowl or cup, the protuberant portion or portions of the tumor. To fix the belt, its interior was spread with a plaster composed of one part of adhesive to two parts of soap plaster. It generally gave the patient much relief; abated the feeling of abdominal weight and pressure and pain in the back; held the tumor steady; and could be applied so as even to compress it. In other cases where no tumors were present, but the abdominal parietes and contents were relaxed, or the spine weak, the same form of plaster often afforded a great degree of comfort and relief, and enabled patients to take exercise, &c., when, otherwise, they could not without fatigue and suffering. They generally required to be removed and renewed every four or six weeks.

ON INFLAMMATORY AND NON-INFLAMMATORY RUPTURES OF OVARIAN DROPSICAL CYSTS.

(From Edinburgh Monthly Journal of Medical Science, December, 1852, p. 52.)

The common multilocular dropsy of the ovary may terminate fatally in various ways when left altogether uninterfered with by art.

In some cases, when the tumor at last reaches those enormous dimensions which it sometimes acquires, the mere strong compression laterally, backwards, and forwards, of the diseased mass upon the various abdominal viscera, vessels, and walls, and upwards upon the diaphragm and thoracic organs, proves sufficient in itself to lead gradually on to a fatal termination, preceded by marasmus, exhaustion, dyspnœa, &c. In such instances there is a slow but increasing physical clog set to the machinery of various organs that are necessary to the continuance of life—more particularly to the processes of nutrition and assimilation; but latterly, even respiration and circulation come to be more and more interfered with: till at last the impaired and obstructed mechanism of the body is fatally arrested. Very often, however, before such a termination occurs, œdema, particularly of the lower extremities, and ascites, come to be superadded, hastening the fatal result by their presence; and almost always it is also hurried onward by the supervention, during the last stages of the disease, of a greater or less amount of irritative fever.

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[1] Extracted from Proceedings of Edinburgh Obstetric Society, March 12, 1848.

But patients suffering under dropsy of the ovary do not always die from the mere mechanical increase of the tumor, and its mere mechanical pressure and irritation. Much more frequently *inflammatory action* attacks the walls or dissepiments of the diseased mass during the latter stages of its growth, and expedites the progress of the malady towards a fatal termination. Occasionally the inflammatory action recurs from time to time in the same cyst or in different cysts, accompanied with fever and local pains, generally of a slight and obscure kind. Under repetitions of such attacks, the cysts rapidly increase in size, inflammatory effusions being poured into their cavities or deposited upon their lining-membranes; and sometimes the intervening septa and walls of the tumor become diseased and broken down under them. Its external or peritoneal surface is frequently also the seat of inflammatory effusions, and of consequent local adhesions between it and the neighboring viscera and surfaces. One or more cysts, left with purulent effusions in their cavities, or with the structure of their walls disintegrated, often enough remain as permanent sources of local and constantly recurrent inflammatory action in the tumor. Hectic fever generally comes to be set up in the system as a consequence; and under the repeated recurrence of such local and constitutional irritation, the powers of life gradually give way.

Again, occasionally, when inflammatory action, whether acute or subacute in its course, is present in an ovarian multilocular tumor, it proves fearfully more rapid in its course, and leads to speedy death by a different mode. For when the tumor, in one or more of its cysts, is the seat of acute inflammation, such cyst or cysts sometimes become so over-distended by inflammatory effusions, as ultimately to rupture, and allow of the escape of their contents into the peritoneum. In these cases the walls of the over-dilated cysts are occasionally rendered friable and lacerable in their structure by the inflammatory action of which they are the seat, and in consequence of this morbid softening of tissues, the rupture in question the more readily occurs. In one or two cases, I have seen the walls of the inflamed and ruptured cyst present ulcerations upon its interior, the perforation of the cyst in such instances being begun by ulceration of the lining membrane and tissues of the cyst, and perforated by mechanical laceration of its exterior or peritoneal coat.

During the last two months I have met with two cases of rapid death from the rupture of inflamed multilocular tumors of the ovary, and the consequent effusion of their morbid contents into the cavity of the abdomen. The first of these cases occurred in a patient under my care in the ward set aside for female diseases in the Royal Infirmary. She had been previously under the care of Dr. Brown of Carronshore.

Case 1.—Mary W—, æt. 34, married, was admitted October 6, 1852. She had borne three children, the youngest of them now four years old. In May last, she first observed a small rounded tumor in the lower part of the abdomen. The tumor was then of firm consistence, painful on pressure, and slightly movable. At the date of admission, the abdominal tumor was nearly as large as the uterus at the full term of utero-gestation, and unequal and bosselated on its surface. It was tense and painful under pressure, and a distinct fluctuation could be traced in it. The os uteri was situated very high in the pelvis (*elevatio uteri*), and the vagina was much narrowed and stretched at its upper extremity.

During the week subsequent to her admission, the patient was merely confined to bed for the sake of rest, and placed under some simple antiphlogistic treatment. On the morning of October 13th, seven days after she entered the hospital, she was suddenly attacked with severe pain in the right hypochondriac region, preceded by a feeling as if something had burst in the abdomen. This pain increased in intensity during the forenoon, with great and general tenderness of the abdomen on pressure. In the after part of the day, nausea and vomiting supervened, and the pulse rose in frequency and sank in strength.

On the following day, at the time of visit, she expressed herself, however, much easier; the abdomen was tense, and increased in size, but scarcely now painful on pressure. The face, however, was very pinched and anxious, the surface cold, and the pulse so small, that it could not be counted, and she was altogether so collapsed that there seemed little hope of her surviving the attack. That night, however, and during the subsequent day, she rallied considerably. The pulse became stronger and reduced in frequency, and she improved gradually for the next fifty or sixty hours. Five days, however, after this first rupture and attack of peritonitis another and more fatal one supervened. For, early in the morning of the 18th, when attempting to raise herself in bed, she was suddenly seized with great renewed pain in the epigastrium, followed by vomiting of greenish-colored fluid, extreme coldness of the surface, and other symptoms of sinking and collapse. The pulse became imperceptible; and she died within twelve hours.

On a post-mortem examination, the abdomen was found to contain two or three quarts of turbid serum, while thick layers of unorganized and recently secreted coagulable lymph covered the peritoneal surface of the ovarian tumor and the more exposed parts of the abdominal viscera. At some parts, this layer of lymph was fully an inch in thickness, but quite soft and easily broken down. The left ovary was small and slightly indurated. The enormous ovarian tumor which was present, was attached to the site of the right ovary by a pedicle measuring about two fingers in breadth and thickness.

On removing the tumor out of the abdomen, a quantity of dirty, fetid, purulent fluid made its escape from an opening at the upper and back part of its right side. The aperture was of such size as to admit the forefinger. Its edges were ragged and ulcerated; and it exactly corresponded in position to the seat of pain, when the symptoms of sinking first appeared. On cutting into the cyst with which the opening communicated, it was found to contain upwards of a pint of fluid such as has been described. The wall of the cyst, for about an inch around the point of rupture, was much thinned and softened, and presented a black color.

The remaining portion of the lining membrane of the cyst was covered with patches of recent lymph, and red spots, having the granulated appearance of the intestinal canal in acute dysentery. At the lower and anterior part of the left side of the ovarian tumor, there existed another largish cyst, presenting similar characters when cut into. Several of the smaller cysts of the tumor showed strong signs of recent acute inflammatory action; and pus oozed out in various parts when the large multilocular mass was bisected. The intervening septa were at some parts found to vary from half an inch to an inch in thickness, and presented on division a fibro-cystic structure. No other opening could be found in the external or peritoneal coat of the tumor, except the one already noticed.

The following analogous case of fatal inflammatory laceration of an ovarian dropsical tumor, has occurred still more lately in my private practice. In this, as in the preceding instance, the tumor was unusually rapid in its general growth.

Case II.—The patient, aged forty-three, was married in 1836, and had been for two years a widow. She was the mother of three children. Up to the middle of last year she enjoyed uninterrupted good health. At that time she began to complain of pain in the right side; and at the commencement of the present year, the presence of an abdominal swelling was detected by her medical attendants at Boulogne, where she was residing. In May last she came to Edinburgh, and a large ovarian tumor was then easily diagnosed. It continued to increase and soften with extreme rapidity; and in despite of antiphlogistic measures, iodine, &c., the abdominal swelling and distension were so very great by the first week of September, that tapping became necessary. Twelve imperial pints of a clear glairy fluid were withdrawn, but the bulk of the tumor seemed not much reduced in size by this evacuation; and it was evident that an enormous mass of cysts remained untouched, while a single and comparatively small one only had been opened. She speedily recovered from this paracentesis. By the 16th of October the reaccumulation of fluid was already so great, that tapping was again had recourse to. The fluid now evacuated was of a dark color, and was evidently commixed with pus and inflammatory secretions from the lining-membrane of the perforated cyst. During the few following weeks she complained occasionally of more or less pain and tenderness in the tumor, but was able to take some out-door exercise. On November 17th, she walked to my house, a distance of about half a mile, to ask if I would allow her again to begin the use of some iodine which she had formerly taken. She became sick and vomited on her return home, and subsequently complained of some abdominal pain. Next day vomiting again recurred, and the pain became far more severe and diffused over the whole abdomen. By evening the pulse was very rapid, and almost imperceptible at the wrist, the extremities cold, and the vomiting almost incessant. The symptoms of sinking increased during the night, and she died on the following day about two o'clock, retaining her consciousness to the last, and expressing herself, for some hours previously, as free from all pain.

On making a post mortem examination, a very large multilocular tumor of the right ovary was found, adhering in different parts to the abdominal parietes. A quantity of yellow glairy fluid, mixed with coagulable lymph, was effused into the cavity of the peritoneum. Upon the right side of the tumor, immediately below the liver, two small apertures, in two different but neighboring cysts, were observed; and from these, fluid similar to that in the abdomen welled out upon pressure. At that part of the tumor the cysts were exceedingly numerous, and their walls, at various points, so transparent and attenuated, that under slight pressure they burst, and had their contents evacuated.

Most of the larger cysts in the tumor, and several of the smaller, showed signs of high preceding inflammatory action in their parietes. Many of them contained pus in their cavities, and their injected lining membrane was coated freely, in various parts, with particles and layers of yellowish coagulable lymph. There were perforations in the walls of most of the larger cysts, allowing of free communication between their cavities and the cysts adjoining them.

In few or none of our accounts of the pathology of ovarian tumors, are those morbid appearances described which are produced by inflammation in ovarian multilocular cysts. But when the lining membrane of a compound ovarian cyst is the seat of inflammatory action, it generally presents, as in the preceding instances, morbid appearances similar to those that we see upon inflamed normal serous surfaces, such as the pleura and pericardium. When the effusion from the inflamed membrane is limited to serum merely, this alone is scarcely traceable; because it becomes at once commixed with, and lost among, the normal serous or gelatinous contents of the cyst. But pus is very often a result of inflammation of an ovarian cyst, more particularly if the inflammation has been subacute rather than acute in its type. The purulent matter is usually not seen on tapping or dissection, till the very lowest part of the cyst is emptied, for it is generally observed to have gravitated downwards to the more dependent parts of it. Flakes and masses of loose coagulable lymph are also often present in the contents of the inflamed cyst; or the lymph is attached to the lining surface of it, in the form of granulated spots, or larger patches and layers, or as an organized false membrane covering the interior of the cyst. When the coagulable lymph effused on the interior of an ovarian cyst becomes organized,[1] and the walls of the cyst *again* happen to be the seat of renewed inflammation, blood is frequently poured out to a greater or less extent into the cavity of the cyst, as in hemorrhagic pleurisy; and this termination is perhaps more frequently the result of inflammation of the ovarian cyst, than the result of inflammation of any natural serous membrane. Occasionally the organized lymph deposited by inflammation upon the interior of an ovarian cyst, becomes highly injected under subacute or chronic inflammation; and I have seen it give an appearance of roughness to the interior of the cyst, as if it had been lined by mucous membrane, beset with numerous elongated injected villi. In a few instances the interior of an inflamed ovarian cyst becomes ulcerated, the ulcerated spots being sometimes round, in other cases irregular. The perforations of the walls of continuous cysts seem sometimes to be the result of ulceration; but more frequently, perhaps, they are ruptures produced by mechanical or inflammatory distension. And now and again portions of the walls of a cyst, or of several cysts, are found dead and gangrenous, either as the result of destructive inflammatory action in their tissues, or from the circulation through them being mechanically arrested by the compression and obliteration of the vessels which supplied them with blood.

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[1] In the Anatomical Museum of the University there are some specimens of ovarian cysts beautifully injected by Professor Goodsir, and the lymph on their interior is seen to be highly vascular

In both the cases which I have above described, the effusion of the contents of the lacerated ovarian cyst into the cavity of the peritoneum, was followed by rapid and fatal peritonitis. But many cases are on record, and I have myself seen several, in which the rupture of ovarian cysts into the cavity of the abdomen, led to no such pathological result. And it becomes not only an interesting, but an important practical question to consider—

Under what circumstances is the rupture of an ovarian cyst followed by inflammatory action in the peritoneum; and under what circumstances does this dangerous consequence not supervene?

I believe the proper answer to this question consists in a reference to the condition and contents of the cyst at the time of its laceration. If the walls of the cyst, previous to laceration, had been the seat of inflammatory action, and its contents consist of inflammatory secretions, or perhaps of some other forms of morbid irritating matter, the escape of such morbific fluids into the cavity of the peritoneum will be found, I imagine, to give rise invariably to inflammatory action in the peritoneum itself; and hence, to be always accompanied with danger to the life of the patient. But if, on the other hand, the cyst does not lacerate under inflammatory distension, but has either given way in consequence merely of the gradual thinning and attenuation of its own over-distended walls—or has ruptured under external mechanical injury, as falls, &c.,—and the fluid which escapes through the laceration into the cavity of the peritoneum, is of the mild unirritating character which *naturally* belongs to ovarian cysts in an uninflamed condition, then inflammation of the peritoneum has little or no tendency to supervene. The bland fluid which, under these last conditions, becomes discharged into the cavity of the peritoneum, is not a morbific irritant to that serous membrane, such as a fluid commixed with inflammatory secretions is. Nay, the lacerations of multilocular *uninflamed* ovarian cysts instead of leading to imminent danger and probably speedy death, have frequently, though accidentally, led to the actual preservation, or at least to the prolongation, of the life of the patient. The explanation of this result perhaps merits one or two remarks.

The interior of an ovarian cyst has no power whatever of absorption; and consequently no diuretics, or deobstruents of any kind, have any therapeutic influence on the reduction of an ovarian tumor by removal of its fluid contents by the tissues of the tumor itself. But if the bland uninflamed contents of an ovarian cyst become evacuated by accidental rupture into the cavity of the peritoneum, they may be, and often are, readily absorbed from that position; the peritoneum being normally provided with abundant absorbing powers, and these powers being generally capable of being excited, when required, by the action of diuretics, &c. Consequently when an escape of innocuous unirritating fluid takes place from the sac of an ovarian cyst into the sac of the peritoneum, it may be, and often is, rapidly absorbed and removed from the peritoneal cavity. Cases occasionally occur where nature in this way from time to time spontaneously taps, if we may so speak, an ovarian dropsy into the cavity of the peritoneum; thus ever and anon relieving the patient of the recurrent accumulations of fluid. But another and still happier result has sometimes followed the mechanical laceration of an ovarian cyst into the peritoneal cavity. In fact, in repeated instances it has been observed that in this way, a cure which may be termed a permanent, though a palliative one, has taken place. For, when the laceration in the walls of the ovarian cyst has been originally large—or though originally small, has remained *permanently* open—so as to allow of the continuous escape of the fluid secreted by the ovarian sac into the cavity of the peritoneum itself, the peritoneum under these circumstances, has sometimes acted as a permanent absorbing surface, removing constantly the fluid eliminated by the lining membrane of the ovarian cyst, as a permanent secreting surface. In these fortunate, but rare cases, another result appears sometimes to follow, namely, the ovarian tumor, if it has happened to contain one large and preponderating cyst, becomes collapsed, the fluids which have originally escaped from its own cavity surrounding and compressing its walls externally; and the interior of the cyst, thus kept with its walls in apposition, at last secretes little or no fluid; and possibly perhaps its sides may ultimately adhere together in some very rare cases.

I am acquainted with the history of two cases in which the first tapping of an ovarian dropsy has never been followed by any reaccumulation, the operation in both having now been performed several years ago. And I believe that the secret of this very unusual termination in the two cases in question, is ascribable to the circumstance, that the perforation formed in the walls of the ovarian cyst by the trocar, has remained permanently open like a fistula, allowing of the continuous drain of the ovarian fluid into the cavity of the peritoneum.

Perhaps art will yet be able to imitate, both successfully and with certainty in an appropriate set of ovarian cases, this fortunate accidental termination.

Occasionally, after a patient has been often submitted to the operation of paracentesis, an accidental rupture of the ovarian cyst has produced a comparatively permanent cure, as in the following instance:

Case III.—A patient, now aged 56, the mother of five children, and naturally of a very robust and strong constitution, had up to the end of last year been tapped for ovarian dropsy 44 times by myself and others. Latterly, the paracentesis was required every few weeks, and an enormous amount of fluid was always evacuated. I have repeatedly seen above four gallons of fluid drawn off at a single tapping. Last winter this patient slipped in walking upon a frozen path, and so violently struck the abdomen and ovarian tumor against the ground in her fall as to rupture the cyst. Since that time, however, no new tapping has been required. The abdominal swelling, though still large, is considerably less than it was at the time of the fall, and does not increase in size. For a time the fluid of the cyst evidently escaped freely into the cavity of the peritoneum, and was as regularly absorbed from it. Latterly there has been apparently much less, or, indeed, no perceptible amount of fluid in the cavity of the peritoneum. For several months the patient's skin was in an almost constant state of diaphoresis—a result which to her appeared the more strange, as for years previously she had never been able to excite any perceptible degree of perspiration. This tendency to spontaneous diaphoresis has latterly decreased. The urinary secretion was often previously affected, and greatly diminished as the ovarian tumor enlarged. Since the fall and rupture of the cyst, the kidneys have continued to act very freely and uninterruptedly, the urine secreted being now always clear and limpid.

In the course of the preceding remarks, I have referred to the rupture of ovarian cysts into the cavity of the peritoneum only; but they rupture occasionally also into the intestinal, genital, or urinary canals, or upon the external surface of the abdomen. When ovarian cysts rupture, not into the peritoneum, but into these mucous canals, or on the external cutaneous surface, it is a matter of little moment, in relation to the life and safety of the mother, whether the cysts, before bursting, have been inflamed or not inflamed, and whether their contents be of an acrid and irritating, or of a bland and unirritating character; for there is no danger to the mother from the mere nature of the contents of the cysts when these contents have once escaped into a free mucous canal, or upon the free cutaneous surface of the body. And whatever may be the character of the escaped or escaping fluid, we may equally hope for a temporary or more permanent amelioration of the disease. If the opening is slight and valvular, the fluid accumulating in the cyst may only escape intermittingly or imperfectly; if the opening is larger and more permanent, the contents of the cyst are sometimes kept constantly draining off from the morbid cavity of the ovarian tumor; and this cavity has in consequence, in some cases, diminished and collapsed to a degree amounting to a kind of perfect cure.

I have seen two or three instances in which ovarian cysts have ruptured externally, or into the adjoining mucous canals. The case which I have had an opportunity of watching the longest is the following:

CASE IV.—This patient felt a movable tumor in the abdomen, of the size of the fist, about the age of sixteen or seventeen. I first saw her about eight years afterwards, when the abdomen was greatly distended by a dropsical ovary, larger than the uterus at the full period of pregnancy. She was complaining greatly of the symptoms of over-distension. I removed the fluid by tapping in 1840. Fifteen months afterwards the same operation was repeated, in consequence of the reaccumulation of the fluid. But no paracentesis has been required since that time; and at the present date, November, 1852, she enjoys good health. A few months after the second tapping, the patient had a tedious attack of typhus fever, accompanied and followed by peritoneal inflammation. She was confined for many long weeks to bed. Some time after her recovery, and when the tumor was again increased to a great size, there suddenly supervened, one night on going to bed, much soreness in the tumor; and this was followed ere morning by an abundant and large escape of thickish clouded fluid from the genital canals. The tumor subsided much in size under this discharge, which only, however, lasted for a few days. Again, after this discharge ceased, the tumor increased to an exceeding size; and, on the patient one day twisting herself round on the sofa, she felt, as she herself described it, "something tear" in the right side. In the course of that day, a clear limpid fluid again began to pour profusely from the vagina, and the tumor immediately softened and decreased in size. This discharge has since that time continued, and has now gone on for several years. The discharge is always greatest when the patient is lying or walking, but its total daily amount is at present not great. The ovarian tumor is, she herself believes, now much less in size than after its last rupture, though it is still larger in volume than the adult head. It moves readily under pressure. The patient's menstrual life is regular and normal in all respects; she now enjoys, as I have already stated, the most excellent health; daily performs active house duties; and has latterly become so stout as to weigh upwards of fourteen stone.

The seat of the opening between the ovarian cyst and the genital tubes in the preceding case, it is of course impossible to ascertain. But judging from the dissections which have been made in analogous cases of ovarian cysts emptying themselves by the genital canals, the seat of communication is, in all likelihood, between the ovarian cyst and the Fallopian tube. Lately, Richard has shown, that ovarian cysts do not so unfrequently as was formerly supposed, communicate with the Fallopian tube, and that the cavity of the tube in consequence often becomes distended with the fluid of the adjoining cyst. But though thus distended, the tube is not in many cases sufficiently open at its uterine extremity to allow of the escape through the uterus of the contained fluid. Several cases, however, have now been observed by Morgagni, Boivin, Robertson, and others, in which, after death, ovarian cysts have been found to have such a free communication along the canal of the Fallopian tube, and this tube again with the cavity of the uterus, that the contents of the cyst escaped freely outward along the course of the genital canals.

The desultory observations offered in the preceding remarks may, perhaps, be all briefly recapitulated in the form of the following conclusions:

1. The cyst forming an ovarian dropsy, occasionally rupture, *first*, from inflammatory effusion into and distension of their cavities; or, *secondly*, the contents of the cysts, being only the common bland secretion of such cysts, and unmixed with any inflammatory matter, they may rupture from mere over-dilatation and gradual attenuation of their coats, or under sudden mechanical pressure and injury.

2. When a cyst ruptures from the effects of inflammation, or con-

tains within it at the time of rupture inflammatory secretions and materials, the escaping fluid, if effused into the cavity of the peritoneum, is always liable to be followed by dangerous, and generally fatal, peritonitis.

3. If, however, a cyst bursts into the peritoneum under mechanical injury, or in consequence of simple laceration from over-distension of its cavity, and the fluid effused into the sac of the peritoneum is consequently not commixed with inflammatory secretion, there is little or no great tendency to peritonitis.

4. Sometimes, indeed, when a non-inflamed ovarian cyst thus ruptures into the cavity of the peritoneum, the life of the patient is preserved, or at least prolonged, by this accident.

5. When an ovarian cyst ruptures into a mucous canal, or upon the cutaneous surface, the safety or danger attendant on the laceration is not regulated by the inflamed or non-inflamed character of the effused fluid.

6. In cases in which the fluid of an ovarian cyst obtains an outlet by a mucous canal, or by the skin, a temporary or more permanent reduction of the tumor and comparative cure of it may be the consequence.

Lastly, let me add that, as in many cases and points the surgery of art is an imitation of the surgery of nature, possibly the artificial repetition and establishment of the above modes of relief if they could be imitated safely and certainly, may yet be found capable of temporarily arresting, if not curing, ovarian dropsies in some appropriate cases; and more particularly in instances in which the great bulk of the tumor is formed by one original, large, preponderating cyst, or by several cysts broken up and conjoined into one common cavity or cell.

ON THE TREATMENT OF OVARIAN DROPSY BY INJECTIONS OF IODINE INTO THE CYSTS.[1]

(From Edinburgh Monthly Journal of Medical Science, May, 1854, p. 467.)

It has been often proposed to treat dropsy of the ovary upon the same principles as hydrocele or dropsy of the tunica vaginalis. In accordance with this view, Dr. Hamilton, Scudamore, and others, have in former times injected ovarian cysts with irritating solutions of sulphate of zinc, &c.; but the results have in general proved so unfortunate and disastrous as to prevent a repetition of the practice.

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[1] Read before Edinburgh Obstetric Society, Session XII. 1852-3.

In 1832, Mr. Martin first recommended the use of tincture of iodine as the surest and safest injection for the cure of hydrocele; and this drug seems now almost universally adopted by surgeons in the obliterative treatment of this variety of local dropsy in the male subject.

Latterly, various surgeons, particularly Velpeau, Bonnet, Belluerimi, &c., have extended the practice of iodine injections to the treatment of other local dropsies and cysts; to chronic abscesses; diseases of the joints, &c. And the past experience of surgeons on the subject would certainly seem to show that while the local and direct application of iodine to morbid secreting surfaces has a great power of modifying, altering, and arresting even the secretory action of these surfaces, and often changes suppurative into adhesive inflammation, it shows at the same time wonderfully little aptitude to excite any excess of local irritation and pain. Hence naturally arose the question whether it could be safely injected into such large cysts as those of the common form of dropsical ovary.

In 1846, Dr. Alison of Indiana recorded the history of a chronic case of ovarian dropsy that had been repeatedly tapped, and which he injected at last with a solution of iodine. Severe symptoms followed, but the ultimate result seems to have been favorable. In 1851, Dr. Simpson assisted Mr. Syme in injecting a cyst in the neighborhood of the ovaries, but not a common cystiform ovary. The symptoms which ensued were those of considerable excitement; but the orignal cyst apparently became obliterated. Another one in its vicinity has lately shown itself in this patient.

Within the last year Dr. Simpson has, subsequently to tapping, injected into dropsical ovarian cysts the tincture of iodine in seven or eight cases. For this purpose he has employed the common tincture of iodine of the Edinburgh Pharmacopeia, undiluted. He has usually thrown into the cyst two or three ounces of the tincture. In some cases he has allowed a portion of the injected fluid to re-escape; in others has retained the whole of it in the sac of the cyst that was tapped. From these cases he drew the following conclusions:

1. In none of the cases of ovarian dropsy treated with iodine injections after tapping has he yet seen any considerable amount of local pain follow the injection, with one exception; in most instances no pain at all is felt; and in none has constitutional irritation or fever ensued. In the one exceptional case, considerable local irritation followed; and the pulse rose to 110; but the same phenomena occurred in the same patient after previous tappings, without iodine being used.

2. While the practice seems thus so far perfectly safe in itself, it has by no means proved always as successful as in hydrocele, in preventing a reaccumulation of the dropsical fluid; for in several instances the effusion into the sac seems to have gone on as rapidly as after a simple tapping without iodine injection.

3. But, in two or three of the cases, the iodine injection appears to have quite arrested, for the time being, the progress of the disease, and to have produced obliteration of the tapped cyst, as there is no sign whatever of any reaccumulation, though several months have now elapsed since the date of the operation.

Lastly. Accumulated experience will be required to point out more precisely the special varieties of ovarian dropsy most likely to benefit from iodine injections, the proper times of operating, the quantities of the tincture to be injected, and other correlative points. Perhaps the want of success in some cases has arisen from an insufficient quantity of iodine being used, and from the whole interior of the cyst not being touched by it. The greatest advantage would of course be expected from it in the rare form of unilocular ovarian cysts. In the common compound cyst, the largest and most preponderating cyst is usually alone opened in paracentesis; and though it were obliterated, it would not necessarily prevent some of the other smaller cysts from afterwards enlarging and developing into the usual aggravated form of the disease.

At a meeting of the Edinburgh Medico-Chirurgical Society, November 15, 1854, Dr. Simpson further reported the result of the injection of iodine, in cases which he had recently had under his care at the Royal Infirmary.[1] He mentioned that he had injected a case in his wards a few days ago, from which he had removed 200 ounces of fluid, and that he had tested the urine for iodine, but without success. It was usually present in the urine, however, after the injection of a hydrocele. He had now injected some ten or twelve cases of ovarian dropsy; the disease had recurred in a few of the cases, but in the others, the cure had as yet been permanent. The operation was unattended with any bad results. Indeed, the pulse never rose, and little or no uneasiness was expressed, and that only, if present, in drawing the fluid, apparently the result of adhesions. He employed the Edinburgh tincture of iodine, which had also been recommended for its strength by Mr. Martin, in his paper on the cure of hydrocele by injection.

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[1] See Edinburgh Monthly Journal of Medical Science, December, 1854, p. 565.

OVARIOTOMY:

IS IT—OR IS IT NOT—AN OPERATION JUSTIFIABLE UPON THE COMMON PRINCIPLES OF SURGERY?

ARE—OR ARE NOT—CAPITAL OPERATIONS IN SURGERY JUSTIFIABLE TO THE EXTENT GENERALLY PRACTISED?

"In truth there are Brothers who will brag of the many they have dismembered; but they that truly understand Amputation and their Trade, well know how villainous a thing it is to glory in such a Work—it being more for your credit to save one Member than to cut off many."—*Richard Wiseman's 6th Chirurgical Treatise.*

"Cut off—
Unshriven, unanointed, unaneled:—
If thou hast nature in thee, bear it not."
Complaint of the Ghost in Hamlet.

Dr. Bennett having communicated to the Medico-Chirurgical Society of Edinburgh, on the 17th of December, 1845,[1] a case in which ovariotomy had been performed, Professor Simpson stated, that he believed ovariotomy quite unjustifiable in many of the cases in which it had been had recourse to, but in a few rare instances, like that of Dr. Bennett's patient, quite as justifiable as most of the operations performed in surgery for chronic affections. And it appeared to him that two circumstances prevented ovariotomy from obtaining a fair consideration and fair trial, especially with professed surgeons. *First*, the diagnosis and the operation were, under the existing divisions and arrangements of practice, undertaken by two different sets of practitioners—the former by the obstetric physician, and the latter by the operating surgeon. It was, perhaps, the only capital operation in which the surgeon was now required to proceed upon the diagnostic knowledge of another party; and no one was to be blamed if he felt a natural repugnance to incur so serious a responsibility on such grounds. *Secondly*, surgeons, as a class, still confessedly allow themselves to be greatly bound and swayed by the trammels of authority, and the mere fact that some of the highest names in surgery had once declared, with or without due investigation, against ovariotomy, is with most others an ample and satisfactory reason for totally rejecting the operation. In the same way, but in an opposite direction, the leading authorities in the surgical world having agreed to consider ligature of the *arteria innominata* as a legitimate operation, it has now been repeatedly performed. But what has been the result? Why, the vessel has been tied some twelve times, according to Mr. Phillips—it might be oftener, or it might not be so often—but at all events, *as often as* the operation had been performed, it had proved fatal; and yet, because it had been decreed proper and justifiable by authority, we find it in the very last text-books on surgery still commented on as such; whilst ovariotomy, as proving fatal in one out of every two or three cases, was loudly denounced as improper and unjustifiable. In the important department of surgery, such inconsistency would doubtlessly betimes become rectified—when, as had been long the case in medicine and midwifery, the dogmatism of mere facts and experience came to be more respected than the dogmatism of mere opinion and authority.

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[1] See Report in Edinburgh Monthly Journal of Medical Science, January, 1846, p. 55.

The diseased condition of the ovary, to which the operation of ovariotomy was particularly applicable, if applicable at all, was, in Dr. S.'s opinion, that form of ovarian dropsy which was by far the most frequent of all, and consisted in multilocular cystic degeneration of the organ—the gelatiniform, or areolar cancer of some authors. All other forms of ovarian dropsy, as they were called, were rare in comparison to this; and to it all remarks, in such a discussion as this, principally or entirely applied. In most instances—in nine cases out of ten—this species of ovarian dropsy pursued, he believed, a regular progress onward, towards greater or less enlargement, insufferable distension, more or less repeated palliative tappings, frequently disintegration of the morbid structure, local irritation, constitutional exhaustion, and death. Generally, it took a series of years to run its course, but sometimes it passed through its phases and progress more rapidly. We want a sufficient body of well-observed facts to know the average duration and simple natural history of this, as of most other diseases. Some authorities averred that the disease occasionally went on for twenty, thirty, forty, or fifty years. He doubted entirely the truth of such alleged cases, and believed that abdominal tumors, with this history, were not affections of the ovary at all, certainly not its cystic multilocular disease, but fibrous tumors of the uterus, which were often exceedingly chronic in their progress, and, as he had repeatedly seen, were very frequently mistaken for the ovarian affection under dispute. Again it had been as strongly averred that cases of multilocular dropsy of the ovary had been absorbed and cured. He equally and entirely doubted the validity of this observation. Errors in diagnosis would, he believed, account readily for all such therapeutical incredibilities. He had seen hysterical tympanitic distension of a portion of bowel, and collections of fæces, mistaken for ovarian tumors; and these were certainly quite curable. He had, in several instances, seen also ovarian dropsy very perfectly simulated in form, figure, situation, &c., by large chronic inflammatory effusions into the cellular tissue of the pelvis and broad ligament, always commencing with and accompanied by inflammatory phenomena, and these, like similar inflammatory effusions elsewhere, were always more or less completely amenable to medical treatment. But he had no belief whatever that iodine, or mercury, or muriate of lime, or aqua potassæ, or diuretics, or deobstruents, or aught else, were capable of absorbing and removing the complicated structure and contents of a multilocular cystic tumor of the ovary. He would almost as soon believe that the head could be absorbed and removed by medicine. When the disease was accompanied with much local vascular action and congestion, the occasional loss of blood was certainly sometimes beneficial. But in the general run of cases of this malady, he had long come to the conclusion that we did all that was possible with medicine, when we kept the individual functions of the economy as near as possible to their individual standards of health. Break down the activity and vigor of the system by mercury or other debilitating medicines, and then the ovarian disease only too often progressed with double strides.

Seeing medicine was of so little direct use—what measures had surgery to propose? The cystic structure of the tumor had been tapped and injected in imitation of the treatment of hydrocele—setons had been passed into it, and through it—incisions had been made into its walls, &c. &c.; but all such operations were now, he believed, abandoned by general consent, as useless in their effects, and far too often fatal in their practice to admit at all of repetition. In fact, two measures only were at the present day applied to the surgical treatment of the disease, namely, 1. Tapping; and, 2. Total extirpation. The first of these operations—tapping—was professedly adopted merely as a palliative measure—for the present relief of the patient—not for the cure of the disease. In a very few instances the tumor appears to become bound down by adhesions after tapping, and no reaccumulation takes place; but these cases are so very rare, that in practising the operation, we scarcely even venture to reckon upon the possibility of this occurrence. In some cases where the tumor is very large, but the cells small, and containing gelatiniform matter, tapping is of no use, and cannot in any degree evacuate or diminish its contents. Fortunately for the success of this operative procedure, the anterior and superior cell or series of cells were generally large, and dilated more than the others, in consequence of least resistance being opposed to their growth and distension in this direction. And tapping, when adopted, though a palliative measure only, was by no means so free from danger, as some practitioners think, and some writers would seem to allege. We had as yet no sufficient collection of data to show its actual results. But Mr. Southam had commenced the inquiry, by tabulating the results of twenty cases of the operation. Fifteen of these cases had been recorded by Drs. Bright and Barlow, without apparently any view to such an investigation, and hence afforded the more valuable and unprejudiced evidence. Four of the twenty patients, or one in five, died of the effects of the first tapping. Four patients died of inflammation within a few days after the operation; three more died in one month; fourteen in all died within nine months after the first tapping. Of the remaining six, two died in eighteen months, and four lived from periods varying from four to nine years.

Paracentesis, whilst thus merely a means of *palliation*, was still a proceeding in which no inconsiderable amount of danger appeared to be incurred. Ovariotomy, on the other hand, was an operation which, if successful, was professedly a means for the perfect and radical *cure* of the disease. But it was undoubtedly a most serious and dangerous operation; was it therefore warrantable or unwarrantable, when judged of by the *principles* applied by surgeons to the determination of the propriety of other capital operations in chronic diseases? Let us consider ovariotomy and the objections to it in this point of view; for by such a comparative test will the propriety or the impropriety of the operation be best ascertained and determined. The principal objections which Dr. Simpson had heard urged against ovariotomy were as follows:

1. *It was an operation accompanied with great danger and mortality.*

All parties are ready fully to admit this point. But it is by no means a matter decisive, as some think, of the impropriety of the operation. At all events, if ovariotomy is to be condemned and suppressed on this account, several of the legitimatized capital operations in surgery must be equally, or still more strongly, condemned on exactly the same charge; for it is in reality not more fatal than many of these operations, and even not so fatal as some of them. On this subject, the mortality accompanying capital operations in general, very erroneous views seem to be entertained by many members of the profession. The statement of a few simple statistical facts will serve to prove the position assumed, and may, perhaps, surprise those who have not directed particular attention to the subject. Dr. Churchill, Mr. Phillips, Dr. Atlee, and Dr. Cormack, (see his Journal for May last), had each calculated the mortality in ovariotomy, from the cases on record, and came to nearly the same conclusion. Dr. Simpson took Dr. Cormack's results as being those of a writer against the operation, and hence his tables could not be suspected of any unfair leaning towards ovariotomy.

Out of 89 cases in which ovariotomy had been either performed or attempted, 34 sunk, or nearly 4 in every 10 patients died.

Out of 65 cases, collected by Dr. Cormack, in which the operation had been perfected, 25 died, or between 3 and 4 out of every 10 patients were lost.

Now Malgaigne has shown, that out of 852 amputations of the extremities of all kinds, including those of the fingers and toes, which were performed in the Parisian hospitals from 1836 to 1841, 332 died, or about 4 out of every 10 proved fatal.

Among these,	out of	201	amputations of the	thigh,	126 died, or 6	in every	10
"	"	192	"	leg,	106 died, or 5½	"	10
"	"	91	"	arm,	41 died, or 4½	"	10

Of the amputations of the thigh, in 46 cases the operation was performed for severe injury of the limb: of these 34 died, or more than 7 out of every 10.

When we looked to the results of amputation nearer home, the results were not much more encouraging. In the Glasgow Infirmary, from 1795 to 1840, Dr. Lawrie has shown that out of 276 amputations performed, 101 proved fatal, or nearly 4 in 10 died.

Among these,	out of	128	amputations of the	thigh,	46 died, or 3½	in every	10
"	"	62	"	leg,	30 died, or 5	"	10
"	"	53	"	arm,	21 died, or 4½	"	10

In the Edinburgh Infirmary, during the four years commencing July, 1839, there occurred 72 amputations of the thigh, leg, shoulder joint, arm, and fore-arm. Of the 72 patients, 37 recovered and 35 died—or nearly 5 in every 10. Of these amputations, 18 were primary. Out of 4 primary amputations of the leg, one patient recovered and 3 died. Out of 4 similar amputations at the shoulder-joint, 1 recovered, and 3 died. There was one primary amputation of the arm; the patient died. There were eight primary amputations of the thigh; all the eight patients died.[1]

Mr. Phillips has collected the histories of 171 cases in which the larger arteries of the body were tied; of these 57 died, or about 3½ in every 10. Dr. Inman has collected 199 cases of these operations; 66 died, or about 3½ in every 10. Out of 40 cases of ligature of the subclavian artery which he has tabulated, 18 proved fatal, or nearly 5 in every 10 died.

In his work on hernia Sir A. Cooper records 36 deaths among 77 operations for that disease, or nearly 5 in every 10 died. Dr. Inman has collated 545 cases of operation for hernia; 260 proved fatal, or nearly 5 in every 10 of the patients died.

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[1] See Dr. Peacock's Official Reports.

In the earlier years of life, lithotomy is comparatively a safe and legitimate operation, and few die. But it is quite different when the operation is submitted to at forty years of age, and upwards. At and above this term of life, Dr. Willis has shown, from numerous statistical returns, that from 2 to 5 out of every 10 operated upon, die.

Even what we deem slighter operations are sometimes attended in the absolute by no inconsiderable danger to life. Out of 95 cases of excision of the mamma, referred to in Dr. Cormack's Journal for February, 1843, 20 died, or 2 in every 10. In how many cases of the remaining 75 would the disease inevitably return and ultimately destroy the patient?

Ovariotomy, then, is fatal in the proportion of about 35 or 40 in every 100 operated upon; but in most capital operations we have singly as high or even a higher mortality than 35 or 40 per cent. Amputation of the thigh has a higher mortality. So has amputation of the arm. So has ligature of the subclavian, for aneurism. Tying the innominata is fatal in every case. The operation for hernia has a higher mortality. Lithotomy is as fatal in most hands after the middle term of life. Even amputation of the leg below the knee is scarcely more safe, or at all events as many, or more, die after amputation of the leg, in the hospital practice of Paris and Glasgow, as die after ovariotomy.

It had been foolishly objected to the statistics of ovariotomy, that we did not know all the unsuccessful cases. Dr. S. believed that the ascertained statistics regarding it were as full and complete as the statistics regarding any other capital operation. It was too serious and too startling an operation for any cases of it to remain easily hid. On the other hand, it could be readily shown that the statistics of our major surgical operations were not always reported in the most faithful manner, and so as to give the most accurate results. Malgaigne candidly confesses as much in regard to the elaborate statistics which he has collected of various surgical operations from different hospitals.

The existing results regarding ovariotomy would, probably, be on all hands allowed to demonstrate one point, namely, that exposure of the cavity of the peritoneum was not so dangerous a proceeding as was formerly thought by pathologists. Surgeons have exposed it often in hernial operations, and even left ligatures upon its omental vessels when necessary, and not unfrequently with impunity. In 1842-43 a portion of the omentum was removed in six operations for hernia at St. George's Hospital, London. "In some of the cases, two ligatures, each embracing one-half of the omental mass, were applied; in the other cases, ligatures were applied to all the bleeding vessels." Five of the six patients recovered. One died comatose, a few hours after the operation, from disease of the brain.[1] But still, it must be confessed, extreme dread of all such abdominal surgery was, and probably is, the prevailing idea. The comparative success of the Cæsarean section in the hands of continental accoucheurs, might almost have taught us a different lesson, the peritoneal cavity in that operation being of necessity freely opened up; and we may daily see the same done upon the females of some of our domestic animals, with remarkable impunity, in the coarse operation of spaying.

2. *The ultimate results of cases of Ovariotomy were alleged to be unknown and unfavorable.*

It was urged that the reports of cases had been published with too great haste, and before the final effects could be known months and years afterwards. Great weight had been attached to this argument in the question of ovariotomy. But it probably would be found to tell against other capital operations with much more truth and effect than against ovariotomy. In how few instances were the published reports of capital surgical operations carried beyond a few weeks? And what a large proportion did die within a short period after escaping from the more immediate consequences of the operation for aneurism, and stone, and cancer, and amputation—and that with very broken and imperfect health, too, during the interval. The primary history of cases of these operations was given, yet not their ultimate history. Dr. Simpson at the same time adduced various facts to show that as far as regarded ovariotomy, the allegation did not in reality hold good. The process of reparation after ovariotomy is, say some, too great to be accomplished with health and safety. Theory may argue so—but facts here give a direct and practical denial to theory, by demonstrating the reverse to be true. In one of the first cases operated on, Emiliani's in 1816, the patient has since become the mother of five living children—an ample proof of the completeness of the cure. Dr. Simpson read a note from Dr. Clay of Manchester, stating the present condition, on 14th December, 1845, of the patients that he had operated upon two or three years back. His first patient, operated on 12th September, 1842, "continues quite well, and follows her household duties with ease and comfort." A patient, subjected to ovariotomy on the 25th September, 1842, "is at this time perfectly well, and capable of greater exertion than most women of her age, viz., 60." Regarding a third patient operated upon in November, 1842, Dr. Clay states, "I saw this case a few days ago on account of a polypus of the nose—in every other respect she is quite well." A patient, operated upon in August, 1843, "is at this time perfectly well—saw her a few days ago." Dr. Clay operated on two cases in October, 1843, "the first remains at this time quite well"—the second reports herself "in better health now than in any part of her former life." A case was operated upon in November, 1843: "I have," says Dr. Clay, "seen this woman frequently of late whilst attending other branches of the family—she is quite well;"—and so on with regard to some others.

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[1] Hewett, in Medico-Chirurgical Transactions, Vol. XXVII.

3. *It was argued that the extirpation of the affected ovary would not necessarily effect a perfect cure of the disease, or secure the patient against its return.*

This certainly holds true of the diseases for which several of the major operations in surgery were performed, but as certainly it did not hold true of multilocular dropsy of the ovary. The surgeon amputates a limb, or excises a tumor for some form of carcinomatous disease, hazarding more or less the life of his patient for the temporary removal of a diseased action which is almost perfectly certain to recur. He ties the subclavian for aneurism—but is it not a disease which is very liable to coexist in different vessels at the same time, or to form consecutively in different parts—and if the patient escapes the great and immediate dangers of the operation, has he any surety against its reappearance elsewhere? You amputate the thigh to get rid of a scrofulous or tubercular knee joint. But in how many cases is local tubercular disease the mere result of a general diathesis, that ere long will betray itself in some other part or organ. Dr. Simpson thought it a point of the highest practical moment to consider that, on the contrary, the pathological nature of multilocular disease of the ovary was such that it had no tendency to recur after its complete removal. From the character of its morbid structure, and its clinical history, it was certain that it presented no liability to spring up again, like malignant or tubercular disease, in the same locality—or in distant and in different organs of the body. The other ovary might be partially affected, and if so, might require removal along with the first—a step which at the time would probably not add much to the absolute danger of the operation—seeing the abdomen was once opened. Do not surgeons operate for popliteal aneurism, when it is present in both limbs, even with the additional chances of an analogous diseased condition of the vessels existing internally. Probably it will be found that a surgeon would more rarely require to repeat ovariotomy, in consequence of the remaining ovary subsequently becoming diseased, than he now requires to repeat lithotomy, in consequence of a second or a third stone forming after a time in the bladder.

4. *Ovarian disease, it was averred, does not produce such dangerous and urgent symptoms as to demand an operation.*

Dr. Simpson said that he had already adverted sufficiently to the dangerous and ultimately fatal tendency of the common multilocular dropsy of the ovary. He had at present charge of one case, where an enormous ovarian tumor produced occasional most intense suffering, in the form of severe abdominal pains and spasms resembling the agonies of labor. In many cases where it had reached a large size, it more or less incapacitated the patient, by its simple weight and volume, from following the ordinary duties belonging to her station; and if poor, threw her upon the bounty and charity of others. In most it was, after a time, liable to be attended with local attacks of irritation and inflammation, fever, &c., or produced dyspnœa, difficult progression, &c.

He doubted if, in many cases operated upon, of aneurism or necrosis, or ulcers, &c., supposed to demand amputation, &c., the suffering or the incapacity from the duties of life were greater than in a large proportion of ovarian cases. But, argue the surgeons, *we* operate early in aneurisms, &c., because they continue to increase —the same is true of ovarian tumors; because the aneurismal swelling is, after a time, liable to affect the structure of neighboring parts, and render late operative interference less successful—the same is true of ovarian tumors; because with the aneurismal disease the constitution will sympathize and become debilitated—the same is true of the ovarian tumor; because the aneurism may burst and endanger life—the same is true of ovarian tumors. He had, two years ago, seen one burst into the peritoneum, and prove fatal; its parietes were eroded by small internal ulcerations at several points, and at last had given way. Any argument urging haste in the one case, would, he feared, equally apply to the other. On the contrary, would proper palliative treatment applied to *local* aneurisms not stay their progress, and make them as chronic, if not more so, in their course, than multilocular tumors? Mr. Fergusson has lately stated that he has watched one case of axillary aneurism "for several years" without its increasing. And aneurisms sometimes are, at last, spontaneously cured; much oftener, he believed, than ovarian dropsies. Take another case that happened in the Hospital practice this morning. A man applied with stricture, and symptoms of stricture only. On passing a small bougie, a stone is struck in the bladder, and the patient is forthwith advised to submit his life to all the perils and consequences of lithotomy, though he has no suffering traceable to the calculus. Would it be justifiable to advise a patient with an ovarian dropsy, giving her no trouble, to submit in the same way to ovariotomy? He most assuredly thought it would be utterly unwarrantable. And the palliative treatment for urinary deposits and calculus was, it must be further recollected, now far more advanced than the palliative treatment of ovarian dropsy. A calculus of this kind would not be likely to increase so rapidly as to destroy the patient in five or ten years. An ovarian tumor very likely would do so. And sometimes, as in this case, a urinary calculus does not really give rise to such uneasiness as to demand any very active palliative treatment. Do we not sometimes see calculi in the bladder after death, which have never given rise to any marked symptoms during life? Again, does not the operation for the obliteration of the varicose veins of a limb sometimes prove speedily fatal, and yet the disease itself is one easily palliated by rest and bandages. Besides, this recognized legitimate surgical operation for varicose enlargement was not only dangerous to life, but, he feared, useless in its effects. In most cases, at least, the disease was as bad again in a few months as it was before surgical interference was adopted. Did we not sometimes see surgeons amputate the limb, when it was merely the seat of simple and benign, but untractable ulceration? And ulceration might be a very serious inconvenience to a laboring man; but here we have a dangerous and often fatal operation performed for a disease which was not fatal nor dangerous in its own character, and that easily admitted of palliative treatment. Altogether, it appeared to him, that the question of *when* we should conscientiously deem ourselves entitled to practise ovariotomy, or any other dangerous operation for a chronic disease, was one that had hitherto received no sufficient attention. It was a question that probably must always be decided much upon the merits of each individual case, and in regard to which different minds may come to opposite and yet conscientious conclusions. It always embraced a difficult moral and professional problem, in cases where the required operation was, as in ovariotomy, ligature of the larger vessels, amputation, lithotomy, &c., directly and immediately dangerous to the life of our patient. It resolved itself in such a case into a question of this kind: *Am I conscientiously* ENTITLED *to inflict deliberately upon my own fellow-creature, with my own hands, the imminent and immediate chance of* DEATH, *for the problematical and prospective chance of his future improved* HEALTH *and prolonged* LIFE? In calculating what *amount* of danger of present death ought to be incurred for the hazard of future good, many secondary elements necessarily entered into the problem—such as the existing chance of otherwise palliating the disease and prolonging life with certainty for months or years—the extent of attendant suffering—the probability of the affection recurring—or already existing elsewhere, &c. &c. In such a calculation, the ideal glory of a successful operative result has probably been too often allowed to dazzle the calm judgment of both the operator and his patient, and the darker but equally truthful shades of the picture have been, for the moment, so far obscured and unseen. With the patient, the stern reality of danger and death too frequently vanishes, here or elsewhere, before the strong hope of life. And the surgeon, like the soldier, is, in the computation of his successes, perhaps too liable to forget the actual amount of human suffering and human fatality through which these successes are obtained.

5. *It has often been argued against ovariotomy, that the operation, when begun, could not sometimes be completed from adhesions, &c.; or no tumor could be found.*

These circumstances were the results of imperfect diagnosis; and he adverted to the occasional difficulties connected with the discrimination of the ovarian tumors, and admitted them to their full extent. He explained that he could scarcely conceive the repetition of some of these errors if due caution were adopted. If other means failed, an exploring needle would always certify the presence of a tumor, and its structure or nature; the uterine bougie would show if the tumor were situated in the uterus or ovary, &c. The chief and ruling difficulty at this moment was assuredly that of discovering the existence or not of adhesions of the tumor by false membranes, their extent, &c. If this point could by any measures be cleared up, it would remove one of the great, perhaps the greatest, existing objection to ovariotomy. Nor was it totally hopeless. One of the most sure and solid advances made by modern pathology was our gradual but great improvement in the physical diagnosis of the diseased states of different organs. Probably the next marked step in this path would be the detection of some measure or measures for improving our knowledge of the physical diagnosis of diseases of the abdominal viscera. It was not more extravagant to expect this than thirty years ago it would have been extravagant to expect all the vast aid and certainty which we now derive from auscultation in the physical diagnosis of diseases of the chest; and he believed some important steps had been already made regarding the detection of ovarian adhesions by Dr. Frederick Bird of London, and others. Dr. Bennett's contribution was under this head of the highest pathological and practical value. As soon as the ovarian tumor in the case described by him was exposed, it was evident to all who had taken an interest in the question, that the accompanying ascitic effusion oozed by apertures from the interior of the ovarian tumor, and was a secondary result. But if, as Dr. Bennett would, he doubted not, be able ultimately to show, it was possible to distinguish by microscopic characters between the fluid of common ascites and the fluid of ascites thrown into the peritoneum through small ulcerated apertures in the walls of an ovarian tumor, it would clear up various points in a set of cases formerly surrounded with perplexing difficulties. It would enable us to detect the pathological cause and source of the great ascitic collections sometimes attendant upon comparatively small ovarian tumors. Cases with this complication, that is, ovarian tumors with apertures allowing their secretions to pass into the general peritoneal cavity, evidently in general run a very rapid and fatal course. If these secretions were acrid and irritating, as when mixed with inflammatory effusions from the walls or cysts, they might at once excite fatal peritonitis. This, however, was rare, and the exception to the rule. Usually the secreted fluid appeared to be blander, distilled slowly through the morbid openings in the parietes of the tumor, and, accumulating in the peritoneum, required ever and anon to be removed from that cavity by tappings, which soon became more and more frequent, and more and more exhausting. This variety was probably, he suggested, of all ovarian cases, that most surely justifying the adoption of extirpation. And besides, in these very cases, it was generally ascertainable whether there were adhesions or not, for the tumor was surrounded by a fluid medium, and hence admitted more easily of this point of diagnosis being made out by its mobility in that medium. Perhaps it was, on the other hand, unjustifiable in our present state of knowledge to operate where there were many adhesions, or any great want of certainty about the existence and extent of them; as it was, where the tappings, though many and frequent, did not, as was seen in a few exceptional cases on record, exhaust rapidly the powers of the patient, or threaten her life with any prospect of urgent or immediate danger.

But, admitting to their fullest extent the occasional difficulties which have been found to beset the diagnosis of ovarian tumors for operation, do we not meet with occasional difficulties of exactly the same kind in other surgical operations, and which do not yet deter surgeons from interfering? Is the trephine never used without detecting any effused blood, or pus, or depressed and fractured fragments of bone? In tying the carotid and subclavian and iliac arteries for aneurism, it has now repeatedly happened that all the great dangers of these operations had been submitted to most uselessly, the disease, during the operation or after death, being found not to be aneurismal at all,[1] and hence not at all curable by such a procedure; and when aneurismal, the operation has been sometimes left incompleted—the vessel searched for either not being secured, or, as has happened with Dupuytren and others, it has been reached and fatally transfixed with the ligature, instead of being surrounded by it. Have not the antrum, and the mamma, and the testicle, &c., been sometimes found to be the seat of simple inflammatory and curable effusion, after all the usual operative measures for the removal of supposed malignant tumors from these localities had been commenced, or even completed. A surgeon had excised ten scirrhous mammæ, and in every case with perfect success. In not one was there any return of the disease. Sir Benjamin Brodie was requested by this active operator to see a new case of scirrhus which he had determined to remove. "It was nothing more," says Sir Benjamin, "than a chronic abscess of the breast, which he denominated scirrhus."[2] Dr. S. had seen amputation of the thigh performed by a celebrated surgeon for supposed scrofulous disease of the knee-joint, and where, on examining afterwards the amputated limb, no traces of such a disease could be found. Most of them had seen cases of diseased limbs threatened with, or actually condemned to the knife, and which yet afterwards got quite well, when surgical interference would not be submitted to by the patient. In some cases of hernia, is it not occasionally found impossible, as in some cases of ovariotomy, to finish the operation, and return the bowel in consequence of extensive morbid adhesions or other causes? Is not the stone sometimes found encysted in lithotomy, and for that or other causes its removal rendered impossible after the bladder is cut into? Is the operation for the removal of an incarcerated piece of necrosed bone not sometimes found impossible after it was begun? Grave errors have been committed in diagnosis in ovariotomy cases, in relation to the propriety and practicability of the operation, but he doubted if as grave errors were not as frequently committed in some other recognized capital operations. A much greater amount of caution was undoubtedly requisite on this head.

In summing up his statement, Dr. Simpson allowed that ovariotomy was a most serious and dangerous operation; but at the same time he maintained, that surgeons in declaiming against it had used a series of arguments, all, or almost all, of which would equally, and some of them still more strongly, apply against those capital operations for chronic maladies, regarding the propriety of which they did not affect to entertain one single doubt, and which they every day performed without the slightest scruple. For his own part, however, he entirely doubted whether surgeons should resort to many of these operations, under the circumstances in which they often adopted them. He doubted whether, for example, they should at once subject a man to all the immediate and fearful perils of lithotomy and lithotrity, because he had a stone in the bladder which gave him little or no uneasiness, and which might allow him, under proper regimen and treatment, to live and perform the duties of life for a long series of years. He doubted whether, in a case of axillary, or carotid, or popliteal aneurism, slowly increasing or not increasing at all, having some small chance of spontaneous cure, and having no inconsiderable chance of being followed or accompanied with the same disease in other parts of the arterial system, all the dangers of the ligature of the vessel nearer the heart, should be at once recklessly encountered. He doubted whether, in malignant or carcinomatous disease of the forearm or leg, amputation of the arm or thigh should be at once resorted to, with the hazard of death in a few hours or days in one out of every two operations, and the almost perfect certainty of the same morbid action reappearing sooner or later in the stump, or in some other part, if the patient did happen to survive. And, on the same principle, he doubted whether ovariotomy had not been employed in some cases under perfectly unjustifiable conditions, when the health and life of the patient were not immediately threatened by the stage and progress of the malady, when the tumor was a source of inconvenience and deformity, rather than a source of danger, and when the evils of the disease were as yet prospective rather than real. But if the health of the patient were becoming rapidly undermined by the disease—if the progress of the affection showed that ere long it would inevitably prove fatal—if the question were thus reduced to one of certain and not distant death from the course of the malady, or *possibly* an entire escape from the affection, with prolonged life and health from the operation—and if, in addition, the ascertained or apparent freedom of the tumor from adhesions and other circumstances were such as to present no counter-indication—then he believed that ovariotomy might be undertaken under conditions far more justifiable and legitimate than the surgeon could possibly urge in favor of some of his stone, and aneurism, and other capital operations for pathological lesions of a similarly chronic character, and course.

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[1] During the discussion, Dr. Spittal mentioned, that out of fifty-nine cases, collected by Dr. Norris, of ligature of the subclavian artery for aneurism, "in three no aneurism existed, and in two the tumor was mistaken for aneurism and punctured." Hence, in one out of every twelve of these cases, the diagnosis was perfectly wrong.

[2] Medical Gazette, 1844.

Lastly, he stated, that if betimes ovariotomy came to be recognized as a surgical operation, fit and proper in such cases of ovarian disease as he adverted to, or others, he had no doubt the steps of the operation itself would meet with improvements. Such improvements were almost always wrought out by experience. How different is amputation now, from what it was formerly with the hot iron, or boiling pitch, to seal up the cut vessels. How comparatively safe and simple is the tying of an artery now from what it was half a century ago, with the flat double ligatures, and ligatures of reserve, &c. One great source of danger in ovariotomy was the irritation and injury inflicted on the intestinal canal and peritoneum from the strong ligature which was required for the stalk of the tumor, being passed through the abdominal cavity, and out at the external wound —remaining there for days or weeks, and keeping a portion of the wound in the abdomen necessarily open by its presence, and, consequently, so far still more hazarding the occurrence of peritonitis. Probably it might be possible to devise some other measures of securing the large vessels, principally *veins*, be it remarked, of the pedicle, and thus save the several dangers arising, 1st. From leaving the ligature to irritate there; 2d. From the ligature, by its constriction of the stalk, producing strangulation; and 3d. From its exciting phlebitis. And if the ligature still continues to be employed, it would, he believed, be found a great improvement, as had been suggested to him by his excellent friend and assistant, Dr. Keith, to pass it down, perforate the very thin layer of serous and mucous membranes dividing the utero-rectal reflection of the peritoneum from the upper and back part of the vagina, and bring it out along the vaginal canal. He knew that on the dead subject this could be done with the greatest facility. It would have several advantages. 1. It would enable the surgeon to close at once the whole length of the incision in the abdominal parietes; 2. The sides of the vaginal canal, being in contact, would act as a valve sufficient to prevent that dangerous access and egress of air to and from the peritoneum under strong respiration, vomiting, &c., which had sometimes occurred through the aperture kept open by the ligature, in the old form of operating; 3. The ligature would not pass through the same extent of the peritoneal cavity, and would scarcely, if at all, touch or irritate the folds of the intestinal canal; and, 4. If the uterus happened to be displaced backwards upon the rectum, the ligature applied to the posterior surface of its broad ligament would be included and imbedded in a cavity almost divided and separated from the general cavity of the peritoneum, and where the process of reparation and adhesion might often go on without fatally extending upwards into the general peritoneal sac. Farther, the cases already published recounted some errors which the experience derived from them showed might be avoided in future. We were thus warned to take great care to close, as accurately as possible, the peritoneal side of the wound, to prevent strangulation of a fold of intestine in its edges; to adopt precautions with the same view, of not allowing a similar effect from the portion of ligature passing through the abdomen; not to allow the bladder to become distended, lest it drag upon the uterus, or disturb the reparative process; not to excite inflammation by dragging at the ligatures, &c. &c.

Dr. Simpson subsequently added a few observations in reply to some remarks made by Mr. Spence. According to Mr. Spence, trephining for the discovery of effused blood, and tying the arteria innominata, were not now looked upon by surgeons as justifiable operations. Probably, the Society would allow that the lately published text-books by Professors Fergusson and Syme were fair standards of the existing state of British Surgery. Now, Mr. Fergusson not only in his work advises trepanning for effused blood, but even speaks of cutting through the dura mater in search of it. Mr. Syme, in treating of the ligature of the innominata, states that it is a dangerous operation, but he does not give the most remote hint as to its being regarded by him or others, as an unjustifiable one; and, on the contrary, he describes the steps of the operation, and suggests means for rendering it safer. Mr. Spence had alluded to the spontaneous cure of aneurism, and thought him wrong in his ideas about its frequency. Dr. S. did not know of any data calculated to show how often, or how seldom, the spontaneous cure of a *local circumscribed aneurism*—such as surgeons operated for—might be expected; but of eight or ten cases of popliteal aneurism, seen in the hospital within as many years, nature set up an inflammatory action in the sac or vessel, or both, and cured one case, a patient of Dr. Cunningham, before art had an opportunity of interfering. At all events, he felt assured, that if local external aneurisms were treated by common palliative measures, their spontaneous cure would be found not to be so rare as the spontaneous cure of ovarian dropsy; and he feared that all Mr. Spence's arguments for early operation in the one case, most unwittingly applied with similar appropriateness to the other.

IMPERFECT DEVELOPMENT OF THE UTERUS, GIVING RISE TO AMENORRHŒA, ETC.[1]

(From Edinburgh Monthly Journal of Medical Science, September, 1854, p. 276.)

DR. SIMPSON mentioned, that he had seen a considerable number of cases of short or imperfectly developed uteri in the living subject. The imperfect development of the organ was ascertained, on examination by the finger, by the small and atrophied cervix uteri, and by actual admeasurement of the length of the cavity by the uterine sound. Instead of being 2½ inches in length, the cavity in such cases was only 2 inches, or more frequently only 1½ or 1 inch long. The subjects of this imperfect development were often well made and formed in other respects. But the malformation led to various functional defects, especially amenorrhœa and sterility. The amenorrhœa was usually persistent, and when a patient applied for medical relief, who was already twenty-five or thirty years of age, this malformation would in a large proportion of cases be found to be the organic cause. In some such cases of amenorrhœa, there was great vascularity of the face, and occasionally a most unconquerable form of acne. He had seen in some of these instances the wearing of an intra-uterine galvanic, or zinc and copper, pessary—gradually increased in size—followed by the best results, and even occasionally by cure of the amenorrhœa. The uterus developed itself around such a foreign body, when it filled its cavity, as it did around a fibrous tumor or an ovum.[2]

We have often seen galvanic pessaries used by Dr. S. in ordinary cases of amenorrhœa, which had resisted all the usual means of treatment, and with perfect success. The galvanic pessary has the same form as Fig. 20, but one portion of the intra-uterine stem is of zinc and the other of copper.—(ED.)

ON THE NATURE OF THE MEMBRANE OCCASIONALLY EXPELLED IN DYSMENORRHŒA.

(From Edinburgh Monthly Journal of Medical Science, September, 1846, p. 161.)

IT is well known that in some cases of dysmenorrhœa an organized membrane is expelled, with much pain, from the uterus during the course of the catamenial discharge, and that this happens either occasionally only, or, what is far more common, during a long succession of menstrual periods.

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[1] Extracted from Proceedings of the Edinburgh Obstetric Society, January 26, 1853.

[2] See ante, pages 89 and 113.

All authors who have expressed an opinion regarding the nature of this dysmenorrhœal membrane have, so far as I am aware, regarded it as a morbid structure formed by the exudation of coagulable lymph or fibrin upon the free surface of the mucous membrane of the uterus.

"It is composed," says Dr. Churchill, "of plastic lymph, such as we see secreted by the mucous membrane of the trachea in croup, thrown off by the lining membrane of the uterus, and taking generally the form of the cavity of that organ, although it may be discharged in shreds."[1]

"We shall probably be correct," Dr. Montgomery observes, "in referring such productions [dysmenorrhœal membranes] to any cause capable of exciting a certain degree of irritation, or perhaps of inflammation, by which fibrin is poured out on the internal surface of the cavity of the uterus, and assumes a membranous texture, as we find occurring in other hollow organs lined with a mucous membrane, as, for instance, in the intestines in cases of diarrhœa tubularis, and in the trachea and air-tubes."[2]

"The membranous shreds passed in some of these cases [of dysmenorrhœa] evidently," remarks Dr. Copland, "consist of plastic lymph thrown out in the cavity of the womb. . . . . . That a degree of inflammatory irritation exists in the internal surface of the uterus, even in the neuralgic form of the disease, is proved by the formation and expulsion of a false membrane in many cases of that form. That this membrane is induced by the similar state of inflammatory action to that which sometimes occurs in other mucous surfaces, and gives rise to a similar exudation, is most probable, notwithstanding the absence of other inflammatory phenomena, and the neuralgic character of the pain."[3]

In a number of cases, I have had an opportunity of examining from time to time the form and structure of these dysmenorrhœal membranes. Two or three years ago, my observations upon them led me to believe that they were not new or false membranes formed of coagulable lymph, and secreted by the mucous surface of the uterus, but that they in reality consisted of the superficial layer of the mucous membrane of the uterus itself, hypertrophied and separated. All my later observations have gone to confirm me in the same opinion, viz., that the productions in question are not the results, as is generally supposed, of fibrinous or plastic *exudations upon* the free surface of the mucous membrane of the uterus, but that they consist of actual *exfoliations* of that membrane itself.

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[1] Diseases of Females, p. 102, Edition of 1844.

[2] Signs of Pregnancy, p. 147.

[3] Dictionary of Practical Medicine, vol. ii. pp. 844, 845. See, also, Dr. Fergusson in the Library of Medicine, vol. iv. p. 311, "a not uncommon effect of dysmenorrhœa is the formation of *coagulable lymph* modelled to the shape of the inner surface of the uterus." Dr. Rigby's Essay on Dysmenorrhœa, p. 39, "*fibrinous exudations* every now and then attend these cases of Dysmenorrhœa."—Dr. Ashwell's Treatise on Female Diseases, pp. 106 and 107, &c.

The proof of this opinion rests upon different grounds:

First, The dysmenorrhœal membrane presents anatomical peculiarities that are never seen in any simple fibrinous or inflammatory exudation; and these anatomical peculiarities, on the other hand, specially pertain to, and are characteristic of, the structure of some mucous tissues, such as that of the uterus. One special illustration may suffice. Professor Reid, Krauss, and others, have shown, that the surface of the mucous membrane of the uterus is marked by numerous orifices of small tubular glands, crypts, or follicles, opening upon it (the uterine glands of some modern authors). This structure I have distinctly traced in different specimens of dysmenorrhœal membrane from different individuals.

Secondly, The general configuration and character of the surfaces of the dysmenorrhœal membrane are such as would result from the origin which I have attributed to it, namely, the exfoliation or detachment of the mucous membrane of the uterus. In those instances in which the membrane is thrown off in one piece, and without disintegration, it presents exactly the flattened triangular appearance of the uterine cavity. Its sides may be so compressed that the expelled mass at first appears solid; but a little careful dissection or maceration will readily show that it consists of two layers, and that there are the remains of a cavity between them. The interior of the cavity is smooth, and marked by the orifices of the uterine mucous crypts that I have above alluded to. Occasionally we can easily trace three large openings at its three angles, corresponding to the openings of the two Fallopian tubes and cervix uteri. But the external surface of the mass is rough and shaggy, marking the effects of dilaceration from the tissue of the uterus. Sometimes we see a piece discharged quite smooth on one surface, and rough on the other. When this is the case, we may be perfectly certain that it is a portion only of the membrane which has been expelled, or, at least, preserved for inspection. For, if the portion of mucous membrane lining the anterior wall of the uterus alone, or lining its posterior wall alone, be discharged and examined, and not that of the whole cavity, it will necessarily display the apparent anomaly alluded to. If the membrane is thrown off in broken or disintegrated fragments, as sometimes happens, it will be more difficult to trace the structural characteristics that I have mentioned. Another form of difficulty is occasionally produced by blood being infiltrated into or upon the dysmenorrhœal membrane. In some instances the membrane is found encased in one or more layers of coagulated blood; and if that blood has already become decolorized, and assumed a fibrinous appearances, mistakes might easily occur, provided the inquirer were not aware of this source of fallacy.

One of the earliest descriptions of the dysmenorrhœal membrane upon record is given by Morgagni. He gives an exact account of the appearances which it presented in the case of a "noble matron," long afflicted at the menstrual period with "pains like those of childbirth." Morgagni's description of the dysmenorrhœal membrane expelled on these occasions is so exact and excellent, that I shall perhaps be excused quoting it. "In almost the middle," as he states, "of the membranous flux, a membranous body, as it appeared, was discharged from the uterus; and that in such a form, and of such a magnitude, as perfectly corresponded to the triangular form of the uterus; being moderately convex externally; on which surface it was unequal and not without many filaments that seemed to have been broken off from the parts to which they had adhered, but internally hollow; on which surface it was smooth and moist, as if from an aqueous humor, which it had before contained, but had discharged, at its own exit, by an ample opening, which was at one of its angles, that had been readily opened by rupture."[1]

Thirdly, The dysmenorrhœal membrane exactly resembles the decidual membrane, the *decidua vera;* and all our highest authorities in anatomy are, I believe, now willing to grant that, as pointed out by the researches of Sharpey, Weber, Goodsir, and others, the decidua vera is not a new membrane, formed in the uterus after conception, but merely the normal mucous membrane of the uterus, hypertrophied, with its mucous crypts or follicles increased in size, and the cells of its interstitial tissue greatly developed and multiplied. In the dysmenorrhœal membrane the mucous follicles or crypts are perhaps not enlarged and developed to the same proportionate degree as they are in the decidual membrane. In other respects the two membranes are identical. They have the same triangular form. There is the same appearance in both, of openings at their three angles, and in both, these openings are occasionally more or less perfectly sealed up when the tissue of the membrane, in their immediate neighborhood, is developed in an unusual degree. The external surface of each membrane has the same shaggy, ragged form. In each we have the same cribriform appearance upon their smooth internal surface, marking the orifices of the mucous follicles. When examined under the microscope, the interstitial or interfollicular tissue of both membranes, shows a similar structure, namely, one wholly composed of an agglomeration or superposition of simple nucleated cells. And altogether, if, on the one hand, it be allowed that the structure of the decidua proves it to be the mucous membrane of the uterus in a state of high development and hypertrophy, then, on the other hand, the structure of the dysmenorrhœal membrane is so similar to that of the decidua, as to prove a perfect identity with the decidua in its characters, and, consequently, also in its origin.

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[1] Morgagni, "The Seats and Causes of Diseases," &c. vol. ii. p. 706.

In some respects the evidence which we have in favor of the decidual membrane being merely a hypertrophied state of the mucous membrane of the uterus, is still wanting, in so far as regards the dysmenorrhœal membrane. For, *first*, in cases of patients dying at different periods of early pregnancy, a regular progression of observations has now been made, showing the gradual transformation of the true mucous into the true decidual membrane; and, *secondly*, in patients dying after delivery, and, consequently, after the separation of the decidual or lining membrane of the uterus, the actual absence of the mucous surface of the uterus has been often ascertained on dissection. I lately saw a case where the patient died six weeks after delivery, and still, at that late date after confinement, the mucous lining of the uterus was not yet regenerated. No corresponding series of observations has hitherto been made upon the actual formation of the dysmenorrhœal membrane before menstruation, or upon its actual absence after that period. But a more careful investigation of the state of the uterus after death, in patients who have happened to be suffering under membranous dysmenorrhœa, during life, will, I have no doubt, afford the requisite data. It may not be uninteresting to add, that the absence of the mucous lining of the uterus in persons who have died after delivery, or who have been previously subject to membranous dysmenorrhœa, may have given rise to the strong opinions expressed in former times by several anatomists, and particularly by Morgagni, Chaussier, and Gordon, in regard to the human uterus not being normally provided with a mucous membrane. Not meeting with that membrane under some circumstances and in some cases, they were induced to doubt its presence under any circumstances or in any cases.

Modern physiology has made us sufficiently acquainted with the curious fact, that a portion of the epithelial layer of the mucous surface of various organs, exfoliates constantly and normally during the performance of the special functions of these organs. For instance, this holds true with regard to the epithelium of the stomach during digestion, and that of the uterus during menstruation. But there are few circumstances, either in healthy or morbid anatomy, so strange as that which I have attempted to prove in the preceding remarks, namely, that the proper mucous tissue of the uterus itself may, within the compass of a menstrual period, form, enlarge, separate, and again be reproduced; and further, that all this may occur and continue regularly for a succession of months, or, as sometimes happens, for a succession of years.

I have no intention, however, at present of dwelling either upon the various pathological or practical views to which the opinion that I have above propounded regarding the origin and nature of the dysmenorrhœal membrane, very evidently points. It is enough perhaps to remark, that the observations which I have made go to demonstrate that the dysmenorrhœal membrane is not formed, as is generally believed, by a simple inflammatory effusion of plastic or coagulable lymph, and hence is not to be successfully prevented and combated by simple anti-inflammatory treatment. The action giving rise to it may in some cases be combined or complicated with inflammation. I have seen, for example, the membranous dysmenorrhœa in several instances coexisting with inflammatory induration and ulceration of the cervix uteri. But essentially, the normal action of the uterus or ovaries giving rise to the formation of the dysmenorrhœal membrane is not a state identical with inflammation, but a state identical with the condition of these organs after impregnation and during the earlier weeks of pregnancy. It is so far a state and product natural to one special condition of the uterus, but here occurring at an unnatural time, under unnatural circumstances, and with unnatural frequency.

The views entertained by Dr. Simpson as to the nature of the membrane expelled in some cases of dysmenorrhœa, have received singular confirmation from the more recent researches of M. Lebert and Dr. Handfield Jones. The latter found the membrane composed of epithelial "scales, resembling those of the healthy buccal mucous membrane"[1]—the former, besides the epithelial particles, has traced distinctly the presence of the shed uterine follicles and glands.[2] We may add, that on more than one occasion we have ourselves observed, with the aid of the microscope, the simple utricules, and more tortuous uterine glands, in dysmenorrhœal membranes, furnished us by Dr. Simpson; and have preserved drawings of such preparations.—(Ed.)

[1] See Transactions of the Pathological Society of London, vol. iii. p. 392.

[2] See Gazette Medicale, July, 1850, p. 557.

DILATATION AND INCISION OF THE CERVIX UTERI IN CASES OF OBSTRUCTIVE DYSMENORRHŒA.[1]

(From Edinburgh Monthly Journal of Medical Science, August, 1844, p. 734.)

DR. SIMPSON, in speaking of dilatation of the os and cervix uteri as a means of treatment, pointed out the results of this practice in the hands of the late Dr. Mackintosh in the cure of dysmenorrhœa and sterility, connected with normal and inflammatory strictures of the os uteri. His own results had not been so successful as those of Dr. Mackintosh; but he had now seen a considerable number of severe cases, in which dysmenorrhœa that had previously resisted all other kind of treatment, had at once yielded to the mechanical dilatation. Dr. Simpson had found the stricture occasionally at the os internum, or opening between the cavities of the cervix and body, and not at the os tincæ. Dr. Mackintosh had effected the dilatation by long straight bougies of different sizes. Dr. Simpson had found them more easily used when slightly curved. He also showed other instruments, one of them like the dilator for the female urethra, which he had occasionally employed for this purpose. These instruments were all of them intended to be left in the os uteri for only a short period, and their introduction repeated from time to time, as in the usual treatment of stricture of the urethra in the male. Latterly Dr. Simpson had in his practice thrown these aside, and used another, a form of permanent bougie, for this purpose, which he considered to be greatly preferable. These permanent bougies (Fig. 20) were made of Berlin silver; the stem or part included in the uterine cavity was two inches and a quarter in length, the lower end which rested in the vagina was bulbed and enlarged to the size of a large almond, and was perforated below for the purpose of being placed on a temporary handle, used in the introduction. One of the instruments was left in the uterine cavity for three or four days, and by that time the part was so much relaxed, that another of a much larger size could, in general, be readily introduced. They could easily be borne without the slightest inconvenience, and, indeed, without the patient being aware of their presence. Dr. Simpson pointed out that this permanent form of bougie gave altogether much less pain to the patient and less trouble to the practitioner; was more certain and expeditious in its effects, and was especially useful when the surrounding tissues of the lips and cervix were in any degree indurated. Obstructive dysmenorrhœa sometimes depends on other circumstances than ordinary strictures of the os. It is sometimes seen in connection with the conical hypertrophy and elongation of the cervix. He had found it in several instances accompanied with much morbid thickening of the anterior lip of the os uteri, the posterior lip being thin and healthy, and the os stretched out between them of an irregular crescentic shape. In such, and other instances, he had divided the os uteri on each side to the extent of a few lines with a very narrow knife or "*lithotome caché*," and subsequently kept the part temporarily dilated by the sponge-tent. He quoted cases of the perfect success of this simple and safe measure; it placed the parts in somewhat the same condition as that which they present subsequent to miscarriage; and this latter occurrence is known in general to leave without dysmenorrhœa those who have previously been laboring under that affection, whilst at the same time, women after aborting usually soon again become pregnant, there being no such great lacteal determination to the mammæ as occurs after parturition at the full period, and which seems then usually to interfere with the early repetition of conception.

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[1] Extracted from Proceedings of Medico-Chirurgical Society of Edin., July 3, 1844.

From the Proceedings of the Edinburgh Obstetric Society, March 10, 1847, we have also the following:[1]—Dr. Simpson stated that he had now been in the habit for three or four years past of performing the operation of incision of the cervix uteri for obstructive dysmenorrhœa. He first described the operation to the Medico-Chirurgical Society in 1844, and it had latterly been adopted by Dr. Rigby, Dr. Protheroe Smith, Dr. Oldham, and other accoucheurs in London and elsewhere. He had frequently been asked if he had occasion to perform the operation often. Certainly he had. In the last week he had operated in seven cases. He was in the first instance led to incise, instead of dilating the os uteri with bougies, by meeting several years ago with a patient suffering from dysmenorrhœa, who could only remain a few days under his charge. The incisions had in this instance the desired effect; and the lady was delivered of a son within a twelvemonth. She had been previously six or seven years married, but had never been pregnant.

He further explained that he believed the sufferings in obstructive dysmenorrhœa to arise from the uterus being driven into contractions, like those of abortion, to expel its own *retained* menstrual secretions. Now, the menstrual secretion need not *necessarily* be retained when the os uteri is small; for the secretion might form very slowly, and so escape without accumulation and distension. On the other hand, it might be secreted so abundantly by the lining membrane of the uterus, as not to escape sufficiently freely even when the os was of the natural size, and thus, under that condition, lead to retention, accumulation, and expulsive pains. In fact, in order to produce obstructive dysmenorrhœa, there must be a want of relation between the quantity of fluid secreted, and the quantity allowed to escape, so that a greater or less degree of retention is the result. It was of course most apt to occur with a small and contracted os uteri, and these were the cases most frequently requiring the operation.

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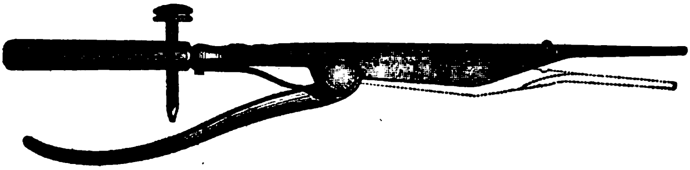
[1] See Edinburgh Monthly Journal of Medical Science, May, 1847, p. 870.

The instrument which he makes use of is a kind of lithotome caché, manufactured by Mr. Young, cutler. The end of the instrument is passed up through the cavity of the cervix, and within the os internum. It is then slightly opened laterally, first on one side and then on the other, so as to divide any fibres that may be causing constriction of the internal orifice. The principal incision is then made in withdrawing the instrument. This incision commences at the union of the cervix with the body of the uterus, and passing gradually more and more into the substance of the cervix as it descends, the blade is brought out at the outer and lower edge of the cervix, at the point of reflection of the mucous membrane upon the wall of the vagina. The instrument is then turned; and a similar cut made on the other side; or the incisions may be made antero-posteriorly instead of laterally. The incision is thus of a conical form, and at its lower part includes the whole thickness of the cervix. Care must be taken that it does not pass beyond the substance of the cervix, as it is closely surrounded by a plexus of veins, which, if cut, would certainly cause severe hemorrhage. If care be taken to regulate the incision in this way, the hemorrhage is usually very trifling. The operation causes little or no pain. The lips of the wound generally get everted, and have very little tendency to cohere. If they offer to do so, it is necessary to touch the raw surface, more especially the angles of the wound, every three or four days, with nitrate of silver.

Fig. 23.



CASE OF ACCUMULATED AND RETAINED MENSTRUAL SECRETION.[1]

(From Edinburgh Monthly Journal of Medical Science, April, 1850, p. 388.)

Dr. Simpson exhibited to the Society a very large quantity of a thick viscid fluid, dark-red in color, which had been removed that day by Mr. Syme from a young woman in the Royal Infirmary.

The girl was nineteen years of age, had never menstruated in the usual way, and made great complaints of severe pains in the back and loins, which were much exacerbated every three or four weeks. About five months ago she was for the first time sensible of a swelling in the hypogastrium about the size of the fist. Since then it had gradually increased, but chiefly at the periods of the monthly exacerbations. On examination, the hymen was found perfect and entire, and the vagina and uterus greatly dilated by the accumulated menstrual secretion. The hymen was divided by Mr. Syme, and the fluid exhibited was evacuated.

Dr. Simpson stated that the same operation had been performed by him in the Maternity Hospital, in a case where the cause of occlusion was adhesion and contraction of the vagina from sloughing, following a previous difficult labor. The quantity of fluid evacuated was almost as great. This patient, like many others who underwent this apparently simple operation, had suffered under a smart attack of irritative fever after it. Dr. Ramsbotham[2] had collected five or six cases, in which this irritative fever had gone on to a fatal termination.

GALLIC ACID IN MENORRHAGIA.[3]

(From Lond. and Edin. Monthly Journal of Med. Science, July, 1843, p. 661.)

Professor Simpson stated that for the last year he had employed gallic acid in some cases of menorrhagia, with the most successful results. Like all the other remedies directed against that disease, it had also occasionally failed in his hands. Some of the cases, which had completely yielded under its use, were of an old standing, and aggravated description. He gave it during the intervals, as well as during the discharge, in doses of from ten to twenty grains per day, made into pills. It had this advantage over most other anti-hemorrhagic medicines, that it had no constipating effect upon the bowels. He was first induced to use it, by finding a case of very obstinate menorrhagia get well under the use of Ruspini's styptic, after many other remedies had utterly failed, and from it being alleged that gallic acid was the active ingredient in that styptic. He suggested whether the anti-hemorrhagic properties of some of our common astringent drugs may not depend upon the gallic acid as much or more than upon the tannin which they contain, or upon the tannin becoming changed into gallic acid within the body.

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[1] Extracted from Proceedings of Edinburgh Obstetric Society, February 13, 1850.

[2] Lecture on Diseases of Women in London Medical Gazette, vol. xvi. p. 327.

[3] Extracted from Proceedings of Medico-Chirurgical Society, April 19, 1843.

At a subsequent meeting of the Society, February 7, 1855,[1] Dr. Simpson stated, as the result of his experience in cases of hemorrhage, that gallic acid was only occasionally successful. Some ten or twelve years ago, he had had occasion to treat a case of severe menorrhagia, where all kinds of medicines had been used without effect. From the recommendation given by Sir Benjamin Brodie in his chapter on *Hœmaturia*, he prescribed Ruspini's styptic; and relief speedily followed. The disease recurred, however; and the patent medicine was found to be too expensive. Acting on the hint of Dr. Anthony Todd Thomson, that Ruspini's styptic contained it, he gave gallic acid, and similar benefit was obtained. Dr. Gairdner said that tannin had greater effect as an astringent in the treatment of hæmoptysis; but this it was difficult to reconcile with the observation of Wöhler, that tannin was converted into gallic acid in its passage through the circulation, and consequently before it reached the lungs. He believed in fact that rhatany, krameria, and other vegetable astringents containing tannin, acted as secondary hæmostatics upon the lungs, kidneys, and uterus, by being carried in the blood of these parts as gallic acid; and that their efficacy in restraining hemorrhage was thus to be explained. Dr. Andrew Wood had stated his experience of the efficacy of the acid in purpura; while it was well known that Garrod had as strongly recommended alkalies in that disease. Such statements he thought could be reconciled by supposing that there might be several forms of purpura, each with their stronger affinity to particular remedies; and he believed that the uncertainty in the treatment of menorrhagia admitted of a similar explanation. After such astringents were given by the stomach, just as after gallic acid, gallic acid, as stated by Liebig, was found in the urine; and consequently was present in the blood.

A paper by Dr. Gairdner upon gallic acid in albuminuria and hæmoptysis having led to some discussion, Dr. Simpson also said that the case which had first impressed his mind with the efficacy of gallic acid in the treatment of albuminuria was that of a brother practitioner who labored under the disease, and was compelled to relinquish practice in consequence. Gallic acid was at length tried, and with the best results. He resumed practice some eight or ten years ago, and to this day remained comparatively well, and had no symptoms of albuminuria. The cure in this instance was very probably, however, a mere coincidence, as he himself had never perceived the slightest benefit from the use of the remedy in any of his other cases of this disease. Besides, there were many sources of fallacy; as, in acute cases of albuminuria, the patient generally got speedily well; and, in the more chronic forms of the disease, it was long before the health was seriously interfered with. One patient he knew who suffered from Bright's disease so long ago as 1826, and Dr. Bright himself had predicted a speedy and a fatal issue. The gentleman was still alive, and one of the strongest individuals in his native town, though the albuminuria remained as before. Dr. Gairdner had exhibited the acid with alkalies—a combination which, according to the observations of Liebig on the effect of gallic acid on the blood, must destroy its efficacy as an astringent.—(Ed.)

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[1] See Association Journal, Feb. 1855, p. 186.

DIRECT LOCAL APPLICATION OF REMEDIES TO THE CAVITY OF THE UTERUS.[1]

(From Lond. and Edin. Monthly Journal of Med. Science, July, 1843, p. 661.)

Dr. Simpson stated to the Society that he had of late applied nitrate of silver, &c., to the lining membrane of the cavity of the uterus, in cases of uterine leucorrhœa, and of dysmenorrhœa, connected with a morbidly sensitive state of portions of the inner surface of the organ, as ascertained by the bougie, and with membranous, sub-inflammatory effusions; in chronic suppression of the menstrual discharge, &c. The results proved, that while *direct* local applications could thus be made with perfect ease and safety to the diseased lining membrane of the uterine cavity, the effects were such, as to lead to the hope of a successful issue in some cases of uterine disease, otherwise almost or indeed totally unmanageable.

In applying nitrate of silver and other solid powders and substances to the interior of the uterus, Dr. Simpson uses an instrument similar to the *porte-caustic* employed by Lallemand for touching sensitive parts of the male urethra.

We may here state, that for some time past, Dr. S. has been in the habit of applying various medicated substances in the form of *powder*, to the cervix uteri and vagina, in cases of leucorrhœa dependent upon vaginal eruptions, &c. Powders of oxide of zinc, subnitrate of bismuth, &c., when thus freely applied, act most successfully in some instances of obstinate leucorrhœa. They are readily applied with the bone tube and piston usually employed in leeching the os uteri.—(Ed.)

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[1] Extracted from Proceedings of the Edinburgh Medico-Chirurgical Society, April 19, 1843.

NOTES ON THE THERAPEUTIC ACTION OF FURFURINE, NICKEL, ETC.

(From Monthly Journal of Medical Science, August, 1852, p. 135.)

FOR many centuries past, the principal and most important additions which have been made to the Materia Medica have been in the form of substances or principles derived from the vegetable kingdom. But new, and perhaps still more potent, remedies will in all probability be yet derived from other sources. Chemistry is daily multiplying upon us the already almost innumerable class of organic compounds. Some of these compounds will no doubt yet be found to possess powerful and important therapeutic properties. Chloroform is an instance to which I may venture to refer in illustration of this remark. Again, among the most potent therapeutic instruments in our present pharmacopœias are to be reckoned the vegetable alkaloids. Modern chemisty has, as is well known, found out the means of forming artificially several alkaloid substances analogous to those existing naturally in the vegetable kingdom. Upon some of these I have made experiments, but particularly upon furfurine—an alkaloid that produces, in experiments with poisonous doses upon the lower animals, many of the symptoms of quinine; and the salts of which I have found to act as a tonic, if not as an antiperiodic, when exhibited to the human subject.

Our modern pharmacopœias contain preparations from the oxides or salts of various metals, as antimony, arsenic, bismuth, copper, iron, lead, mercury, silver, and zinc, besides metalloids and metallic earths. Some of these possess a most decided and valuable therapeutic action upon the human economy. But all, or almost all, of these metals were already used medicinally by the ancient Greek and Roman physicians; and though in later times the list of known metals has been greatly increased by the researches of chemists, the therapeutic action, if any, of these metals has not been made a matter of research by medical men. It seems, however, a priori, highly probable that some of the new, like some of the old, metals, will turn out to have decided, and it may be important therapeutic properties. At all events, few fields seem so likely to yield new therapeutic results—if such results are to be obtained at all—as experiments and observations upon the preparations and salts of those metals that have hitherto not been tested medicinally.

Last year, impressed with these views, I began making various therapeutic experiments upon myself and others with different metals, as cadmium, iridium, tellurium, &c. I have had little leisure to prosecute the inquiry. But I have obtained some results which seem to me not without interest or importance. One of the new metals which I have used most frequently is nickel. As with the others, I have used it generally in the form of a salt, a sulphate, believing that, as in the case of the metals already known, the use of *one* of its salts would give a sufficient view of the generic medical action of the metal.

Sulphate of nickel has appeared to me to act as a gentle metallic tonic. I have generally used it in doses of half a grain or a grain, repeated thrice daily; and have given it in the form either of simple solution or of pill. In large doses it is liable, like sulphate of zinc or copper, to produce sickness and nausea, particularly if taken upon an empty stomach. I have generally requested it to be taken half an hour or an hour after meals. It has appeared to me, as the result of pretty numerous experiments and observations, that the therapeutic actions of the salts of nickel and manganese correspond in a considerable degree with the therapeutic actions of the salts of iron upon the economy; and that these three metals might, under many conditions, be almost used as therapeutic substitutes for each other. But they also specifically differ from each other in some respects. For example, in one most interesting case, the sulphate of nickel arrested a severe form of periodic headache, which had previously defied iron in many different forms, and all other kinds of treatment that had been employed. The patient came from Italy last autumn, in order to place herself under my professional care; and for some months I was as unsuccessful as my predecessors had been in affording her any relief. But let me give the history of the affection, and the ultimate result, in the lady's own words. She drew up the following note of her case several weeks ago:—

"My headaches," she writes, "came on soon after my second confinement, in August, 1847, and continued to return every tenth day without intermission, up to the 1st of February, 1852. During the first four years I was in Italy, and was attended by medical men of all countries—English, French, German, and Italian. I also tried hydropathy and homœopathy, the latter for six months, but all without the slightest effect. The pain came on in a small spot on the right temple, and lasted from twenty-four to thirty-six hours. After the first eight hours, severe sickness followed, which continued up to the sixteenth hour. During the attacks I had violent cold shivering fits, succeeded by a burning fever. At times I was quite delirious from the violence of the pain. I have taken large doses of steel, iron, and quinine, besides many other sorts of medicines. The quinine I took at first only two days before the attack was expected.

I then took six grains every day for a year and a half, but it never put off the headache a moment beyond its day and hour, nor would anything that I could do bring it on before the time. When I first came to Scotland to be under the advice of Dr. Simpson, in August, 1851, he gave me thirty grains of quinine a day for three days before the headache was to come on; but it returned to its hour, and as severe as ever. This was tried also with the next fit, with no better success. Dr. Simpson then tried successively furfurine, bebeerine, and arsenic, but the headaches still continued up to the 1st of February, 1852, on which day I had a most severe attack. On the 4th of February, he gave me the solution of sulphate of nickel to take; since which time, to my astonishment, my usual headaches have altogether disappeared."

To the preceding account I have merely to add, that, if we may judge from the result up to the present time, the cure of this patient by the use of nickel appears entire and complete. And perhaps it is but proper to remark, that this result seems fairly attributable to the action of the nickel alone, inasmuch as there was no relief under the use of any of the means or medicines previously employed for years; while convalescence distinctly began from the date of the employment of the metal in question.

Further, it is perhaps not unimportant to observe, that while the disease had lasted four years without abatement, its subsidence in February could not be the result of change of climate, as the lady had already resided about five months in Edinburgh or its neighborhood, without any noticeable amelioration in the recurrence and intensity of the headaches; and at last they disappeared under the nickel, at a period of the year, viz., the commencement of spring—at which, in our climate, headaches and other periodic diseases are known to be specially liable to become increased and aggravated.

In no kind of case is the beneficial action of iron more remarkable than in the treatment of chlorosis and amenorrhœa. I have seen nickel in a similar way apparently serviceable under the same circumstances. In the latter end of last year, I gave it in a case of amenorrhœa of ten years' duration. The amenorrhœa supervened at the age of twenty-two. At the same time a galvanic intra-uterine bougie was introduced, and left for some time in the cavity of the uterus. In the course of three or four weeks menstruation took place, and has recurred regularly from that period. In such a case, however, it is difficult to say how far the result was attributable to the local means used, and what share the nickel had in the restoration of the patient's health.

SPURIOUS PREGNANCY—ITS FREQUENCY AND NATURE.[1]

(From Edinburgh Monthly Journal of Medical Science, July, 1850, p. 90.)

DR. SIMPSON first showed how frequent and marked the phenomena of spurious pregnancy were among domestic animals, as in the bitch, cow, &c. In these animals such phenomena often occurred both subsequently to unsuccessful sexual intercourse, and also frequently after seasons of heat, during which there was no intercourse with the male. Occasionally the symptoms were in them suddenly interrupted, and terminated a short time after their commencement; sometimes, however, they went on increasing to the full term of pregnancy, and ended in a series of puerperal phenomena, as the presence of milk in the mammæ, &c. &c. He adduced a variety of facts, showing the great frequency with which the same symptoms of spurious pregnancy occur in the human female in a more or less marked degree, and how they are often mistaken for dyspepsia, hysteria, and various anomalous and perplexing ailments. And as these phenomena sometimes occur in the virgin state among the lower animals, so do we find them also sometimes occurring in the unmarried human female, and giving rise to symptoms that have hitherto defied any nosological arrangement. Lastly, he spoke of the great use of aperients combined with tonics, and with nux vomica and galbanum, in the subjugation of the phenomena of spurious pregnancy.

The following further remarks on this subject were made by Dr. Simpson, April 5, 1854.[2] A case of hysteria and spurious pregnancy having at that time been read by Dr. Keiller, before the Medico-Chirurgical Society of Edinburgh, the President, Dr. S., observed that he believed cases of spurious pregnancy were often met with in practice; for many a married lady would acknowledge, when questioned, that she had once or twice thought herself pregnant when, as the result showed, she was not. He had in his own practice seen several cases in which ladies had removed into town with their entire establishments, in the full belief of their approaching confinement, and where he had had the disagreeable duty of informing them that they were not in the family way. Shortly before his predecessor, Dr. Hamilton, died, one lady, in her anxiety to reach Edinburgh, had had the roads cleared of snow for a long series of miles, to enable her to accomplish the journey, which proved after all a useless one, as Dr. Hamilton pronounced her not pregnant. These were instances in which patients progressed onwards to near the full term of pregnancy, suffering all the usual symptoms and discomforts of that state. Sometimes phenomena exactly like those of labor came on at the full term. This seemed to have occurred in Dr. Keiller's patient. He had seen several analogous instances of spurious pregnancy terminating in spurious parturition. In one of the first cases of this kind which he had witnessed, and which occurred in the Maternity Hospital of Edinburgh, he had been suddenly called from lecture to see it; the case being reported by the house-surgeon, a man of remarkable acuteness, to be one of placenta prævia, and requiring the operation of turning. The patient had the phenomena of labor present, with severe menorrhagia; but there was no child to turn, as she was not pregnant. Besides the cases of pseudo-pregnancy, in which, as in the above, the patient went on with the usual symptoms of pregnancy till near or up to the full period of labor, there were other varieties of this curious morbid state. In some, for example, the affection lasted only for a few months; in others, it occasionally continued far beyond nine months, and became, as it were, chronic in its character. As to the symptoms themselves, they consisted of the presence of more or fewer of all the usual sympathetic symptoms of pregnancy, as swelling of the abdomen, nausea and sickness, a feeling of quickening and motion of the child, &c. Dr. Keiller had stated that the mammary signs were not well marked in his patient. Sometimes, however, they were; and he had sketches illustrative of this fact, executed by the patient herself during a state of spurious pregnancy, and in whom, in a subsequent veritable pregnancy, her first, the areolæ did not present a deeper tint than they had done during her pseudo-pregnancy. As to the sensations which had been described in such cases, they were very frequent, and sometimes there were true motory contractions in the abdominal walls. He saw, about a year ago, with Drs. Moir and Weir, a patient who had been sent from the country on account of a supposed difficulty in the delivery, the woman having, according to her statement, been in labor for three days. In this case, there were very strong motions visible in the abdominal walls; so much so that the husband, who was present during the consultation, declared, on their affirming that there was no child, that then there must be an animal inside his wife. Dr. Simpson was inclined to believe that the malady was connected with the ovary. In one aggravated case which had fallen under his observation, there was marked ovaritis, and the ovary subsequently suppurated. Again, it was observed that, although menstruation did occur in the cases which had been noticed, yet that it was much scantier than usual, and sometimes the catamenia were wanting for several months. It was well known that the complaint was not peculiar to the human female. Harvey had long ago remarked that, in hounds who were well fed, many of the phenomena of pregnancy, such as swelling of the abdomen, and the presence of milk in the mammæ, occurred both subsequently to unsuccessful sexual intercourse, and also frequently after seasons of heat, during which there was no intercourse with the male; kittens and other young animals were frequently stolen by the animal, to make the semblance of a litter. The curious observation had also been made by Harvey, that the animals were liable to the diseases of bitches which had recently been delivered. Dr. Simpson stated some similar facts in the case of the domestic cow. The symptoms of spurious pregnancy often occurred in the virgin female dog. And he believed that, when some of the cases of the so-called hysteria in unmarried females were inquired into, they would be found, as he thought he had repeatedly seen, to be really symptoms only of spurious pregnancy. Dr. Keiller had alluded to retraction of the limb as having been a well-marked symptom in his patient. He himself had seen a case many years before the introduction of chloroform, in which the symptom was a prominent one. The lady had been under treatment in Paris, and, on her return to Scotland, a surgeon examined her, and recognized, as he supposed, the presence of a large ovarian tumor—not an uncommon mistake, as, in the records of ovariotomy, six cases are detailed in which no tumor could be found after the abdomen was opened. Dr. Simpson was consulted, and, on percussing, he found the abdomen quite tympanitic, and of course negatived the tapping, and the presence of any ovarian tumor. The leg in this case was much drawn up, and he was informed that, while in Paris, the heel was for a time closely applied to the back of the neck. About two years ago, he saw a lady, with Mr. Syme, whose limb was strongly and permanently retracted, and presented the appearance of hip disease. He was uncertain whether there was abdominal fulness in this case, or not. The appearances were so deceptive, that a practitioner had used the exploring needle for the purpose of evacuating matter. As soon as the patient was brought under the influence of chloroform, however, the limb was readily straightened for the first time for many long months, and the case was at once ascertained to be one of "hysterical disease" of the joint. With regard to the nature of the abdominal swelling in spurious pregnancy, he had tried various experiments to ascertain its cause, but in vain; and as yet he could come to no decided conclusion on the subject. It had been suggested that, while the patient was deeply under the influence of chloroform, contained air escaped unobserved; but, in one very marked case in the hospital, he had passed a tube per anum, its nozzle being kept under water; but not a bubble of air escaped. The diaphragm he was inclined to suspect to be a chief agent in the production of the swelling. One patient who had a small fibrous tumor of the uterus, and a rounded tympanitic abdomen, could greatly diminish the prominence of the abdomen by drawing herself up, and, on allowing the diaphragm to fall, the fulness and the appearance of pregnancy returned. The chloroform, he believed, here acted by relieving the muscles, diaphragmatic and abdominal, from the influence of reflex action, and permitting their relaxation. In one case, he had also seen the Cæsarian section proposed, though in somewhat different circumstances from the case of Dr. Keiller. The patient was dying from another disease, but spurious pregnancy was present; she was positive about the child's being alive, and he had been asked to operate after the mother's death to save the child. On a vaginal examination, &c., the uterus was found small and unimpregnated. The idea of some living animal being contained within the abdomen, in such cases as in Dr. Keiller's patient, was not unfrequent. On two or three occasions, such a belief had been expressed to himself. Dr. Keiller had classed the interesting case which he had communicated under the head of hysteria; but some of the cases to which he himself had alluded could scarcely be properly included under that designation. The phenomena were common to the females of our domestic quadrupeds, and in them would not be designated hysteria. Dr. Simpson suggested that a series of experiments should be made on the bitch during the occurrence of the anomalous pregnancy, to ascertain, by examination of the state of the ovaries and uterus, the true nature of the cause. He thought that some cases of membranous dysmenorrhœa might be referred to the same category as the one under consideration; for in some patients, besides the throwing off a membranous structure or maternal decidua, the other constitutional symptoms were also present. In some cases, the similarity to true pregnancy was very marked even in special and minor details. In a case from Ayrshire, which he had lately seen with Dr. Taylor, a peculiar eruption of intense prurigo was present, which the patient declared had hitherto only appeared when she was really pregnant. He concluded by asking if any member could suggest a rationale of the occurrence of the abdominal swelling, or explain the peculiar effect produced upon it by the inhalation of chloroform?

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[1] Extracted from Proceedings of Obstetric Society of Edinburgh, March, 1850.

[2] See Association Medical Journal, April, 1854, p. 357.

Dr. Simpson had made some experiments on the subject in the wards of the hospital, and had satisfactorily ascertained that, in the cases he had examined, the prominence of the abdomen did not depend upon any arching of the spine.

For the aid afforded by anæsthetics in the diagnosis of spurious pregnancy, see page 100. —(Ed.)

FATAL VENOUS HEMORRHAGE FROM THE PUDENDA.[1]

(From Edinburgh Monthly Journal of Medical Science, February, 1850, p. 195.)

Dr. Simpson related the circumstances of a fatal case of this nature, as reported to him by Dr. Kyle of Dundee, who was called to see the woman, but did not arrive till after she had expired. No grounds could be discovered for any suspicion that the woman had received a wound. She was in the lower ranks of life, but respectable, and living on good terms with her husband and neighbors. She had been straining at the night-stool when the hemorrhage came on. A large quantity of blood was found about her person;—it had flowed from the genital organs. On making the autopsy, Dr. Kyle paid particular attention to the state of the uterus, which was fully expanded in pregnancy; but no effused blood was discovered in or around it. On examining the vagina and vulva, Dr. Kyle found a recent aperture in one labium, which, on further dissection, he traced into a large vein.

Dr. Simpson alluded to the anatomical fact, that there was at the root of each labium a plexus of very large veins, which extended some way into the vagina. One of these veins, probably in a varicose state, had burst in this instance. Possibly the coat of the vein was thickened, as well as dilated, and, consequently, it would not collapse, as veins usually do, but remained open like an elastic artery.

Dr. Simpson further stated, that the case seemed to him particularly interesting and important in relation to medical jurisprudence. A number of criminal trials had taken place in Scotland, within the memory of the members, in consequence of women, generally, but not always, pregnant, having died from hemorrhage from the pudenda, similar to the above. In most or all of these cases it had been averred that the wound had been inflicted, with criminal intent, by the husband or others. Dr. Watson[2] has recorded two or three such cases; Dr. Seller has recorded others; and he himself had seen the examination of the body in two criminal cases of this kind. In both, the women bled to death from very small wounds of the pudenda. He was not aware that in any of the five or six cases, of late years tried before the Scottish courts, the plea of the apparent wound being a spontaneous rupture had been adduced. But such a case as this, that had lately occurred at Dundee, had evidently important bearings on the value of such a plea.

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[1] Extracted from Proceedings of Obstetric Society of Edinburgh, December 12, 1849. See also Proceedings of Obstetric Society, May 12, 1847, reported in Edinburgh Monthly Journal of Medical Science, August, 1847, p. 137.

[2] Edinburgh Medical and Surgical Journal, vol. xxxvi. 1831.

BALL-VALVE OBSTRUCTION OF THE RECTUM BY SCYBALOUS MASSES.[1]

(From Edinburgh Monthly Journal of Medical Science, April, 1849, p. 705.)

Dr. Simpson described a recent case of this not uncommon complication which he had seen with Dr. Husband. The patient, when Dr. Simpson visited her, was suffering from intermitting paroxysms of abdominal muscular effort, like those of labor. Dr. H., on making a vaginal examination, had detected the impacted and distended state of the rectum. A large, hard, oblong, scybalous mass filled up the canal of the rectum. It was incrusted with phosphatic deposit on its external surface. The rectum was in consequence irritated by it, and tender under examination. The patient was chloroformed, the mass broken down by an iron instrument, and with some difficulty removed piecemeal. Subsequently, some smaller and higher-placed masses were expelled by the action of aperients, and the patient was relieved from her sufferings.

Dr. Simpson alluded to some other cases of the same kind which he had seen. Such ball-valve obstructions often caused great distress to the patient; and the disease was occasionally altogether overlooked for a time, and its nature mistaken. This was more particularly liable to happen in consequence of the circumstance, that a small quantity of thin feculent matter often escaped from day to day in these cases, leading to the idea that the uneasy sensations of the patient, and ultimately the paroxysmal and expulsive pains, could not be the result of obstruction in the bowel. Thin feculent matter in this way escaped, in consequence of the scybalous mass not necessarily occupying and shutting up the whole calibre of the bowel, but allowing the more fluid matter to pass between its surface and the sides of the intestinal canal. When, however, the patient used, as was instinctively done, any straining effort to make the expulsion more complete, the scybalous mass was by this means pushed down upon the lower and more contracted portion of the rectum, and acted exactly like a *ball-valve*, so far temporarily closing the bowel, and preventing any further evacuation from it for the time. Such patients could sometimes partially evacuate the bowel in a lying, when they could not effect it in a sitting posture, a result easily understood when we consider the mechanical character of the disease. It was an affection liable to recur from time to time in those who had been once subject to it.

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[1] Extracted from Proceedings of Edinburgh Obstetric Society, January 10, 1849.

PERITONEAL HYDATIDS IN FLUID REMOVED BY TAPPING.[1]

(From Edinburgh Monthly Journal of Medical Science, March, 1854, p. 285.)

Dr. Simpson showed a fluid containing a number of hydatids, which he had procured on tapping a patient, who had previously undergone that operation without the escape of any such fluid. On admission into hospital she presented fearful distension of the abdomen, more so, indeed, than Dr. S. had ever before observed. Fluctuation was present, more particularly in the centre of the swelling. On tapping, a large quantity of fluid containing hydatids was, with difficulty, evacuated. The patient subsequently died, when the hydatids were found to have been contained in the cavity of the peritoneum, and external to a large ovarian cyst. Their origin was traceable to the peritoneal basement membrane, from which they sprung; and in their process of growth they probably projected into the cavity of the peritoneum, and subsequently became detached. They appeared to belong to the genus *astoma* of Goodsir, and many of them presented in their interior curious thread-like bodies resembling vibriones.

CHRONIC PELLICULAR OR ERUPTIVE INFLAMMATION OF THE INTESTINAL MUCOUS MEMBRANE.

(From Edinburgh Monthly Journal of Medical Science, June, 1846, p. 462.)

Professor Simpson stated that observations he had lately made convinced him that this disease, pellicular intestinal inflammation, was not rare, as was generally believed, but, on the other hand, that it was often to be met with in practice. A characteristic symptom was the passing of shreds, or membranes, formed, as shown by the microscope, of plastic or coagulable lymph. He had also found in a number of cases, debility, great marasmus, palpitation, mental depression, and irritability, and in fact all the symptoms usually seen in cases where blood is long and slowly escaping from the system. He traced the symptoms not so much to the actual loss of blood, as to the circumstance that its formation was prevented, by the intestinal extremities of the lacteals being morbidly interfered with by the mucous pellicular inflammation of the bowel. He had treated the disease on the same general principles as are employed in chronic cutaneous eruptions, and had found counter-irritation of much use.[1]

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[1] Extracted from Proceedings of Edinburgh Medico-Chirurgical Society, Feb. 1, 1854.

Since specially pointing out some years ago to the notice of my professional brethren in Edinburgh the disease alluded to in the preceding brief notice, its frequency in practice has become generally recognized among us; and all, I believe, are now willing to acknowledge that it is infinitely more common than the total, or almost total silence on the subject of all our best writers on practical medicine would, *a priori*, lead us to infer.

Acute exanthematous eruptions—small-pox, measles, scarlatina, erysipelas, &c.—are usually recognized as occasionally attacking some parts of the mucous surface, as well as the general cutaneous surface of the body. And there are some specific local inflammations of the mucous membrane, which, if present on the skin, would, no doubt, there be termed eruptions—as diphtherite, dothinenterite, and perhaps more than one form of diarrhœa and dysentery, &c.

Chronic eruptions, however, of the intestinal and other mucous membranes of the body, have scarcely been acknowledged in modern pathology. But perhaps such chronic eruptions and irritations of the mucous surface will yet be found to be scarcely less frequent or less various in type than the well-known chronic eruptions and irritations of the cutaneous surface. (See p. 101).

Chronic eruptive inflammations of the intestinal mucous membrane are frequently attended, as stated in the preceding notice, with the ejection, in greater or less quantity, of shreds or pellicles of thickened mucus, or of actual coagulable lymph, along with the usual contents of the bowels; and sometimes this pellicular effusion presents the appearance of a gelatinous shapeless mass, or of portions of a roundish or tubular false membrane, which is frequently considered by the patient as "worms." Often, however, in apparently other species of chronic mucous or intestinal eruptions, no such secretion is thrown off.

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[1] Proceedings of the Medico-Chirurgical Society of Edin., April 1, 1846.

The pathological anatomy of these morbid eruptions of the mucous membrane has scarcely yet been at all studied on the dead body. In a case where, some months before death from pulmonary tubercular disease, the patient had passed large quanties of "membranous crusts or tubes" from the bowels, Dr. Abercrombie found the mucous membrane of the colon, throughout its whole extent, covered with an immense number of small spots of a clear white color, which, "on minute examination, were distinctly ascertained to be vesicles, very little elevated, but, when punctured, discharging a small quantity of clear fluid."[1] In a case of a still more chronic character, with similar pellicular discharges, which I attended with the late Dr. Wright, and where the patient died in an extreme state of marasmus, the mucous membrane of the colon and the lower portion of the small intestine was everywhere studded with a thickly-set papular eruption.

The principal general symptoms which I have observed in cases of chronic mucous or intestinal eruption, are the following, in different numbers and combinations, and in different degrees of severity in different patients:—

General indefinable debility and emaciation; a condition often of broken and impaired health, without any very appreciable cause; the muscular system easily fatigued and exhausted; sometimes so much palpitation as to lead to the idea of heart disease; the circulation weak, as shown by the coldness of the extremities, &c.; diminution of nervous and mental power and energy, with hypochondriasis, irritability of temper, very often impairment of the memory, sensations of prickling and semiparalysis in the arms or legs, and sometimes lesions of sense; skin very generally dry and inactive, and in some cases eruptions appear upon it, cotemporaneous with, or vicarious of, the internal mucous irritation. The appetite, provided the mucous membrane of the stomach itself is unaffected, may be found scarcely, if at all diminished, but the patient complains of the food swallowed not producing any corresponding amount of strength or nourishment; occasionally, again, there is marked dyspepsia; often, but by no means constantly, there is a feeling of heat and rawness, in some part of the intestinal canal, and a feeling of uneasiness and distension rather than pain, in the abdomen; the action of the bowels is sometimes comparatively normal or easily regulated, but they vary in other instances, both as to torpidity and irritability. The sleep is usually unrefreshing in proportion to its amount.

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[1] Researches on Diseases of the Stomach, case cxiv. p. 298.

Direct evidence of the presence of, and tendency to, mucous eruptions in such subjects, can generally be obtained by carefully examining the state of the mucous membrane that is within sight. Spots of eruption, and sometimes small ulcerations left by them, will frequently be detected on the inside of the lips and cheeks, and on the gums and tongue. Much more frequently the palate and throat present, more or less distinctly, the appearances of chronic eruptive disease; as likewise the mucous membrane of the nose. The tongue, with the mucous membrane lining the cheeks, is not unfrequently so swollen as to be marked and indented by the impression of the teeth. Sometimes, when thus enlarged, the tongue is whiter than usual; but in other cases we see it red and irritable; and, more rarely, one or more distinct and broad patches of eruption are seen on its surface. The mucous membrane of the mouth and throat seems often, in such cases, to be the seat of successive new crops of eruption; and the variation in the general symptoms of the patient would appear further to show that such is also probably the history of the disease on its more internal sites; these successive re-aggravations being sometimes accompanied by a slight degree of chronic feverishness. Sometimes there is a kind of daily periodicity in the morbid sufferings and feelings of the patient.

The general principles of treatment are, as already stated, the same as those used in chronic skin eruptions.

The affection—particularly in its occasional periods of aggravation—is allayed by the use of lime or Carrara water, by aqua potassæ, by subnitrate of bismuth, by doses of nitrate or oxide of silver, or of oxalate or nitrate of cerium; by bitter infusions, as that of quassia, with the addition of two or three drops of medicinal prussic acid; by the cold infusion of Virginian cherry bark, &c. But these medicines act perhaps principally as local sedatives to the diseased mucous surface.

As curative constitutional remedies in this affection, I have seen most advantage from the salts of cerium, from the use of pitch pills, or capsules of tar,[1] and from the preparations of arsenic.

The preparations of pitch or tar have always seemed to me most useful when they produced their characteristic scarlatinoid eruption on the skin.

But most reliance ought, so far as I am able to judge, to be placed on small and very long-continued doses of arsenic, as two drops of Fowler's solution, or a pill containing the sixtieth of a grain of arsenite of potass, taken three or four times a day. Either preparation should be taken with or after meals; and it is, I believe, infinitely better and safer to trust to the curative effect of the long continuance of such small doses of this remedy, than to attempt to arrive at the same result by throwing in larger doses for a shorter period.

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[1] In these cases of chronic mucous eruptions I was induced to use pitch or tar from knowing that, in the latter years of his practice, Dr. Willan trusted to it as his principal constitutional remedy in the treatment of chronic cutaneous eruptions.

After a length of time, and when the general symptoms are much abated, a more direct tonic, as quinine or iron, may be added to the cerium, pitch, or arsenic. But at first all tonic remedies appear to be entirely useless, or to lead even to the aggravation of the morbid state of the patient.

The diet requires to be regulated by the usual rules applicable to dyspepsia. But animal food, in a concentrated form, is often required to sustain the strength, provided it does not irritate. Wine or stimulants very seldom are of benefit. The state of emaciation is sometimes improved by food containing large quantities of fat, as cream, butter, olive and codliver oil, &c. When the patient's stomach will not bear or digest such fatty matters, I have seen the daily inunction of two or three ounces of warm olive oil into the general surface of the skin, followed by the very best effects upon the health and strength of the patient.

Most remedies will fail to produce a permanent remedial effect, unless the state of the skin be attended to, and its healthy condition restored by frequent sponging with warm water, or with warm stimulating lotions.

Lastly, external counter-irritation over the abdomen seems to be an auxiliary means of almost indispensable necessity. A mustard poultice every night at bedtime forms one of the best and simplest means of effecting it; or external counter-irritation with stimulating liniments, or with croton oil, or antimonial ointment, or a strong tincture of iodine, &c., may be used to fulfil this important indication.

From the nature of my practice, I have seen the disease far more frequently in the female sex, and often in patients suffering under obstinate leucorrhea, vaginal eruptions (see page 101), and other uterine diseases. But it also often occurs in the male subject, and especially, as it has appeared to me, in men who, like clergymen and others, are subjected to an unusual amount of intellectual work or mental anxiety.—(*April*, 1855.)

NOTE ON THE THERAPEUTIC ACTION OF THE SALTS OF CERIUM,

IN IRRITATIONS OF THE ALIMENTARY CANAL, ETC.[1]

Chemists are at present acquainted with forty-nine different metals. Preparations, in the form of oxides and salts, from seventeen of these metals are to be found in our British pharmacopeias. Most of the remaining thirty-two metals are of comparatively late discovery, and few, or almost none, have been tried medicinally. But, betimes, the oxides or salts of some, at least, of these newer metals will, in all probability, be found to possess therapeutic qualities as marked as the preparations, that have been so long used, of the older known metallic bodies.

In attempting to make some observations on the therapeutic character of the untried metals, I have been especially gratified by the results which I have found to be produced by the medicinal use of salts of cerium.

This metal, cerium, or cererium, was first discovered in 1803, and almost simultaneously by Hisinger and Berzelius, and by Klaproth. It has been found by these and other chemists to exist in a variety of minerals, as the Edwardsite, Cerite, Euxenite, Gadolinite, &c. The Greenland Allanite is said to contain thirty-four per cent. of cerium.

It is usually found in nature combined with two other metals, lanthanum, or lantanium, and didymium. In the complex process of separating the cerium from these two other metallic bodies, oxalic acid, or rather oxalate of ammonia, is employed in the last stage; and in the markets cerium is thus most readily procured in the form of an oxalate of the protoxide. In using cerium medicinally, I have generally employed the oxalate in the form of small pills, and in the dose of one to two grains, or a solution of the nitrate in water, and in the same proportionate doses.

The principal therapeutic action of the salts of cerium appears to me to be that of a useful sedative and tonic, or, if we may use such an expression, "sedative tonic,"—like the pharmaceutical oxide and salts of silver and bismuth. I have employed it pretty extensively in the treatment of the class of cases described in the preceding paper, chronic intestinal eruptions, and occasionally with the most marked good effect in them, in instances that had resisted all other forms of treatment. It is often useful also in common cases of irritable dyspepsia, gastrodynia, &c. In my own practice and in that of others, I have seen the above preparations of cerium succeed in at once allaying irritable and obstinate vomiting after all other means had failed. Sometimes I have observed it fail, like all other remedies, in giving relief in the sickness and vomiting of pregnancy; but far more frequently I have found its employment accompanied by direct beneficial effects; and I have now repeatedly had occasion to see it both immediately and perfectly successful in some instances where the usual succession of medicines,—prussic acid, naphtha, opium, bismuth, ice, &c.—had all been previously and perseveringly tried in vain.—(*April*, 1855.)

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[1] A short notice of the medicinal action of cerium, tellurium, &c., was read before the Medico-Chirurgical Society of Edinburgh by Dr. Simpson, Nov. 15, 1854.—See Edinburgh Monthly Journal, Dec. 1854, p. 564.—(Ed.)

ON THE ALLEGED INFECUNDITY OF FEMALES BORN CO-TWIN WITH MALES,

WITH SOME NOTES ON THE AVERAGE PROPORTION OF MARRIAGES WITHOUT ISSUE IN GENERAL SOCIETY.

(From Edinburgh Medical and Surgical Journal, January, 1844, p. 107.)

"It is," says Dr. Burns, "a popular opinion, and I do not know any instance to discountenance it, that if twins be of different sexes, the female is sterile." "I have never," he adds, "had an opportunity of examining the state of the uterus and its appendages after death."[1]

Some years ago I took considerable pains to collect a series of data for the purpose of testing the validity of the opinion alluded to by Dr. Burns in the preceding paragraph. The results of the inquiry were, in 1839, laid before the Edinburgh Medico-Chirurgical Society in the following form.[2] I venture to publish the observations now, with a few additions and corrections, under the idea that the subject is not devoid of interest in relation to physiology and legal medicine, and involves one or two correlative questions worthy of some degree of investigation. Besides, I know well the many difficulties connected with a statistical inquiry like the present, however brief and simple it may appear when condensed into its ultimate results; and the publication of the evidence that I have obtained upon the topics in view may probably be fortunate enough to save others from expending time and trouble upon the same research.

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[1] See the last edition (1843) of his well-known Principles of Midwifery, p. 236.

[2] In an article on Hermaphroditism in Dr. Todd's Cyclopædia of Anatomy, Part xvi. (1839) p. 736, I have stated some of the results of the earlier part of this inquiry. See vol. ii. of this work.

ON THE REPRODUCTIVE POWERS OF FEMALES BORN CO-TWIN WITH MALES AMONG OUR DOMESTIC UNIPAROUS ANIMALS, AND IN THE HUMAN SUBJECT.

Mr. John Hunter, in an essay read before the Royal Society of London, in 1779, and afterwards published both in the Philosophical Transactions for that year, and in his work on the Animal Economy, showed that, when among black cattle the cow brings forth a male and female at the same birth, the male is a perfect bull calf, but the apparent female is almost always imperfect in its sexual organization. Female cattle of this kind, born co-twin with males, have long been distinguished in this country under the name of free-martins. In external appearance and form of body, they usually resemble the ox and spayed heifer more than either the entire male or entire female of the species. They commonly grow to a larger size than either the bull or the cow, and have horns like those of an ox, and a tone of bellowing similar to his, with the same marked disposition to become fat under the use of nourishing food. In general they do not show any sexual desire for the bull or the bull for them.

The defective sexual conformation of free-martin cattle is attested, not only by the observation of their sterility during life, but also by the anatomical examination of their reproductive organs. Mr. Hunter had an opportuity of dissecting several free-martin cows. In all of them the external sexual organs were of the female type—the vulva and os vaginæ being in general well developed. The vaginal canal, however, was contracted at its upper part, and the internal female organs, the uterus, Fallopian tubes, and ovaries, were altogether rudimentary and imperfect in their structure. In some there was an apparent superaddition of male organs, testes, and vasa deferentia, probably from a permanence of the Wolffian bodies and ducts of the early embryo; and in one case the ovaries were replaced by bodies having all the external characters of male testicles.

Mr. Hunter's observations have been since corroborated by additional cases and dissections made by Scarpa,[1] Gurlt,[2] and Allnatt.[3] I have myself had an opportunity of dissecting the sexual parts of two adults and a third young free-martin, killed in the shambles of this city, and have found all of them formed after the abnormal and imperfect sexual type pointed out by Mr. Hunter.[1] My friend Dr. Allen Thomson made some years ago a similar observation upon a free-martin twin fœtal calf.

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[1] Mem. della Societa Italiana, tom. ii. p. 846.

[2] Lehrbuch der Pathologischen Anatomie der Haus-Saugethiere, Bd. ii. s. 188. Tab. xxi. Fig. 2, 3, and 4.

[3] London Medical Gazette, vol. xviii. p. 528.

The butchers in Edinburgh and its neighborhood, of a number of whom I have made inquiries upon the subject, seem to be perfectly familiar with the fact, that in the free-martin, whose flesh they usually reckon of a superior quality, the womb, or calf-bed, as they term it, is in almost all cases apparently wanting; and all our intelligent agriculturists in the Lothians are acquainted with the sterile character of these animals.

Though we are certainly indebted to the sagacity of Mr. Hunter for first fully appreciating the value of the physiological sexual anomaly observed in free-martins, and for confirming the fact by accurate anatomical investigation, yet it is but proper to mention, that the circumstance itself of the infecundity of the free-martin cow, has long, as was indeed pointed out by Mr. Hunter himself, been notorious among agriculturists in Great Britain, and is prominently mentioned by Leslie, and some of the older authors on husbandry.

Indeed, the Roman agriculturists seem not to have been unacquainted with the variety of barren female cattle under consideration; or at least their attention appears to have been so often attracted by cases of sterility in the cow, that they found it a matter of convenience to employ, as we do, for their designation and distinction, a specific noun, and named them *Tauræ*. Thus Varro in his work "De Re Rusticâ," tells us "Quæ sterilis est vacca *taura* appellatur;"[2] and Columella, in speaking of the sorting (*delectus*) of the flock, directs that, "those which have brought forth, and the old cows which have ceased to breed are to be removed, and so also the *tauræ*, which occupy the place of fertile cattle, are to be set aside, or to be trained to the plough, since they are not by their sterility rendered less fit for labor than the common heifer."[3] There is no direct evidence, however, to show that the Romans were aware of the particular circumstances, in respect of plural births, under which such *tauræ* were produced.

Though the infecundity of free-martin cows be a very general fact, still it is by no means a universal one. Mr. Hunter, in his original essay on the subject, mentions that in one instance, in examining a free-martin that died when about a month old, he found all the organs of generation well formed. After stating this case, he adds, "I have heard of other twin cows breeding; but as I cannot call to mind the names of the individuals who communicated the circumstances to me, I have only mentioned one of undoubted authority."

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[1] I have described at length the particular anatomical appearances met with in these cases in my Essay on Hermaphroditism.

[2] Libri de Rustica Catonis, Varronis, Columellæ, &c., Paris Edit., Liber ii. p. 82.

[3] Enixæ et vetustæ quæ gignere desierint summovendæ sunt, et utique *tauræ*, quæ locum fœcundarum occupant, ablegandæ, vel aratro domandæ, quoniam laboris et operis non minus quam juvencæ, propter uteri sterilitatem patientes sunt. Ibid. Lib. vi. Cap. xxii p. 232.

An anonymous author in the Farmers' Magazine for November, 1806,[1] has described such an instance in a free-martin belonging to Mr. Buchan of Killingtringham. This cow was well made and a good milker; she produced one calf. The same gentleman, Mr. Buchan, had a second free-martin which never bred. Another writer in the same Magazine for November, 1807, makes the following statements: "On the 11th of November, 1804, a cow of mine brought forth two calves, one a bull, and the other a cow calf; and in spring last the female twin produced a very good male calf; yet a neighbor of mine assures me that a female twin belonging to him would never take the bull, and was sold on that account to the butcher at the age of four or five." Dr. Moulson of Halifax mentions, in Loudon's Magazine,[2] the case of a free-martin cow reared by Joseph Holroyd, Esq., of Withers, near Leeds, which copulated with its own twin bull. "In due time," he adds, "the heifer brought forth a bull calf, and she regularly had calves for six or seven years afterwards."

In the course of making some inquiries in West Lothian after cases of free-martin cows, I have become acquainted with two well-authenticated instances in which these animals proved capable of propagating. One of these cases occurred some years ago at Newton, near Queensferry. The second was reared by Mrs. Cochran of Stewartsfield, Broxburn, and produced several calves. Such exceptional cases, however, as those to which I have alluded, appear, on the whole, to be comparatively so rare in their occurrence, as not to invalidate the generality of the fact with regard to the sterility of the free-martin twin cow, and render it a question of interesting inquiry, whether this law of infecundity in the female of male and female co-twins be confined only to plural conceptions among black-cattle, or extends also to twins among other species of uniparous animals.

In reference to the females of opposite sexed twins among sheep, I have been assured from different quarters that the law of the sexual imperfection and infecundity of the free-martin cow does not hold good with respect to them. Several varieties of sheep, particularly some of those belonging to the white-faced breeds, produce twins with such constancy that we may truly consider this as one of their occasional hereditary characters. These twins are not unfrequently of different sexes, and yet instances of sterility are rarely if ever observed among such flocks.[1]

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[1] See Youatt on Cattle, p. 539.

[2] Magazine of Natural History, vol. v. p. 765.

I have not been able to learn how the matter stands with regard to twins among goats, not having access in this district of the country to any information or direct observation upon the subject in that animal.

I have hitherto been equally unsuccessful in tracing out any instance of a twin mare or she-ass, born under the circumstance already pointed out, being reared to maturity. The mare, indeed, appears only in extremely rare cases to produce twins, and these twins are almost always endowed with such feeble powers of life as seldom to survive for any length of time after birth.

Sir Everard Home, in a paper "On Animals preternaturally formed at the time of birth," inserted in the Philosophical Transactions for 1799, and in the third volume of his Comparative Anatomy, after assuming that certain sexual organs in the male and female are originally identical or neuter in their character, and are only afterwards changed to the male or female type according to ulterior circumstances, adds the following observations: "If it is allowed that the sex is impressed upon the ovum at the time of impregnation, it may in some measure account for the free-martins occurring when two young are to be impressed with different sexes at one impregnation, which must be a less simple operation, and, therefore, more liable to a partial failure than when two of any greater number of ova are impressed with the same sex."

"It may also account," he remarks in reference to the human subject, "for twins being most commonly of the same sex; and when they are of different sexes, it leads us to inquire whether the female, when grown up, has not, in some instances, less of the true female character than other women, and is not incapable of having children." "In warm countries," Sir Everard adds, "nurses and midwives have a prejudice that such twins seldom breed."[2]

In reference to this last remark, it is not unimportant to observe, that, as I have repeatedly found during the course of my inquiries upon this subject, a similar prejudice in reference to the infecundity of human females born co-twin with males, exists to a considerable extent among the peasantry of the Lothians, and has very probably been derived from the analogy of the free-martin cow. "The mischief," justly observes a late physiological author, "to which the opinion might give rise in causing a girl to be rejected as a wife for a defect, or taken for an excellence, according as sterility might be regarded, which she did not possess, is incalculable."[1]

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[1] The tendency to the production of twins in the human subject is sometimes so marked in particular families as to entitle it to be considered as almost an hereditary peculiarity. I know of one family, in the different branches of which twelve pair of twins have been born within three generations.

[2] Comparative Anatomy, vol. iii. pp. 333–4.

The truth or falsity of the opinion itself can only be satisfactorily settled by an appeal to a sufficient number of accurately ascertained histories of cases in which women, born co-twin with males, have reached an adult age and become married.

I have collected what may probably be considered as a sufficient number of such cases for forming some just conclusions upon this subject.

Before, however, bringing forward the results derivable from these collected cases, as bearing upon the question of the fecundity or sterility of human females born under the circumstances in question, I may, in the first place, mention that instances of twins in the human subject, of whom one child is male and the other female, seem not to be at all rare in their occurrence, though the contrary position is generally believed, and as we have seen, was assumed by Home, and made by him at one and the same time a deduction from, and an argument in favor of, the particular theory which he held in respect to sexual development. In proof of this statement, I have analyzed the records of all the labors that occurred in the Edinburgh General Lying-in Hospital from 1823 to 1836, both years included, as well as the published returns of all the cases occurring in the Dublin Lying-in Hospital from 1787 to 1793, as given by Dr. Clarke, and from 1826 to 1833, as given by Dr. Collins,[2] as also those occurring in the London Maternity Charity from 1828 to 1840,[3] which are the only returns that I am aware of in which the average number of twin-labors and the sexes of the different co-twins have been noted. The three following tables have been constructed from these reports. The first of them shows the average number of twin-labors as they occurred in these institutions respectively; the second presents the sexes of the different pairs of twins as they occurred in each; and the third illustrates the proportion in which the differently sexed co-twins were to the general number of labors.

I. *Total number and proportion of twin-labors.*

	Total number of Labors.	Number of Twin-Labors.	Proportion of Twin-Labors.
Edinburgh Hospital, ...	2,888	46	1 in 63
Dublin Hospital (Clarke), ...	10,337	184	1 in 56
Dublin Hospital (Collins), ...	16,414	240	1 in 65
London Maternity Charity, ...	29,489	318	1 in 93
	59,178	788	1 in 75

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[1] See footnote at page 74 of Dr. Fletcher's Rudiments of Physiology.

[2] Dr. Collins' Practical Treatise on Midwifery, table beginning p. 331.

[3] Dr. Ramsbotham's Principles and Practice of Obstetric Medicine and Surgery, p. 621, footnote.

II. *Sexes of the different co-twins in the above* 788 *cases.*

	Two males.	Two females.	Male and female.	Tot. num. of twins.
Edinburgh Hospital,	16	17	13	46
Dublin Hospital (Clarke),	47	66	71	184
Dublin Hospital (Collins),	73	67	100	240
London Maternity Charity,	93	111	114	318
	229	261	298	788

III. Proportion in which the differently sexed co-twins occurred to the whole 59,178 cases of labor:

Male and female twins once in every 199 labors.
Two female twins once in every 226 labors.
Two male twins once in every 258 labors.

The result of these two last tables goes to show, in opposition to the opinion of Sir Everard Home, that twins of opposite sexes are not by any means uncommon.[1] And this circumstance, of co-twins of opposite sexes occurring so proportionally frequent, may perhaps be adduced as bearing somewhat against the opinion of M. Girou, that the sex of the offspring is determined by the sex of that parent whose reproductive power or organism (*puissance prolifique*) is at the time of conception either absolutely or relatively in the greatest degree of vigor.

But to return from this digression. The only data hitherto published, so far, at least, as I am aware, containing an appeal to actual facts for the determination of the question of the fecundity or infecundity of the human female, when born co-twin with a male, are to be found in a short paper published by Mr. Cribb, in the Medical Repository for 1823,[2] and in the notice of a single case of this kind brought forward by the late Professor Meckel, in his essay on Hermaphrodites in Reil's Archives.[3]

In the paper referred to, Mr. Cribb has adduced the histories of seven married women, who were born co-twin with males, of which the following is the result: six had a family; one had no children, though married several years; or one in six was without issue.

The woman mentioned by Meckel was a mother.

I have endeavored to obtain as accurate and authentic reports as possible of the married history of various females born co-twin with males, and have so far been successful in gaining such information as I could implicitly rely upon in relation to 113 females born under such circumstances.[1] Of these 113 female co-twins, 103 had a family; 10 had none; or about one in ten was without issue. Of the ten cases in which there was no family, one had been married above five years; nine from ten to forty years.

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[1] As far as the data of these tables go, they would seem also to show, that, among twin births as a whole, more female than male children are produced. Out of the 1576 children born in the above 788 twin cases, 756 were male, and 820 female.

[2] London Med. Repos. vol. xx. pp. 213–16.

[3] Reil's Archives für die Physiologie, Bd. xi. p. 282.

The history of the male co-twin in the 103 cases in which the female was productive was as follows—in 53 he was the father of a family; in 24, he died in early life, unmarried; in 8, he remained unmarried; in 2, he was married, but had no issue; and in 14, his history could not be accurately ascertained.

In addition to the above cases of twins of opposite sexes, I have traced the married history of the female in four instances of triplets, in which there were born either two males and one female, or two females and one male.[2] In all of these four cases the female, whose history I ascertained, had a family. In a case of quadruplets, recorded in the Medical Repository for 1827, there were three males and one female. The males were all reared, and the female became herself the mother of triplets.

If we unite together all the various facts I have alluded to, we shall have the married history of 123 females born co-twin with males. The results, so far as they refer to the question we are discussing, may be stated in the following form; of 123 females born co-twin with males, 112 had a family and 11 had no issue, though married for several years. In other words, the marriages of the females, born under the circumstances we are considering, were *unproductive in the proportion of one in ten.*

It may at first sight be supposed that this result, though not fully bearing out, yet supports rather than otherwise the popular opinion and the statement of Sir E. Home, with regard to the infecundity of the female in twins of opposite sexes. Before, however, assenting to such a modification of the view, it is incumbent on us to inquire—

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[1] For assistance in the collection of these cases, I am much indebted to various professional friends, and particularly to Mr. F. Angus of Holytown, Mr. Girdwood of Falkirk, Dr. Gilchrist of Leith, and Dr. Cowan and Mr. Carmichael of Edinburgh. Tables, containing the name, address, and other particulars of each case, are in the hands of the Secretary of the Medico-Chirurgical Society.

[2] In one of these cases of triplets the three, two males and one female, all reached adult life. Dr. Merriman (Synopsis of the various kinds of Difficult Parturition, 4th edition) observes, p. 260, "So many years had elapsed, notwithstanding repeated inquiries, before I could meet with a well-authenticated instance of three children at a birth being all reared, that I began very much to doubt the fact." Besides the above, three other well-authenticated instances have been reported to me of triplets being viable and reared to maturity. Most certainly, however, one or more generally die shortly after birth.

WHAT IS THE AVERAGE PROPORTION OF PRODUCTIVE AND UNPRODUCTIVE MARRIAGES IN GENERAL SOCIETY.

On this point I have been able to gain but little precise information from the statistical or physiological works that I have had an opportunity of consulting. I have made personal inquiry on the same matter of several of our most eminent statisticians without being able to obtain any accurate facts bearing on the subject. In the Dictionnaire des Sciences Médicales,[1] it is stated that Hédin, a Swedish minister, had noticed that in his parish, composed of 800 souls, one barren woman is not met with for ten fertile. It is further stated, that Frank asserted, but from what data is not mentioned, that it would be found on investigation, that in most communities containing 300 to 400 couples, at least 6 or 7 would be sterile, without anything in their physical condition to explain the fact. It seems to have been from this assertion of Frank's, that Burdach, who is almost the only author who even alludes to the matter, has given the general statement that one marriage only in 50 is unproductive.[2]

For the purpose of ascertaining the point by numerical data, I had a census taken of two villages of considerable size, viz., Grangemouth in Stirlingshire, and Bathgate in West Lothian,[3]—the one consisting principally of a seafaring population, and the other of persons engaged in agriculture and manufacture.

The following form the results in these two places:

Of 210 marriages in Grangemouth, 182 had offspring; 27 had none; or about one marriage in ten was without issue.

Of the 27 unproductive marriages, all the subjects had lived in wedlock upwards of five years, and in all the female had been married that period before she reached the age of 45.

Again, of 402 marriages in Bathgate, 365 had offspring; 37 had none; or about one marriage in eleven was unproductive. There were at the same time living in the village 122 relicts of marriages, and of these 102 were mothers; 20 were not mothers; or about one in six had no family. In all, of 467 wives and widows, 410 had offspring; 57 had none; or about one marriage in 8 was unproductive. Of these last 57, six had not been five years married, and there were other six above the age of 45 when married. If we subtract these twelve, we have, of 455 marriages, 410 productive; 45 unproductive; or one in 10⅑ without issue.

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[1] Loc. cit. vol. vi. p. 245. See also Neue Abhandlungen der Schwedischen Akademie der Wissenschaften, vol. xi. p. 70.

[2] Dr. Allen Thomson's excellent essay on Generation, in Todd's Cyclopædia, vol. i. p. 478, footnote.

[3] My young friend, Mr. Thomas Girdwood of Falkirk, was so good as to make the census of Grangemouth for me. For the data relative to Bathgate I am indebted to the kindness of Mr. William Dick of that place.

Returns such as I have just now adduced are exceedingly difficult to obtain, in consequence of no registers being anywhere kept, so far as I know, that could be brought to bear upon the question. If it had been otherwise, I would here, if possible, have gladly appealed to a larger body of statistical facts, in order to arrive at a more certain and determinate average of the proportion of unproductive marriages in the general community. For the purpose, however, of extending this basis of data, I have analyzed with some care and trouble the history of 503 marriages detailed by Sharpe in his work on the British Peerage for 1833.

Among the British Peers there were 401 marriages with issue; 102 without issue; or of

503 existing marriages among British Peers in 1833,

74 were without issue after a period of five years. Of those who had not yet lived in the married state for five years, 28 were still without family; and in Burke's Peerage for 1842, there still remained among these 28 marriages,

7 without issue, making

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81 as the total number of unproductive marriages among the original 503; or the proportion of the unproductive to the productive marriages among this number is as nearly as possible, 1 in 6²⁄₇.

In the above calculation I have excluded eight unproductive marriages, in which the age of the husband at the date of marriage exceeded 56. These eight, however, ought to be deducted from the original sum of total marriages that were included, or, in other words, the 503 should be reduced to 495, and then the whole result would stand thus; among 495 marriages in the British Peerage, 81 were unproductive, or 1 in 6⅑ were without any family.

The total result of the three series of facts that I have brought forward with regard to the average number of unfruitful marriages may be tabulated, then, as follows:

	Total marriages.	Marriages without issue.	Proportion of unprod. marriages.
In Grangemouth, ...	202	20	1 in 10⅒
In Bathgate, ...	455	45	1 in 10⅑
In British Peerage, ...	495	81	1 in 6⅑
	1252	146	or 1 in 8⁴⁄₇

We thus see that if the marriages of females born co-twin with males are, as we have found them, unproductive in the proportion of 1 in 10, they do not, in this respect, exceed the degree of un-productiveness of marriages in other portions of the general community. Nor would I be inclined to forego this deduction, even if the average of unproductive marriages in society should be found, on a broader and more extensive basis of data, to be less than the above facts would seem to show. For certainly my own impression is, that probably I have had reported to me, and have entered among my notes and calculations, a greater proportional number of unproductive females among opposite sexed twins than may actually exist. In relation to such a question as the present, all minds are too liable to be impressed with, and recollect instances illustrative of the supposed rule and common opinion, whilst the apparent exceptions to it are unattended to or forgotten. In the earlier part of the inquiry I happened to take notes of several cases that had, in this way, been long stored up, by those that reported them to me, as evidence of the infecundity of the female co-twin, and who deemed them so far to afford sufficient proof of the popular opinion. The latter part of the investigation has, in a great measure, if not entirely, escaped this source of fallacy.

Before finally attempting to draw all the conclusions that are deducible from the facts we have collected, let us consider for a moment the question under another point of view, and inquire—

WHAT IS THE AVERAGE PRODUCTIVENESS OF MARRIAGES IN GENERAL, AND DOES THAT OF THE FEMALE IN OPPOSITE SEXED TWINS COME UP TO THE COMMON STANDARD?

Various authors, in treating of statistics and population, have calculated the average degree of fecundity of marriages in different climates and districts, as Malthus,[1] Macculloch,[2] Hawkins,[3] Saddler,[4] Sussmilch,[5] Quetelet,[6] and others.[7] Their observations show this average to vary from the number of 3 children to each marriage as a minimum, to 5¼ or $5\frac{8}{10}$ as a maximum. In an elaborate investigation into the subject made by Benoiston de Châteauneuf,[8] that eminent statistician states as average results upon a large series of observations, the following general conclusions:

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[1] Essay on the Principle of Population, 1803, p. 224.

[2] Statistics of the British Empire, vol. ii. p. 413.

[3] Elements of Medical Statistics, p. 221.

[4] The Law of Population, vol. ii. p. 380, &c.

[5] Gottliche Ordnung, Th. iii. s. 64.

[6] Sur l'Homme, ou Essai de Physique Sociale, 1836, tom. i.

[7] See a collection of evidence on this point in the Report from His Majesty's Commissioners for inquiring into the Poor Laws, 1834. Appendix F. of Foreign Communications, and table in Preface, p. xcix. &c.

[8] Annales des Sciences Naturelles, tom. ix. p. 431. "Notice sur l'Intensité de la Fecundité en Europe au commencement du dix-neuvieme siècle."

In Southern Europe, 457 births result from every 100 marriages, or about $4\frac{1}{2}$ from each; in Northern Europe, 430 births result from every 100 marriages, or about $4\frac{3}{10}$ from each.

Mr. Farre[1] calculates the mean fruitfulness of marriages in England in ordinary periods to be in every

Marriages.	No. of children.	Prop. of children to each mar.
100	420	$4\frac{2}{10}$

In relation to Scotland, Benoiston de Châteauneuf states,[2] that he had the patience to go over the tables of population in seventeen out of the twenty-one volumes of Sir John Sinclair's Statistical Account of this kingdom, and found in every

Marriages.	No. of children.	Prop. of children to each mar.
100	430	$4\frac{3}{10}$

What is the usual degree of fruitfulness in married life of females born co-twin with males? To make as near an approximation as possible to the solution of this question, I have added together the total number of children produced by 94 of those female co-twins, whose history I have collected. We omit the others remaining on the list, merely because we have no notes taken of the exact extent of their families. The total number of children produced by the 94 mothers alluded to, amounts to 409, or the result, when stated in relation to the standard of every 100 marriages of them, would be as follows:—In

Marriages.	No. of children.	Prop. of children to each mar.
100	424	$4\frac{2}{10}$

But in 15 of the 94 cases, the mother had as yet only one child at the time her case was noted, having either only lived in wedlock for a year or two previously, or the single child being the result of illegitimate intercourse. If we deduct these 15 cases only, though other mothers with a young and increasing family of two or three children might be excepted from the calculation in the same way, and on the same grounds, we shall then have 79 mothers producing 364 children; or, again, to state it in reference to the supposed standard of 100 marriages, we have this result as the degree of fruitfulness of such co-twin females. In every

Marriages.	No. of children.	Prop. of children to each mar.
100	460	$4\frac{6}{10}$

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[1] See Macculloch's British Empire, loc. cit.

[2] Annales des Sciences Naturelles, tom. ix. p. 447.

The whole inquiry detailed in the few preceding pages forms an apt illustration of an old remark, that in medicine it often requires a much greater extent of observation and research to disprove satisfactorily an alleged and accredited fact, than was ever expended, either upon the original development or subsequent confirmation of it. In the present instance, the results have turned out to be perfectly contradictory of the opinion which I, in common with others, held regarding the infecundity of the female in double sexed twins, when I commenced looking into the subject; and instead of finding my preconceived ideas confirmed by the investigation, they have on the other hand, been completely confuted by it. For the data that I have adduced do, so far as they go, evidently prove—

1. That, in the human subject, females born co-twin with males are, when married, as likely to have children as any other females belonging to the general community.

2. That, when they are married and become mothers, they are, in respect to the number of their children, as productive as other females.

3. That the same law of the fecundity of the female in opposite sexed twins seems to hold good among all our uniparous domestic animals, with the exception of the *cow* alone.

Indeed, the strong confirmatory evidence which the preceding inquiry affords of this last exceptional point constitutes one of its most interesting results. For certainly it cannot but be considered as an extraordinary circumstance, that, in the cow, the twin existence in utero of a male along with a female should, as a general principle, lead—

1. To so great a degree of malformation as we have described, in the sexual organs, and in the sexual organs only.

2. That this malformation should be limited entirely to the reproductive organs of the female twin, while those of the male twin are perfectly and fully developed.

3. That this sexual malformation should, apparently so far as we yet know, occur in the case of twins in the cow only, and in this species of uniparous animal alone. The curiosity of the fact becomes heightened and increased when we recollect that when the cow has both twins of the same sex, as two males or two females, these animals are always both perfectly formed in their sexual organization, and both capable of propagating. The whole series of circumstances, when considered in conjunction with each other, seems to form, in relation to the origin of malformations, one of the strangest and most inexplicable facts to be met with in the study of anormal development.

PART II.

PHYSIOLOGY AND PATHOLOGY OF PREGNANCY.

DURATION OF HUMAN PREGNANCY;

ITS FREQUENT IRREGULARITY AND OCCASIONAL PROTRACTION.

(From Edinburgh Monthly Journal of Medical Science, July, 1853, p. 50.)

"I HAVE never," declared Sir Charles Clarke, "yet seen a single instance in which the laws of nature have been changed, believing the law of nature to be, that parturition should take place *forty weeks* after conception."

The preceding statement was made by Sir Charles Clarke when giving his evidence on the famous Gardner Peerage Case, before the House of Lords, in 1816.

On the same occasion, four other London medical practitioners, viz., Dr. Gooch, Professor Davis, Dr. Blegborough, and Mr. Pennington, averred it was their belief that women never passed the normal period of pregnancy; or, at least, never passed it beyond two or three days at the utmost. These various witnesses, however, against the protraction of pregnancy beyond its normal period, were by no means agreed among themselves as to what the normal period of pregnancy actually was. For, while Sir Charles Clarke stated it as forty weeks, that is, 280 days, or ten lunar months, or nine calendar months and a week—on the other hand, Dr. Gooch observed, in relation to the period of pregnancy, that "its normal duration is nine calendar months, or rather less than nine calendar months," or from 273 to 275 days. "It is," he observed, "generally stated in the books to be forty weeks, but I believe forty weeks to exceed the usual term of pregnancy. The writers," Dr. Gooch added "say nine calendar months or forty weeks; now the fact is, that nine calendar months is scarcely more than thirty-nine weeks." Dr. Blegborough declared it as his opinion, that "the period of gestation is thirty-nine weeks or 278 days; but," he adds, "forty weeks I consider as the ultimatum."[1]

While these gentlemen thus varied as to what they considered the normal period of pregnancy—some stating that period to be thirty-nine weeks, others forty weeks—all of them agreed, to quote again Sir Charles Clarke, that "forty weeks is the *ultimum tempus pariendi mulieribus constitutum.*" Some physicians of the present day still hold that the law of nature is quite fixed in this respect, and that human pregnancy never exceeds the term in question. We have now, I believe, however, sufficient physiological and obstetric evidence accumulated to prove—1. That this doctrine regarding the ultimate length of gestation is incorrect; 2. That the term of human pregnancy is not, as a general law, so very definite and precise as many practitioners think it to be; and 3. That deviations from it, both in the way of diminution and of excess of time, are perhaps far more common than is generally supposed among the members of the profession at large. On the present occasion, I leave out of view the subject of abridged or shortened gestation; and I will chiefly confine my remarks to the much more vexed and debatable question of its occasional prolongation or protraction.

Most practitioners, from time to time, meet with cases in which there is apparently every reason to believe, that the term of pregnancy has exceeded the usual normal duration of 274 to 280 days. During the course of the last year or two, after setting aside some less remarkable instances, I have taken notes of two or three cases in my practice, in which, after making the common calculation of 280 days, or nine months and a week from the cessation of the catamenial discharge, pregnancy exceeded this period by several weeks, keeping the patients and their friends in a state of anxious waiting; and in all of the instances inflicting the unnecessary, as it proved, presence of a nurse in the house for a considerable time before labor actually occurred. The obstetric history of the first of these cases is interesting in more than this one respect.

CASE I.—This patient was married in September, 1845, and was supposed by her medical attendant to be pregnant in the earlier months of 1846. The case, however, proved one of that common affection, "spurious pregnancy;" and the catamenia, after being eight months obstructed, returned regularly. I first saw her in the course of the following year, in consequence of her suffering under leucorrhœa, and symptoms of chronic inflammation of the cervix uteri. The cervix uteri was enlarged and ulcerated, and the fundus greatly anteverted. The uterine inflammatory symptoms yielded under appropriate treatment; but the anteversion remained. In July, 1848, an intra-uterine pessary was introduced with the view of rectifying this displacement. The instrument remained in the uterine cavity till the end of October following. Immediately after its removal, pregnancy occurred, and she was delivered of a daughter in July, 1849, the labor being rendered very severe and tedious, by extreme rigidity of the tissues of the cervix uteri. From this cause the patient was, during parturition, fourteen hours continuously under chloroform. It was in her next or second pregnancy, that there was the apparent prolongation of utero-gestation. In the first week of January, 1851, the menses were present, and disappeared on the 4th or 5th of the month. On the 20th of the same month, January, there occurred a return of menstrual discharge for six days, in consequence of great mental distress at the death of a favorite brother. No menstrual discharge appeared from this date, and she was confined on the 28th of December, that is, 336 days after the last appearance of the catamenia. From the patient wishing to go abroad, I was asked to ascertain if she were in the family way, towards the end of April. In consequence of the size and shape of the uterus, &c., I had no hesitation in concluding at the time, that she was then at least two months advanced in pregnancy; and I calculated for her that she would be confined about the middle of November. Parturition, however, as I have already stated, did not occur till the 28th of December.

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[1] See Lyall's Medical Evidence in the Gardner Peerage Case, pp. 4–9, &c.

Case II.—Mrs. ———, the mother of two children, and always quite regular in her catamenial periods, except when pregnant and nursing, began to menstruate about the 20th of September, 1851, and the discharge ceased on the 24th. Shortly after this date, she had feverish symptoms, and from the catamenia not returning, she considered herself pregnant. She was not delivered, however, till August 3d, that is, 332 days from the last day on which the catamenial discharge appeared.

This lady's case was interesting in another point of view. In the middle of December, while in London, in consequence of a long walk, she was threatened with symptoms of miscarriage, requiring rest and treatment. On the 3d, or rather morning of the 4th of January, a large steamer in which she was, caught fire when two or three days out at sea, and only a small number of the passengers and crew escaped. After making almost superhuman exertions to save herself and a young son seventeen months old, whom she held in her arms, and after having her body severely bruised and contused, she was exposed for seventeen hours in an open boat, with little or no clothing, and sitting immersed several inches deep in water, during the whole of that long and anxious period. Yet, all the fearful mental excitement and bodily exertion to which she was thus subjected were accompanied by no tendency to miscarriage, though, as I have already said, two or three weeks previously a long walk had nearly brought on abortion. Could the protracted mental agitation and trial have in any way led to the unusual prolongation of her pregnancy?

CASE III.—A patient had the usual menstrual discharge in March, 1852, and it left her on the 23d day of that month. She was not delivered till the 5th of February 1853, or till 319 days after the last appearance of the catamenia. That the patient became pregnant very shortly, if not immediately, after her last menstruation, I have every ground for believing; for, in May, I was asked to see her before she left Edinburgh for the country, in order to determine whether the obstruction which had occurred was the result of pregnancy or not, as various arrangements depended upon this. On examination, I found the uterus so enlarged, as to leave no doubt whatever that she was then about two months pregnant. The patient was, like myself, distrustful of the obstructed menstruation being a sufficient test, from one point in her previous history. She had been married several years, and without any family, when she first came under my care. The uterus was retroflexed, and for this she wore for some time an intra-uterine pessary. Within the first half-year after its removal, menstruation was absent for three months, and she had various symptoms of pregnancy; among others, the areolæ became dark, and their glandulæ enlarged. The uterus, however, did not increase in size—showing the symptoms to be those merely of spurious pregnancy. The areolæ were altogether so dark and marked, that a drawing of them was made about the third month. These sketches presented all the usual changes as distinctly as those figured by my friend Dr. Montgomery in his plate of the true areola at that period—and being preserved, they were found on comparison as marked as the patient's own breasts were at the same date a short time subsequently, when actual pregnancy had supervened. The prolonged pregnancy mentioned above occurred with this lady's fourth child.

When lately conversing with my friend Dr. Young, upon the possible prolongation of pregnancy, he stated to me a case which some time ago had occurred in his own practice; and of which the following is a note of the date and circumstances:

CASE IV.—The patient was always regular in her menstrual life and had the catamenia present from the 9th to the 14th of July. This was the last appearance of the menses. She felt sickness towards the end of the same month, which continued more or less for four months, or till the end of November. Fœtal movements were distinctly felt on the 17th of November. She was delivered on the 3d of June, or in ten months and eighteen days; being 324 days after the last appearance of the catamenial discharge, and six months and sixteen days, or 198 days after the first symptoms of quickening.

In the preceding cases, the duration of pregnancy as calculated from the date of the last appearance of the last menstruation, up to the date of delivery, were respectively 336, 332, 319, and 324 days. But, of course, it does not necessarily follow that the pregnancy in these instances actually extended to the preceding durations. For, it may be objected, that none of the patients became pregnant till immediately before the return of the next menstrual period; or, on an average, till twenty-three days later than the date of the day of the last catamenial discharge. Even, however, if, for this reason, we thus subtract twenty-three days from each of these cases, the duration of gestation would still considerably exceed the normal limits; for twenty-three days thus subtracted from the first case would still leave a pregnancy of the duration of 313 days; from the second case, of 309 days; from the third, of 296 days; and from the fourth, of 301 days. Or the cases would stand thus:

Case I.	No. of Days from last Menstruation, ...	336 — 23 = 313
II.	" " "	332 — 23 = 309
III.	" " "	319 — 23 = 296
IV.	" " "	324 — 23 = 301

There is another and greater source of fallacy to which such observations are liable, viz., that the catamenia are sometimes arrested from other causes than impregnation, for a month or two before conception actually occurs; and that hence, if we date the duration of gestation from the last appearance of the catamenia, we may fall into a serious error of calculation. I have specially recorded Cases I. and III., because in these instances where gestation happened to be so much prolonged, this source of error was fortuitously avoided; in both, the actual existence of pregnancy having been ascertained within eight or nine weeks after conception, by the most certain of all tests at that time, viz., the regularly marked enlargement and altered shape of the uterus.[1]

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[1] It has sometimes seemed to me also not improbable, that some cases of apparently prolonged gestation can be accounted for by another explanation. All our latest and best anato-

Three authors—Dr. Merriman, Dr. Murphy, and Dr. Reid—have each published a long series of observations from their own practice, to show the date of delivery as calculated from the last day of the catamenial discharge. Dr. Merriman gives as data for making this calculation, the dates of the birth of 114 mature children, calculated from the day on which the catamenia were last distinguishable, but not including that day.[1] Dr. Murphy's cases analogous amount to 168;[2] and Dr. Reid's to 500.[3] In the following table I have attempted to bring into a comparative view the results obtained by these three physicians in the 782 cases upon which their observations are founded. The table shows in divisions of weekly periods the data of delivery of these 782 patients, as calculated from the last day of the appearance of the last catamenial discharge in each individual:

TABLE I.

DATES OF DELIVERY, CALCULATED FROM LAST DAY OF CATAMENIA.

Weeks.	Days.	Merriman.	Murphy.	Reid.
37th	From 252d to 259th	3	12	23
38th	" 260th to 265th	13	14	48
39th	" 267th to 273d	14	27	81
40th	" 274th to 280th	33	28	131
41st	" 281st to 287th	22	39	112
42d	" 288th to 294th	15	21	63
43d	" 295th to 301st	10	25	28
44th and upwards.	" 302d to 326th	4	2	14
		114	168	500

The result of the preceding observations of Dr. Merriman, Dr. Murphy, and Dr. Reid, may perhaps, however, be shown still more accurately by reducing the facts mentioned by them into another form, and calculating the weekly percentage of deliveries from the last day of the catamenial discharge, as brought out in the observa-

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mists seem now to agree that the decidua of impregnation is merely an altered and hypertrophied state of the mucous membrane of the cavity of the uterus; and that, consequently, the decidua is not at first a shut sac, but has the orifices of the Fallopian tubes and os uteri for a time opening into it. And it seems not entirely impossible that the ovulum of one impregnation dying without the decidua being immediately cast off, the cavity of the decidua —or, in other words, of the uterus—remaining permeable, a second impregnation may possibly take place within a short time, and the decidua of the first impregnation serve as the decidua of the second conception. If ever such an occurrence takes place, a catamenial period might be passed without any catamenial discharge—and hence an unavoidable error in calculation be fallen into.

[1] Medico-Chir. Trans. vol. xiii. p. 340.

[2] Report of the Obstetric Practice of University College Hospital, p. 9.

[3] Lancet, for July 20, 1850, p. 81.

tions of each of these authors. The following table is intended to give this view of the subject:

TABLE II.

PERCENTAGE OF WEEKLY DELIVERIES FROM LAST DAY OF CATAMENIA.

Weeks.	Days.	Merriman.	Murphy.	Reid.
37th	From 252d to 259th	2·65	7·14	4·60
38th	" 260th to 266th	11·40	8·33	9·60
39th	" 267th to 273d	12·28	16·07	16·20
40th	" 274th to 280th	29·00	16·66	26·20
41st	" 281st to 287th	19·30	23·21	22·40
42d	" 288th to 294th	13·16	12·50	12·60
43d	" 295th to 301st	8·77	14·86	5·60
44th and upwards.	" 302d to 324th	3·50	1·20	2·80
		100·	100·	100·

The preceding facts show, that if we take as the starting-point or commencement of our calculations regarding the duration of pregnancy, the last day of the last catamenia, the date of delivery is far less decided and determinate than is generally believed by the medical profession; the cases registered in these tables showing very great irregularities and deviations in this respect. It is further deserving of especial remark, that a large proportion of all the cases, from 12 to 16 per cent, terminated as early as during the course of the 39th week, or from 267 to 273 days after the last day of the last menstruation. On any one individual day, only a small proportion of deliveries occurred. The largest actual number of mothers delivered on individual days was in Dr. Murphy's series of 186 cases, 10, or 6 in every 100, on the 285th day after menstruation; 7 on the 273d, 279th, and 283d days; and 6 on the 270th, 278th, 284th, 287th, and 297th days after the same date. In Dr. Reid's series of 500 cases, the greatest number of mothers delivered on individual days was 25, or 5 per cent on the 282d day after the last catamenia; 22 on the 278th day; and 21 on the 274th and 279th, &c. Out of Dr. Merriman's 114 cases, 9 were delivered on the 280th day; 8 on the 277th; 6 on the 283d; 5 on the 281st and 288th day; 4 on the 264th, 274th, 285th, 298th, &c.

One circumstance accounting, no doubt, for much of the apparent irregularity or instability in the duration of pregnancy, as shown in the above tables, in the cases prolonged to the 40th week, or beyond it, is the fact, that though impregnation usually takes place within a very few days after the last catamenial discharge, it may not necessarily do so. For conception may in fact occur at any date previous to the recurrence of the next menstrual period. This gives an uncertain limit for the actual date of impregnation, of twenty-two or twenty-three days—the average interval between two menstrual periods. But the possibility of variation from this circumstance will by no means account for the great amount of variation in gestation which is so frequently observed. For there has now been recorded a long comparative series of observations upon the duration of pregnancy among our domestic animals, and particularly upon the cow, in which these sources of variation and fallacy with regard to the length of gestation, were avoided by the fact, that the exact day and date of impregnation in these experiments, from a single intercourse with the male, were in each instance accurately noted and fixed. Thus Lord Spencer has reported the date of delivery in 754 cows, in which the day of impregnation from a single coitus with the bull had been carefully registered.[1] M. Tessier had previously published 572 observations of a similar kind upon the same animal.[2] If we omit a number of cases in each of these two series of observations, in which delivery supervened earlier than the thirty-seventh week, we have the results of Lord Spencer's and M. Tessier's observations presented in the following table.

TABLE III.

PERIODS OF GESTATION IN COWS.

Weeks.	Days.	Spencer.	Tessier.
37th	From 252d to 259th	12	6
38th	" 260th to 266th	4	8
39th	" 267th to 273d	24	51
40th	" 274th to 280th	121	166
41st	" 281st to 287th	392	202
42d	" 288th to 294th	175	105
43d	" 295th to 301st	16	27
44th and upwards	" 302d to 321st	7	7
		751	572

This table shows that in the cow, even when the day of impregnation was fixed and ascertained, the period of delivery was still very far indeed from being quite stable and determinate.

But to make the comparisons between these observations on the cow, and those reported above on the human subject, more complete, let us reduce the experiments on the gestation of the cow to a percentage form, as we have already done in Table II., in relation to the cases in the human subject, recorded by Drs. Merriman, Murphy, and Reid. The following table shows the actual proportion of weekly deliveries in Lord Spencer's and M. Tessier's observations on the cow, as calculated from the known date of impregnation of the animal; or, in other words, the proportion of cases in every 100 in which the gestation was normal, abridged, or prolonged:

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[1] Journal of the English Agricultural Society, Part ii. 1839.

[2] Memoires de l'Academie Royale des Sciences, 1819, Tom. ii. p. i.

TABLE IV.

PERCENTAGE OF DIFFERENT PERIODS OF GESTATION IN COWS.

Weeks.	Days.	Spencer.	Tessier.
37th	From 252d to 259th	1·60	1·05
38th	" 260th to 266th	0·55	1·40
39th	" 267th to 273d	2·80	8·91
40th	" 274th to 280th	16·53	29·02
41st	" 281st to 287th	52·27	35·31
42d	" 288th to 294th	23·18	18·36
43d	" 293d to 301st	2·12	4·72
44th and upwards	" 302d to 321st	0·93	1·22

In making any comparison between the results of the period of gestation in the cow, as shown in Table IV., and the period of gestation in the human female, as shown in Table II., it is necessary to remember, that, while the normal period of gestation in the human female is probably from 274 to 280 days, that of the cow extends, according to Lord Spencer, to 284 or 285 days. And hence, while we find that the largest percentage of deliveries in the cow takes place from the 281st to the 287th day of gestation, the largest percentage of deliveries in the human female, as calculated even from the last day of the catamenial discharge, occurred in Dr. Merriman's and in Dr. Reid's observations, from the 274th to the 280th day; though in Dr. Murphy's series of cases, his largest weekly percentage occurred in the week following the 280th day from the last catamenia—an occurrence probably to be explained by the circumstance of impregnation not following till two, three, or more days after the last menstruation.

Amongst the various observations which might be elicited by a comparison of these tables, let me state one more: That though the period of gestation in the cow is found liable to considerable variations, and is by no means so precise and determined as many authorities have supposed it to be, yet the period of pregnancy in the human mother is not even so regular as the period of gestation in the cow. This greater apparent variation of the duration of pregnancy in the human subject in these tables, doubtless depends upon the fact already adverted to, that the last day of the catamenia does not by any means, in the human mother, fix determinately the actual day of conception.

Occasionally, however, in the human mother, pregnancy occurs under circumstances which fix the date of conception to a precise day. Dr. Reid, for example, has collected from the observations of various authors, and from his own practice, the history of forty cases in the human female, where impregnation was the result of a single intercourse, the date of which was accurately known; the cases being all of them "instances of single women who dated from one coitus, or of married females whose husbands had been absent for a considerable time before the last intercourse," and all of them being selected with a view to avoid as far as possible every source of deception. Below I have thrown these forty cases of gestation into a tabular form. This table demonstrates the varying duration of pregnancy, and consequently the variable date of parturition and delivery, in the human female, even when the date of impregnation is previously accurately established and known. The last column in the table presents in the percentage form, so far as can be calculated from so small a number, the relative proportion delivered at different dates from the day on which conception occurred.

TABLE V.

PERIODS OF FORTY DELIVERIES IN HUMAN MOTHERS—IMPREGNATION BEING CALCULATED FROM A SINGLE INTERCOURSE.

Weeks.	Days.	Total Number of Cases.	Percentage.
38th	From 260th to 266th	5	12·50
39th	" 267th to 273d	7	17·50
40th	" 274th to 280th	18	45·00
41st	" 281st to 287th	6	15·00
42d	" 288th to 294th	4	10·00

This table shows that in the human subject, and with the day of impregnation known, pregnancy in nearly one-half, or in 45 per cent, extended from 274 to 280 days; that in 15 per cent it was protracted from the 281st to the 287th day; and in 10 per cent[1] it was prolonged from the 288th to the 294th day. Three only out of these 40 cases, or 1 in 13, were delivered on the 280th day after conception. Seven, or 1 in 6, were delivered on the 274th day.

Admitting then the possibility of the occasional protraction of human gestation beyond its common normal period, another question has sometimes been asked, viz., What is the extreme limit of this protraction, or, in other words, to how many days beyond the normal period of gestation is it *possible* for a woman to carry a child? I confess that some of the late cases recorded, particularly by our American brethren, appear to me to be beyond the bounds of this possibility. Thus, Dr. Meigs of Philadelphia has reported a case on "facts" which he conceives to be "trustworthy," where a patient supposed herself pregnant in July, 1839, quickened on the 20th of November, had spurious labor pains on the 10th of April, but her child was not born till "the 13th of September, 1840, about daylight;" the pregnancy thus, according to Dr. Meigs, having "endured nearly fourteen months or 420 days."[1]

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[1] Perhaps this proportion is made larger than it should be, by some of the published cases which Dr. Reid has collated having been possibly recorded merely because they were marked instances of protraction.

Professor Atlee of Philadelphia has published two cases of protracted gestation occurring under his own observation. In the first of these, the patient lost her catamenia on March 22d, 1832—quickened August 5th of the same year—and was delivered with forceps March 22d, 1833. It was her fourth pregnancy. In Dr. Atlee's second case, the patient saw appearance of menstruation for the last time on August 6th, 1832—quickened on December 25th—but was not delivered till August 13th, 1833. Dr. Atlee, who states that he has not "the least doubt of the truthfulness of the evidence in the above cases," imagines, from the date of the quickening in each, that both mothers became pregnant shortly after their last catamenial periods, and that consequently, in both instances, the patient went pregnant nearly the entire circle of the calendar, or 365 days.[2]

Without stopping to inquire as to the various sources of fallacy in these and similar observations, let me merely add, that I believe our best criterion for fixing the "legal limit," or ultimate possible period of gestation in the human female, will be derived from careful and repeated observations upon the ultimate period of gestation in the cow; allowing always for the difference of four or five days of excess in the normal period of pregnancy in the cow, as compared with the human mother. Lord Spencer, in one instance, found the period of gestation in the cow to extend to 313 days from the date of impregnation; and in one of his observations on the same animal, M. Tessier states, that parturition did not supervene till 321 days from the date of conception.

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[1] Obstetrics, the Science and the Art, 1852, p. 230.

[2] Tucker's Elements of Midwifery, p. 149.—Dr. Tucker records at some length the opinion of the American Court regarding the possible protraction of pregnancy, as elicited at a trial for bastardy at the Lancaster Quarter Session. In this case the prosecutrix swore that her child was begotten on the 23d of March, 1845, and was born on the 30th of January, 1846—making the period of gestation 313 days—or two days longer than the alleged term of gestation, 311 days, in the Gardner Peerage case. In the American trial, six medical practitioners testified with more or less positiveness, against the possibility of the protraction of pregnancy. On the other side, five medical practitioners declared their belief in the occasional extension of gestation beyond the normal period, and in the possibility of its protraction to 313 days. In charging the Jury, the President of the Court held, that protracted gestation for a period of 313 days, "although unusual and improbable, was not impossible;" and in accordance with this charge the Jury found the defendant guilty.

Such direct experiments and observations upon the lower animals afford evidence, which necessarily, I think, forces us to admit, that in exceptional cases in the cow—and hence also, as we certainly may fairly infer, in the human female—the period of gestation may be prolonged 30 or 35 days beyond its normal and usual duration. And it is not impossible that further accurate and repeated experiments, of the nature of those performed by Lord Spencer and M. Tessier, may yet establish, by the same kind of proof, even a more extended limit as the *ultimum tempus pariendi.*

Those obstetricians who maintain that the period of human gestation is a fixed period, and can never by any possibility exceed the fortieth week (as was sworn to by the five London accoucheurs who gave evidence in the Gardner peerage cause, and by the six medical practitioners who more lately gave similar evidence in an analogous cause before the American Court at Lancaster,[1]) have none of them adduced any reason why the period of pregnancy should be thus stable and invariable, while all other periodic processes in the human body—as dentition, puberty, menstruation, the date of quickening, &c., are universally known and acknowledged to be apt to vary extremely. These obstetricians have offered no reasons, so far as I know, for holding that similar variations could not take place in the duration of pregnancy. Indeed, it would be against all analogies from other actions and processes in the animal kingdom to suppose that such variations did not occur in regard to the function of gestation.

Cases of protraction of utero-gestation beyond the usual average period have been attempted to be accounted for by some physiologists, by tardiness in the escape of the ovulum from the ovary after impregnation, by delay in its transit through the Fallopian tube, and by other deviations in time connected with the earlier history of development. These explanations, however, are at the best mere assumptions and hypotheses. There are, however, one or two known circumstances which seem to have an occasional, though not very marked, influence on the prolongation of pregnancy.

In the individual cases of protraction which I have detailed towards the commencement of the present communication, the protraction was limited to individual pregnancies in the mothers. With other children, these mothers had not exceeded the normal term. Occasionally, however, a disposition to prolongation appears to be peculiar to some mothers in *all* their pregnancies. Dr. Dewees states, that he had occasion to observe the anormal prolongation of pregnancy to upwards of ten calendar months, or above 300 days, as an habitual arrangement in at least four females whom he had attended.[1] Dr. Hamilton has recorded the protraction of pregnancy as a marked individual peculiarity in one of his patients. In this case the lady lost her first two children from what was supposed to be prolonged gestation to the eleventh menstrual period. In her three next pregnancies Dr. Hamilton induced labor with success a week after the tenth menstrual period; in all of these pregnancies, the patient seeming to show the same tendency to the prolongation of gestation.[2] My friend, Professor Retzius of Stockholm, has informed me of a still more strange example that has occurred in his practice of extreme protraction of pregnancy, not as a peculiarity in an individual mother, nor as attending individual pregnancies only, but as an hereditary peculiarity in a mother and her two daughters.

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[1] See preceding footnote from Dr. Tucker's work.

It is a common idea among agriculturists in this country, that pregnancy is more apt to be protracted in cows, &c., in proportion to the frequency of their previous pregnancies. I am not aware of any collected facts calculated to prove or disprove this idea. The sex of the calf is also very usually supposed by farmers to have some influence upon the duration of pregnancy—cows when they pass beyond the normal period being averred to produce more frequently a male than a female calf. In Lord Spencer's experiments—if we omit the cases of twin births—among all the cows delivered up to the 284th day after impregnation, there were 53 per cent. of female, and 47 per cent. of male calves born; while among 331 cows whose period of gestation lasted from 285 to 313 days, as many as 61 per cent. of the calves produced were males, and only 39 per cent. females. In the table which Dr. Murphy has published of the duration of pregnancy in 184 human females, the sex of the child is stated in each with a few exceptions. I find that out of 90 cases noted in which the mothers were not delivered till upwards of 280 days after menstruation, 57 per cent. of the children born were boys, and 43 per cent. were girls.

Perhaps the state of health and activity of the uterus as an individual organ of the body, may sometimes have the effect of leading on to prolongation of gestation. In two of the cases, viz., Cases I. and III., which I have detailed, it is not impossible that this condition may have been the cause of the protraction.

The cause why parturition generally comes on in the human female from the 274th to the 280th day has been much debated among physiologists and accoucheurs; and perhaps a knowledge of it may be necessary before we can understand all the circumstances which lead to the frequent irregularities and occasional prolongation of pregnancy. All the explanations which, so far as I know, have been hitherto offered on this subject, such as those that refer the excitation or supervention of labor to the development of the body of the uterus, to the state of the cervix, to the state of the ovary, to the state of the fœtus, &c., will, I believe, be found untenable. And I would venture to suggest that, in the human female, the exciting cause of parturition is to be traced to changes going on, or rather accomplished, between the uterus and its deciduous lining,—which changes lead to parturition, when they have proceeded so far as to effect the necessary amount of disintegration and separation between the relatively attached surfaces of the uterus and decidua. In these communications, I may take another opportunity of stating the different facts which seem to me to bear on this view of the question, and show more particularly the changes in the structure of the decidua—both *vera* and *serotina*, but especially the former—and in particular of its outer or attached surface, to which I allude. Let me in the meantime merely observe, that these changes, in the connection between the decidua and uterus, seem of a nature analogous to the so-called fatty degeneration, which occurs in effete and worn-out structures in other parts of the body; and that we artificially imitate these changes and their effects in inducing premature labor, when we separate the membranes from the interior of the uterus with the finger or the sound, or when we inject water into the uterine cavity, &c. These changes of textural degeneration and detachment are not necessarily accomplished with precision to a certain fixed day; and hence, I believe, the very great irregularity which we have seen in the preceding tables to exist in relation to the time at which parturition supervenes after impregnation. In Lord Spencer's and M. Tessier's tables, we have evidence of the extreme irregularity of the period of utero-gestation, and the comparative frequency with which anormal protraction of gestation occurred in a series of experiments, which, it must be remembered, were limited to about 1300 cases only;—and when we call to mind, at the same time, that in Great Britain alone, some 600,000 cases of human gestation and parturition take place annually, we cannot avoid concluding, that irregularities and anormal protractions in human gestation might be found in obstetric practice far more frequently than the profession generally suppose, provided the circumstances bearing upon this point were always duly and properly investigated.

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[1] Compendium of Midwifery, p. 465.

[2] Practical Observations, p. 106.

THE APPEARANCE OF THE AREOLA AS A SIGN OF PREGNANCY.[1]

(From Edinburgh Monthly Journal of Medical Science, March, 1848, p. 693.)

Dr. Simpson showed to the Society a woman seven months gone with child, whose breasts gave no indication whatever of her pregnant state. The case was a peculiar one. There was no doubt of her being pregnant, as the fœtal heart could be distinctly heard; but, at an earlier stage, great doubt had been thrown on the nature of the case, from the presence of several large fibrous tumors in the walls of the uterus. These had even given rise to the suspicion, on the part of the physician she had consulted, that the fœtus was extra-uterine. She had been four years married, and was now pregnant for the first time. On examining both breasts, there is no appearance whatever of a single enlarged gland in the areola; besides, the areola is not at all tumid, and is scarcely darker than the surrounding skin. Dr. Simpson had caused a drawing to be made of this breast, which he contrasted with another of the areola of a lady who had never been pregnant, but was suffering from great uterine irritation. In this last instance the areola was turgid, and of a dark brown color, the papillæ were numerous and much enlarged, and the superficial veins very large and prominent. He had observed the same appearance as in the last case in other non-pregnant women.[2] In one, the woman could never be pregnant, as the uterus was malformed, and not more than one inch and a half in length. In another case, the marks were so distinct, that the late Dr. Hamilton, trusting mainly to them as undoubted indications of pregnancy, had, two or three years before, pronounced the lady pregnant, when the uterus was only enlarged by a mass of fibrous tumors.

OBSERVATIONS ON THE INFLUENCE OF THE DEATH OF THE FŒTUS,

IN RELATION TO ITS RETENTION IN OR EXPULSION FROM THE UTERUS.[3]

(From Edinburgh Monthly Journal of Medical Science, May, 1848, p. 836.)

Dr. Simpson, in exhibiting some "secondary" fœtuses from the University Museum, stated the following general propositions regarding them. The general law which regulated the occurrence of labor in relation to the death of the fœtus was well known to be this:

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[1] Extracted from Proceedings of Edinburgh Obstetric Society, December 22, 1847.

[2] See ante, page 274.

[3] Extracted from Proceedings of Edinburgh Obstetric Society, February 9, 1848.

1. That usually, in from one to three weeks after the fœtus dies, uterine contractions supervene, and effect its expulsion. But to this very general law there are various exceptions. For—

2. If the embryo die in early embryonic life, and the fœtal appendages continue to live and vegetate, expulsion may not supervene for months. He showed a case of hydatigenous ovum, where the embryo was not larger than an embryo of the sixth week, but the placenta, or rather the chorion, was the seat of hydatigenous hypertrophy and degeneration; and the mother, a patient of Mr. Girdwood's of Falkirk, calculated that she had passed the usual term of utero-gestation, not having menstruated for eleven months previously to the expulsion of this diseased ovum.

3. When the fœtus dies from the third month onwards, in consequence either of disease in its own organization, in its umbilical cord, or in its placenta, and a second twin living fœtus exists at the same time in utero, and this second fœtus continues to grow and keeps up a correspondence of development between the organ and its contents, the dead and undeveloped twin may be retained up to the full term of pregnancy, and be then born along with the other living and full-sized child.

4. When the dead fœtus is thus retained, it is preserved free from the decomposition usually following death, by all access of air to it being prevented. Sometimes it retains its usual rounded appearance and form, if it continues to be surrounded by a sufficient quantity of liquor amnii; but in other cases where this protecting medium of liquor amnii is defective, the fœtus becomes gradually more and more squeezed between two forces, viz., the parietes of the uterus on one side of it, and the other living twin or its membranes on the opposite side; and at last, when born, it is found compressed and *flattened* in form. Two such flattened fœtuses are in the University Museum; and many such cases are on record.

5. The birth of such undeveloped dead twins has sometimes given rise to most groundless and erroneous ideas of the existence of superfœtation.

6. Occasionally when one of twins dies early in pregnancy, it is after a time expelled, when it happens to be situated near or over

the os uteri; afterwards the uterus closes, and pregnancy goes on to the full time with the remaining living child. He mentioned the case of a lady aborting of a fœtus about the third month, going on in pregnancy to the full time, and then being delivered of twins; having originally conceived of triplets.

7. This last circumstance evidently led to the practical deduction—that when a dead fœtus in its envelopes is expelled during the currency of pregnancy, and the uterus notwithstanding still remains large and apparently distended, its further contents should not be in any way interfered with; but rest and other means employed to avert the excitement of any additional uterine contraction, under the hope that a living twin may still be retained, and be carried onwards to the full term of utero-gestation.

HEMORRHAGE IN CONNECTION WITH ABORTION.

INDICATIONS OF TREATMENT.

(From Edinburgh Monthly Journaal of Medical Science, September, 1854, p. 277.)

VARIOUS authors have described cases of fatal hemorrhage accompanying abortion. Dr. Denman has given a drawing of the ovum in such a case still partially attached to the interior of the fundus. A fatal result from this cause is, however, of very rare occurrence. Dr. Simpson described to the Obstetric Society, in 1853, a-case to which he had been called some miles from town, where great hemorrhage had been going on for days, and the patient was already moribund. He removed with ease the ovum, which was impacted in the os uteri. The flooding ceased; but despite of the free use of stimulants, the patient sank in two or three hours. An attempt was made at transfusion; but the surgeon failed to find a proper vein.

Dr. Simpson previously had offered to the Medico-Chirurgical Society, July 3, 1844,[1] some observations on the introduction of the sponge-tent into the os of the pregnant uterus, in certain conditions connected with abortion, as well as a means of inducing premature labor. When abortion was inevitable, and the hemorrhage great, a small expanding sponge-tent, passed into the os uteri, was, he stated, more effectual than a large vaginal plug. It at the same time opened up the os uteri, so as to allow of the more easy escape of the contents, whilst uterine contractions were, in most instances, ultimately induced by its presence. For the same reasons it was often a valuable means both of opening up the os uteri, and exciting the necessary degree of uterine action in those occasionally perplexing cases where, in abortion, the embryo escapes, but the secundines are long retained. He found that the tent, when made and introduced in the mode already stated, required no vaginal plug, or other means to hold it in situ. By its use the first stage of labor, or the dilatation of the os uteri, could, in a great degree, be advanced, before the labor itself actually began.

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[1] See Edinburgh Monthly Journal of Medical Science, August, 1844, p. 735.

INHALATION OF LAUDANUM

AS A REMEDY FOR VOMITING DURING PREGNANCY.[1]

(From Edinburgh Monthly Journal of Medical Science, April, 1847, p. 795.)

Dr. Simpson mentioned to the Society a case in which he had employed the inhalation of laudanum. It was the lady's second pregnancy. She miscarried at the third month during her first pregnancy. On the present occasion, severe sickness and vomiting came on at about the same time after conception, creating great fear of another miscarriage. The retching and vomiting had continued, with slight intermission, for nearly two days, in despite of the use of ice, prussic acid, half-grain opium pills, &c.; and the patient was complaining much of weakness and want of sleep, when Dr. Simpson made her inhale some laudanum for a few minutes from a small ether inhaler, hot water being applied to promote its evaporation. The patient speedily began to complain of drowsiness, and was left in a state of sleep, from which she wakened in a few hours, much refreshed. The irritability of the stomach afterwards disappeared; and in four or five days she was able to proceed on a journey of 300 or 400 miles.

The treatment of obstinate vomiting during pregnancy by oxalate of cerium, will be found on a previous page.—(Ed.)

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[1] Extracted from Proceedings of Edinburgh Obstetric Society, February 10, 1847.

PART III.

NATURAL AND MORBID PARTURITION.

THE DETERMINING CAUSE OF PARTURITION.[1]

(From Edinburgh Monthly Journal of Medical Science, September, 1854, p. 276.)

AFTER stating and refuting the various theories that had been suggested on this point, such as the supposed origin of the act of labor in certain states of vital development or physical expansion of the fundus, body, or cervix uteri—in some supposed conditions of the fœtus, liquor amnii, or placenta, &c., Dr. Simpson suggested that the loosening or decadence of the membranes, or membranes and placenta, from the interior of the uterus, constituted the determining cause of parturition; and that this loosening or decadence was itself the result of the effete degeneration of the structure of the decidua towards the full term of pregnancy. Various circumstances in obstetric physiology and pathology were stated in evidence of this view. It was so far also proved experimentally, for we bring on labor artificially by imitating this process when we separate the membranes with the fingers or catheter; or when we inject tepid water into the cavity of the uterus, or, in other words, between the membranes and interior of the uterus—the latter a plan which Dr. Simpson had followed in twenty or thirty cases.[2]

SOUND HEARD DURING DETACHMENT AND EXPULSION OF THE PLACENTA.[3]

(From Edinburgh Monthly Journal of Med. Science, August, 1852, p. 188.)

M. CAILLANT has described[4] a new stethoscopic sound that he considers indicative of the separation of the placenta. "On applying the stethoscope," he alleges, "to the hypogastric region, after the expulsion of the child, and before the expulsion of the placenta, a sound is heard with each returning uterine contraction, feeble at first, but the intensity of which increases in proportion as the uterine contraction becomes more energetic, and then subsequently diminishes and disappears as the uterine contraction subsides. The sound is composed of a series of rapid small scratching noises, somewhat like those produced by drawing the nails at right angles across the seat of a sofa. It returns with each uterine contraction till the placenta is expelled from the uterus, when it ceases." M. Caillant believes "that it depends, not upon the uterine contractions, but upon the mechanical disunion or separation of the placenta;" and hence he proposes to term the sound "bruit de decollement placentaire."

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[1] Extracted from Proceedings of Edinburgh Obstetric Society, session xii. 1853.

[2] See ante, page 311.

[3] Extracted from Proceedings of Edinburgh Obstetric Society, January 14, 1852.

[4] See l'Union Medicale for 1850, and more lately his Thèse Inaugurale, Paris, 1852.

Dr. Simpson stated the result of M. Caillant's investigations on this supposed stethoscopic indication of the separation of the placenta; but while describing and admitting the sound, he added various reasons for dissenting altogether from the explanation offered of it by M. Caillant. He believed the sound to be produced by the mere physical compression of the placenta, as it is being crushed within and expelled from the uterus; and in various experiments on the subject, he found that it could be artificially imitated with a placenta, after its expulsion from the body, by pressing it through such an aperture as that of the os uteri and os vaginæ.

MECHANISM OF NATURAL LABOR.[1]

(From British and Foreign Medical Review, October, 1841, p. 462.)

In looking at the subject of natural labor, the first important topic which meets us is that of the mechanism of the process. The mechanism of parturition, or the mode in which the fœtal head is applied to and enters the pelvis of the mother, and the different changes which it undergoes in its relations, during its passage from the brim through the cavity and outlet, independent of its physiological and anatomical interest, is undoubtedly of the highest importance in its bearings on practical midwifery. It forms, indeed, the very basis both of the theory and of the art. Its mere mechanical nature may, in the eyes of some, place it inferior in medical interest to other pathological and physiological questions in the department of obstetric medicine, but it is certainly subordinate to none in actual utility.

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[1] From a Review of the works of Rigby, Ramsbotham, and Davis, by Dr. Simpson.

We cannot see how any one can detect, with certainty, the various deviations that are apt to occur from the usual course of labor, unless he be thoroughly acquainted with every step of the mechanism in the natural process. Nor can we see how any one can expect to assist instrumentally with perfect certainty, and to the fullest advantage, the various evolutions and motions of the fœtal head in the different varieties of labor, without knowing what the most natural and consequently the most easy evolutions of each particular variety actually are. No remark, however, is certainly more true than that made by Dr. Denman, that natural labor was the last sort of labor properly studied and properly understood, and we have no hesitation in pronouncing our opinion even at the present day, that though it is a subject worthy of the greatest attention, it has, at least in this country, hitherto excited very little notice, and though a topic on which, above all others, it is necessary to have correct opinions, there are certainly few departments in midwifery upon which the practitioners of Great Britain entertain, generally speaking, more loose and more incorrect ideas.

It is curious, in looking back to the past history of practical midwifery, to weigh the talent and ingenuity that have been bestowed upon the fabrication of instruments to assist, as it was called, in difficult labors, and at the same time to reflect that the fundamental facts on which the principles of their application and employment rest, were entirely unknown to their inventors. Thus, when the Chamberlens first invented the forceps to drag the fœtal head through the pelvis, they neither knew the manner in which the fœtal head enters the pelvis, nor any one of the relations which it occupies in its passage through the pelvic cavity. They, and many of their followers, endeavored to assist nature in her movements before they knew what these movements were.

In making these remarks, we beg to state that we are perfectly aware that various authors, both British and foreign, have long ago endeavored to describe more or less minutely the mechanism of natural labor and its different modifications. Their descriptions, however, were drawn from theory rather than from experience, from fancy more than fact, from rude experiments made with the dry bones of the head and mother's pelvis, and not by careful and minute observation of the process as it is conducted in nature. Hence the accounts of different authors, as might readily be expected, show the greatest discrepancy and discordance even on the same points. Whole paragraphs, and whole pages, as Naegele in his comments upon this subject has remarked, are to be found in obstetric works, with their contents respecting the mechanism of parturition perfectly disagreeing with one another, and still more disagreeing with nature. Relations and movements are described which do not occur, and those that do occur are in many instances left undescribed. Authors have but too often shown how they would move the head through the pelvis, if they had the direction of it, rather than endeavored to show how nature herself conducts the process.

In an article such as the present it is impossible for us to enter at length into the various questions connected with the mechanism of labor. We will offer merely a few remarks to illustrate some more general facts with regard to it, and to show the truth of what we have previously stated in regard to the discrepancy of opinions that long prevailed upon the subject.

In the first place, it is almost unnecessary to observe, that all admit that the head of the child presents at the brim of the pelvis in a very large proportion of cases of labor. This proportion is as great as 94 or 95 in the hundred; or, in other words, the exceptions to this general rule are only about 1 in every 30 cases of labor according to Collins, 1 in 33 according to Osiander, 1 in 34 according to Carus, or, as Meckel has derived it from various obstetric statistics, only 1 in 35.

But the next question in the consideration of the mechanism of parturition is a much more undecided one, namely, in what direction does the head of the child enter the brim of the mother's pelvis? Important as an accurate knowledge of this fundamental fact is to an acquaintance with the subsequent evolutions which the child's head undergoes, and to the practical management of labor, it was not till the middle of the last century that the slightest approach was made to an accurate opinion on this subject. Since that period several different doctrines have been promulgated in respect to the direction in which the long or antero-posterior diameter of the child's head enters the brim at the commencement of the process of parturition.

A glance at the history of these opinions will illustrate the subject better than any formal comments on it.

1. By all the older authors the head seems to have been supposed to enter the superior margin of the pelvis, in the same relation in which they observed it to emerge from the outlet, namely, with the sagittal suture or long diameter parallel to the conjugate or antero-posterior diameter of the brim.

2. In 1742, Sir Fielding Ould, of Dublin, called this first opinion in question, and maintained that, in entering the pelvis, the long diameter of the head lay parallel to the transverse diameter of the brim. We shall quote his own words, as his work is now somewhat rare, and his original observations on this point little known:

"When a child presents itself naturally, it comes with the head foremost, and, according to all authors that I have seen, with its face towards the sacrum of the mother, so that when she lies on her back, it seems to creep into the world on its hands and feet. But here I must differ from this description in one point, which at first sight may probably seem very trivial: the breast of the child does certainly lie on the sacrum of the mother, but the face does not, for it always, when naturally presented, is turned either to the one side or the other, so as to have the chin directly on one of the shoulders."[1]

From this it is evident that Ould believed the child's head to be so greatly twisted round on its trunk "as to have the chin on one shoulder." In 1752, Smellie corrected this notion with regard to the contortion of the child's neck, but otherwise fully adopted Ould's views of the direction of the head. In 1771, Deleurye first promulgated the same opinions in regard to the position of the head in labor, in France; and in later times Schmitt and Mampe, in Germany, have upheld the same doctrine.

3. In 1771, two obstetric essays were published upon the continent, the one by Saxtorph of Copenhagen, and the other by Solayres de Renhac of Montpellier, containing observations made independently of each other, but both agreeing in this one great fact—that the long diameter of the head of the child in natural labor entered the pelvis in a direction neither parallel to the conjugate nor to the transverse diameter of the brim, but parallel to one of its *oblique* diameters; that is, with the sagittal suture running in a line directed at one extremity to the sacro-iliac synchondrosis behind, and to the foramen ovale anteriorly. They further both showed that of the two oblique diameters, the long axis of the head, in a very large proportion, occupied the right, or that running between the right sacro-iliac synchondrosis and left foramen ovale.

Baudelocque, the pupil of Solayres, adopted the opinions of his master as one of the great foundations of his classification of labors, though he did not adhere exclusively to his master's views. The great influence of Baudelocque over the French and other schools of midwifery has spread widely the knowledge of this doctrine of the oblique position of the head at the brim. It was assuredly, however, not until Professor Naegele of Heidelberg published his incomparable essay on the Mechanism of Parturition, in 1818, that the great and almost exclusive predominance of the oblique direction of the head at the commencement of labor was fully acknowledged. His opinions have daily gained more and more ground on the continent of Europe, but though his essay was translated into our own language in 1828, we regret to add that the older opinions on the subject of parturition are still taught in many of our British schools. We feel certain, however, that the more his essay is studied, and the more his observations are accurately compared with what occurs in nature, the more will Naegele's opinions be found to gain weight with the practitioners of this country.

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[1] Ould's Midwifery, p. 28.

From the preceding slight historical sketch it is evident that it has been supposed that the head of the child may, at the commencement of labor, enter the brim of the pelvis in four different positions; namely, with its long diameter, 1st, parallel to the conjugate diameter of the pelvis; or, 2d, parallel to its transverse diameter; or, 3d, parallel to its right oblique diameter; or, 4th, to its left oblique diameter. The first two have been sometimes named the direct or straight positions of the head, the last two the oblique or diagonal. Further, the long diameter of the head may be placed in each one of these pelvic diameters, in two different relations, viz., with the forehead directed to one extremity of it, or the head being completely reversed, the occiput may be directed to the same point.

We make these remarks in order to show the foundation on which different authors have based the classifications of cranial presentations that they have proposed. Various writers have described the head as capable of entering the brim of the pelvis in natural labor in all of these eight positions; others have reduced them to six, and among the authors who have done so, there are some who, as Baudelocque and his followers, have thrown out the two positions referable to the conjugate diameter of the brim, and others who, as Lachapelle, on the contrary, have renounced the two positions referable to the transverse diameter. All our best observers for the last sixty or seventy years have reckoned the oblique positions as the most common. Capuron, Naegele, &c., consider the four oblique as the only position seen in cases of natural labor, and Dr. Rigby has endeavored still further to refine upon this, by reducing the natural presentations of the head at the brim to the two that can occur in the right oblique diameter alone, namely, 1st, with the face pointed towards the right sacro-iliac synchondrosis, and the occiput to the left acetabulum; and 2dly, with the face looking to the left acetabulum, and the occiput to the right sacro-iliac synchondrosis.

The different authors who have adopted the different classifications of cranial presentations to which we have referred, have affixed to each an ordinal number to designate them, but, unfortunately, they have but too often employed the same number to designate different presentations. Hence we find, for example, the third or fourth positions of one author not at all corresponding to the third or fourth of others. It is unnecessary to comment on the discordance to which this has led. In taking up each separate author on the subject, we have first to teach ourselves his particular language on this point, before we can either understand his observations or the rules of treatment which he lays down. As a key to the obstetric reader in his study of some of the more valuable authors of the present day, we beg to offer the following table:

Table showing the Numerical Order of Classification followed by different Authorities in their systematic Arrangements of Cranial Presentation.

Numbers affixed to Presentations, by						DESCRIPTION OF PRESENTATION.
Rigby.	Naegele. Capuron. Maygrier. Duges. Halmagrand.	Baudelocque. Dubois. Gardien. Davis. Dewees.	Lachapelle.	Boivin. Flamant. Moreau.	Ramsbotham.	Anterior part of Cranium pointing
1	1	1	1	1	3	To Right Sacro-iliac Synchondrosis.
	2	2	2	2	4	" Left " "
2	3	4	3	4	6	" Left Foramen Ovale.
	4	5	4	5	5	" Right "
		3		3	7	" Promontory of Sacrum.
		6		6	8	" Symphysis Pubis.
			5	7	1	" Right Os Ilium.
			6	8	2	" Left "

The classification of head presentations adopted by the authors whose works we have under review will be sufficiently apparent from this table. In it we have placed Dr. Davis as following Baudelocque's arrangement, as he evidently professes to do at page 621 of his work, though at the same time we must remark that, after announcing it, he himself does not strictly adhere to that arrangement, for, and we could quote other examples, while at page 622 he describes the third position as that in which the occiput lies behind the symphysis pubis, in page 808 he describes the same third position as that in which the right ear lies immediately behind the symphysis pubis and the occiput directed to the left side of the pelvis. We do not stop to comment on the confusion to which such a discrepancy of statement must lead in the mind of the obstetric student.

Though Dr. Rigby has admirably described the mechanism of parturition in his work, we confess that we feel somewhat dissatisfied with his innovations upon the arrangement and nomenclature of Naegele, whose work he himself first introduced to the British public.

We are so old as to dislike to learn a new language to designate objects with which we have long been familiar. In other respects we can earnestly recommend Dr. Rigby's chapter on the mechanism of labor to the attention of all those who wish to obtain accurate ideas with regard to this matter.

Dr. Ramsbotham has, as will appear from the foregoing table, proposed an arrangement of cranial positions with a numerical order peculiar to himself. We have the same objection, of unnecessary innovation, to urge against him in this respect as we have just made against Dr. Rigby. Further, we certainly consider the number of presentations that Dr. Ramsbotham has described, as too great and complicated. We are not inclined with some authors absolutely to deny that the direct position of the head in the conjugate and transverse diameters cannot possibly occur. We could, we think, if this were a proper place, point out some particular elongated forms of the brim of the pelvis, which would almost oblige nature, if we may so speak, to enter the child's head in one or other of these diameters. But these forms are so exceedingly rare, and these varieties of presentation so very seldom occur in practice, when the child's head and mother's pelvis are of the natural size, that they may certainly be kept out of any classification of labors that are purely normal. Obstetric writers have almost always numbered the presentations in correspondence with the comparative frequency according to which they respectively occur, or, as Naegele has done in regard to his second position, in deference to the numbers already appended to them by others. Dr. Ramsbotham has set aside both these circumstances as the basis for his nomenclature. The numbers he has appended to the positions are, we believe, different from those appended to them by any preceding author; and though he inadvertently states, at page 135, that his first four presentations are "by far the most frequent," yet his fourth (the second position of Naegele) is certainly seldom met with, and is greatly less frequent than his sixth (the third position of Naegele), while both his first and second are alleged by Baudelocque, who introduced them into his own classification of presentations, to be so exceedingly uncommon, that he had himself never met with one instance of them out of many thousand cases; Boivin found them twice in 19,000 labors, and they are hence entirely rejected from the works of most of our recent and best authors.

After this last remark we could have wished that our space had permitted us to show, by the statistical reports of Lachapelle, Boivin, Naegele, Duges, Halmagrand, and others, the comparative proportion of instances in which the different positions of the head have been ascertained to occur in actual practice. It is an important topic, and one to which we may recall the notice of our readers at an early period. In the meantime we shall content ourselves with presenting the few following calculations of the percentages of frequency of the four oblique positions of the head.

The *first* position of Naegele, with the child's face to the right sacro-iliac synchondrosis, has occurred to that author in his own practice in 69 per cent. of all his head presentations, to Halmagrand in 74 per cent., to Madame Lachapelle in 77 per cent., and to Madame Boivin in the ratio of 80 per cent.

The *fourth* position of Naegele, with the child's face directed to the right foramen ovale, has occurred in his own practice in the very small proportion of ·03 per cent. It has been met with equally rarely by others. Lachapelle and Halmagrand have found it in ·04 per cent, and Boivin in ·05 per cent.

So far we find a striking correspondence in the different observations of these authors, in a series of cases amounting in all to nearly 60,000. Their results, however, are much more discrepant as regards the relative frequency of the second and third positions of Naegele. They all nearly agree, be it remarked, in regard to the absolute proportion of cases in which the face is directed to the left side of the pelvis, in the same way as they all nearly agree as to the absolute proportion of cases in which the face is directed to the right side. They differ, however, much as to the proportionate number of instances in which the face, being directed to the left side, is pointed originally towards the left sacro-iliac synchondrosis (constituting the *second* position of Naegele), or towards the left foramen ovale (constituting the same author's *third* position of the head). Thus while Naegele states that in above 1290 cases he only met with the *second* position in one instance, or in the proportion of about ·07 per cent, Halmagrand describes it as occurring in 5 per cent. of his cases, Boivin in 19 per cent., and Lachapelle in as great a ratio as 21 per cent. On the other hand, while Naegele found not less than 359 cases of the *third* position in 1210 cases of cranial presentations, or as many as 29 per cent. of instances in which the face looked, at the commencement of labor, to the left groin, Lachapelle reports only ·07, and Boivin, ·05 per cent. as presenting in the same situation.

Various explanations have been attempted of these and other reported differences of results in the comparative frequency of the second and third positions. The observations of Naegele were, we believe, made by himself in most if not in all the cases to which he refers, most of those of Lachapelle and Boivin by females attached to the Maternité Hospital of Paris. The observations at the hospital were collected, without there having been previously strongly pointed out a great source of fallacy in confounding the second and third positions, and those of Naegele were conducted with a perfect knowledge of and a view to this fallacy. We are inclined, therefore, both from what we have ascertained by our own experience, and from what we know of the high character and candor of Naegele, to attach more credit to the results of his observations than to those who have been opposed to him on this point. And we further believe that the great source of fallacy to which we have alluded and which is so apt to deceive is this, that if the face present to the left, and the position of the head be not examined till it is emerging through the outlet, it will in a very great proportion of cases be found by that time turned round into the second position, though it originally presented in the third. We shall afterwards see that this rotation of the face backwards is the general and normal course followed by cases of the third position.

We have not space to pause and point out the anatomical reasons which, on physical principles, prevent the occurrence of the direct positions, and lead, on the other hand, to such an overwhelming proportion of cases in the right oblique diameter. The subject, however, is one of great interest.

The part of the child's head that lies lowest in the pelvis, or in other words, the exact nature of the presenting part in cranial presentations, has given rise to almost as great difference of opinion as the direction of the head itself. Daventer long ago pointed out the oblique position of the fœtus and the uterus itself in relation to the vaginal and pelvic cavity; but, notwithstanding his observations, we find misstatements in works up even to the present time in respect to this point, and more especially in respect to its influence upon the regulation of the presenting part. The anterior fontanelle, sagittal suture, and still more commonly the vertex, have each been alleged to be the parts that the finger of the accoucheur first touches upon. The acknowledged obliquity of the child's body and head in relation to the vaginal canal might have been sufficient alone to have taught the fact, that it could not be a mesial part of the child's head that presented in natural labor, and we believe that every one now, who has actually studied the matter in nature, agrees with what Naegele first distinctly pointed out, that the posterior and superior portion of one or other of the parietal bones is first felt on examination by the accoucheur. When the head presents with the face looking towards the right side of the pelvis, it is the right parietal bone that first descends; when the face looks towards the left, it is the left parietal bone. On the above points Dr. Rigby offers the following sound generalizations, in his consideration of the mechanism of head-cases of the first position, or with the face looking to the right sacro-iliac synchondrosis:

"The head enters, passes through, and emerges from, the pelvis obliquely; and this is the case not only as to its transverse diameter, but also as to the axis of its brim; the side of the head being always lowest or deepest in the pelvis. This shows the beautiful mechanism of the process, for on account of its oblique position, there is no moment during the whole labor at which the greatest breadth (still less length) of the head is occupying any of the pelvic diameters; even at the last, when the head is passing under the pubic arch, the complete obliquity of its position, in order that it should take up the least possible room, is very remarkable, for the ring of soft parts, by which the head is now encircled, passes obliquely across it, running close behind the left and before the right parietal protuberance. The head never advances with the occiput forwards under the pubic arch, as is stated in works on midwifery; still less with the sagittal suture parallel to the antero-posterior diameter of the pelvis; for the direction of the right lambdoidal suture, as also of the posterior fontanelle, and the position of the cranial swelling, or caput succedaneum, as it has been called, completely prove the inaccuracy of such a theory. The sagittal suture crosses the left labium at an acute angle, the right lambdoidal suture being parallel with the left descending ramus of the ischium."[1]

He adds another important remark:

"Not less incorrect is the theory, for we can call it nothing else, of the head presenting with the vertex, and turning with its long diameter from the oblique into the antero-posterior or conjugate diameter, and the face into the hollow of the sacrum, for it is disproved by all the above-mentioned facts, which careful examination during labor puts us in possession of. When the head is born, the face looks backwards and to the right, viz., to the back part of the mother's right thigh, for the shoulders are by this time passing through the pelvis in its left oblique diameter, the right shoulder being forwards and to the right, and lowest in the pelvis; it is also that which is first expelled."[2]

Dr. Ramsbotham, in the beautiful plates which accompany his work, has given us eight figures illustrative of the modes in which the head of the child may be supposed to enter the brim of the pelvis in each of his eight positions. In these his artist has unfortunately represented throughout, the fœtal cranium as entering the pelvis directly, and not in the oblique mode in which it does in nature. In plate 35, indeed, the fœtal heads are seen, by looking at the basis of the cranium, to be somewhat oblique, but the artist has shaded them so that they look obliquely backwards instead of forwards. In plate 39, as representing the second stage of labor, the face is turned far too directly into the hollow of the pelvis. We much admire, however, plate 41, representing the position of the child immediately after the head is expelled. Dr. Ramsbotham's own description of the usual course of natural labor is in most respects excellent, as are his directions for its treatment. On the management, indeed, both of natural and morbid labors, we have a precision and minuteness that must render his work a valuable practical compendium for the student and practitioner. Dr. Ramsbotham recommends the first two fingers of the *left* hand to be used in vaginal examinations in the first stage, in those cases in which the os uteri is situated so high that it cannot be perfectly investigated by the fore-finger of the right hand. We believe this to be a very useful direction. The artist in representing this mode of examination, plate 42, has incorrectly represented the whole hand, with the exception of the thumb, as introduced into the orifice of the vaginal cavity.

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[1] Rigby's System of Midwifery, p. 126.

[2] Ibid. p. 127.

In supporting the perineum, Dr. Ramsbotham justly states that it is not necessary that we should make powerful pressure, nor resist the child's exit by the employment of any exertion. Continuing, as it were, the resistance and curvature of the sacrum by our hand, we are only to afford passive support; to allow the head, covered by the perineum to be protruded against our hand, rather than forcibly press our hand up against the head. We must recollect further, that, as Dr. Rigby particularly mentions, "the passage of the head is not the only moment of danger to the perineum, for laceration is even still more liable to be produced during the expulsion of the shoulders; any slight rupture of the anterior edge is now apt to be converted into a considerable laceration, unless the support be continued until the thorax be expelled." As a specimen of the excellence of Dr. Ramsbotham's practical directions, and the vigorous style in which he lays down his rules and doctrines on this and other subjects, we extract the following short passage on an important point of treatment first very prominently brought under the attention of the profession by Mr. White of Manchester, viz., the treatment of the delivery of the body after the head is born:

"It used to be the custom to surround the neck with the thumbs and fingers of both hands, and forcibly extract the body the moment the head was in the world, for the purpose of liberating the woman from pain, and terminating the delivery as speedily as possible. Such practice is attended with double danger; great chance of injury to the child, by the tension of the neck, and no small probability of hazard to the mother, by the uterus being prematurely emptied. It is thus left in a flaccid state; the stimulus which previously disposed it to contract is suddenly taken away; that disposition ceases or is suspended; hemorrhage is induced; and necessity probably arises for the artificial removal of the placenta, and incalculable mischief is the consequence. Those persons who commend such meddling interference, and who estimate the skill of the obstetrical attendant by the rapidity with which he can extract the body after the head is born, found their eulogium on the most dangerous premises."[1]

Dr. Rigby describes, with great minuteness and clearness, the treatment, &c., of natural labor, and of the puerperal state. He directs the attention of his readers to auscultatory and other phenomena of labor that have hitherto been little described in English systematic treatises. We extract from his work some remarks relative to a source of prognosis of the duration of labor which we believe will be new to most of our readers:

"The celebrated Wigand of Hamburg considered that the form of the vagina would frequently furnish the means of a pretty certain prognosis as to the duration of labor; thus, if it were wide and yielding throughout its whole length, the labor would be quick, both at its beginning and termination; if, on the other hand, it were small, rigid, and contracted throughout, the labor might be expected to be of a very opposite character. If, on examination, the vagina is found roomy and well dilated at its upper part, but contracted and rigid near the os externum, the labor will be probably quick and easy during the first half, but slow and difficult afterwards; on the contrary, when the os externum is yielding and wide, but the upper portion of the vagina narrow, the labor may be expected to be slow at first, but to be brisk and active afterwards. We have already stated that the course of labor varies in every possible way; in some cases, the same peculiar character of labor shows itself through two or three successive generations; hence it has been observed, that very tedious or very violent and rapid labors sometimes seem to be hereditary; the mother, the daughters, and granddaughters being all remarkable for their lingering or rapid labors."[2]

We extract from Dr. Rigby's chapter on natural labor one more passage to show the different positions, &c., in which women are placed in different countries during the process of parturition. After speaking shortly of the history and form of the old labor-chairs of the continent, he remarks:

"In some remote parts of Ireland and also of Germany, the patient sits upon the knees of another person, and this office of substitute for a labor-chair is usually performed by her husband. Labor-chairs, as far as we are acquainted with their history, were never used in this country, nor have they been used for the last century in France, where the patients are usually delivered in the supine posture on a small bed upon the floor, which has not inaptly been termed *lit de misère*. A modification of the labor-chair is the labor-cushion first used by Unger, and afterwards by the late Professor

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[1] Ramsbotham's Principles and Practice, p. 137. [2] Rigby's System of Midwifery, p. 111.

Von Siebold of Berlin, and Professor Carus of Dresden; it is a species of mattress, with a hollow beneath the nates of the patient for receiving the discharges which take place during the labor. The patient is compelled to lie upon her back during the greater part of the labor, and thus maintain the same posture for some time, which must necessarily become irksome and even painful to her. In this country and in Germany, the patient is delivered upon a common bed, prepared for the purpose, as above mentioned; in England she is placed upon her left side, the nates projecting to the edge of the bed, for the greater convenience of the accoucheur; in Germany, except in Vienna and Heidelberg, where the English midwifery has in a great measure been introduced by Böer and Naegele, the patient is delivered upon her back. In former times the supine posture was also used in this country, but for about a century the position on the left side has been preferred."[1]

The position upon the left side is still, we have reason to know, changed by some old practitioners at the present day into that upon the back, whenever the labor becomes instrumental. This practice is certainly open to many objections, and attended with no advantages of which we are aware.[2] We have heard that in the Edinburgh Infirmary a regular perforated obstetric chair was employed about half a century ago, and we recollect that some thirty years since it was shown by the late Dr. Hamilton to his midwifery class. In Mr. Michael's work on the Ergot of Rye, it is alleged that in Cornwall even at the present day it is difficult to persuade the female in labor to adopt any other than the standing posture, or a position upon her knees.

ON THE FREQUENCY OF

FISSURING AND LACERATION OF THE STRUCTURES OF THE PERINEUM AND CERVIX UTERI IN LABOR.[3]

(From Edinburgh Monthly Journal of Medical Science, May, 1851, p. 488.)

Dr. Simpson directed the attention of the Society to this important subject; and, as the result of a long series of observations on the matter, he drew the following conclusions:

First, Fissuring and laceration of the cervix uteri and perineum are not, as is generally conceived, rare lesions during labor; on the contrary, they are very common occurrences, especially in primiparous labors.

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[1] Rigby's System of Midwifery, p. 109.

[2] Latterly Dr. Simpson has found advantage from placing patients upon the back, particularly in long-forceps cases; and he is inclined to recommend this position as sometimes the most favorable for operating.—(Ed.)

[3] From Transactions of Edinburgh Obstetric Society, January 22, 1851.

Secondly, These lesions are not, as has been often alleged, necessarily the result of mismanagement, but they occur constantly in practice, despite every modification of management, and in cases also in which no kind of management has been adopted.

Thirdly, Evidence of the great frequency of laceration of the anterior structures of the perineum is furnished by,—1. Almost every careful autopsy of women after delivery, whether assisted or not assisted during their labor; 2. By the contracted or shortened state in which the perineum is almost always found, when vaginal examinations are made for uterine disease in women who have borne a family; and 3. By the fissuring or laceration itself being usually traceable, under careful tactile examination, particularly in first labors, when that examination is instituted in the interval of pain, immediately before the passage of the child's head, or after its birth.

Fourthly, Lacerations of the perineum may be often felt beginning in the form of slight roughish rents or fissures upon the mucous surface of the perineum, and these may extend either backwards or forwards; and if they extend forwards, they at last run over the edge of the perineum, and along its cutaneous surface; the mucous and cutaneous structures of the perineum being thus sometimes lacerated, while its middle, cellular, and fascial tissues are comparatively entire, or at least not so deeply and extensively injured.

Fifthly, The proper management and support of the perineum no doubt modifies and diminishes this form of perineal lesion, but it fails far more frequently than is generally supposed, in entirely preventing it.

Sixthly, The evidence of the frequency of fissuring of the os and lower or vaginal portion of the cervix uteri is the same in character, and consists principally—1. In the frequency with which slight laceration of the edges of the os, and of the mucous and middle coat of the cervix, is detected in autopsies after natural labors, and particularly with first children; and 2. In the permanent marks of its previous occurrence, as exhibited in those cicatrices and irregularities of the cervix uteri, which anatomists have long empirically, but correctly, laid down as proofs that they, in whose bodies they are found, have been previously mothers.

Seventhly, Fissures and lacerations of the vaginal portion of the cervix uteri not unfrequently occur to a very considerable extent in cases in which the tissues of the cervix have been rendered rigid by previous inflammation, by carcinoma, or by other morbid causes; and in such cases this fissuring or laceration, if limited to the lower or vaginal portion of the cervix, seems to be accompanied with little or no danger.

In a later part of the Transactions of the Society,[1] it is stated that "Dr. Simpson communicated some observations on the propriety of incising the cervix uteri, in certain cases of rigidity, and referred to the lacerations which frequently take place in the os in natural labor; from the occurrence of such natural wounds, Dr. S. deduced the propriety of having recourse to artificial wounds or lacerations in some cases by incision."

INEFFICIENCY OF UTERINE ACTION AS A CAUSE OF TEDIOUS LABOR.[2]

(From British and Foreign Medical Review, October, 1841, p. 477.)

DRS. DAVIS and Ramsbotham discuss in single pages the nature and symptoms of inefficient uterine action as a cause of lingering labor. The subject is one most highly interesting to the practitioner from the frequency with which it occurs, and important as an object of study from our defective knowledge of its pathology. The various *local* morbid causes of delay and difficulty, both on the part of the child or body to be expelled, and of the maternal passages through which that body has to pass, have each and all been made objects of successful investigation by various accoucheurs. We do not recollect one author who has endeavored, on the other hand, to ascertain and fix the varieties of morbid action and states that, by their presence, may interfere with the organ or organs which are concerned in mechanically expelling the child. And yet we believe, as we have already stated, that, practically speaking, the consideration of this last subject is one of very great moment—perhaps of greater moment than the former.

Dr. Rigby has devoted a very instructive chapter to this topic. It is a chapter which is full of matter calculated to excite the reflection and direct the observations of the obstetric pathologist. We are certainly not prepared to adhere to all the opinions that Dr. Rigby has broached in this part of his work, but we think it requires no stretch of sagacity to predict that it will prove highly useful in rousing the attention of British accoucheurs to the subject, and probably it will be more frequently referred to by after writers, than some of the more finished parts of Dr. Rigby's publication.

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[1] Proceedings of Obstetric Society, Sess. xii. 1853. See Edinburgh Monthly Journal of Medical Science, February, 1855, p. 178.

[2] From a review of the works of Rigby, Ramsbotham, and Davis.

The mechanical powers by which the child is expelled from the uterus are of two kinds, 1st, the involuntary action of the uterus, assisted, 2dly, by the partly voluntary and partly involuntary action of the abdominal muscles and diaphragm. Dr. Rigby, in reference to the morbid conditions of the latter or accessory expulsive powers, states briefly various pathological causes which may more or less impede their action. It would have delighted us if he had entered into this fertile field of inquiry more fully. We have a conviction that the cultivation of it will ere long yield results which will explain the difficulties of many dangerous obstetric cases, and determine more precisely the practice to be pursued in them. The full investigation of the effects of existing cardiac and pulmonary diseases alone upon labor would, we believe, clear up the history and treatment of many at present anomalous cases. Dr. Rigby discusses at considerable length the causes which may retard labor, by inducing a faulty or morbid condition in the involuntary action of the uterus. We wish we had space to extract his views on this matter. For them we earnestly refer our readers to the work itself. We shall give merely some of his opening general observations, to show the spirit in which he treats the subject.

"On the approach of labor, the uterus, which hitherto had been merely performing the office of a receptacle and a means of conveying nourishment to the fœtus, now assumes a totally different character; from being in a nearly passive state, it assumes an entirely opposite condition, namely, of high irritability and powerful action. We might almost suppose, that its connection with the nervous system was become more close and intimate; for it is now sensible to the influence of impressions which had before produced no effect upon it. Thus, we see, that affections of the mind, even but of moderate intensity, and to which it was before labor nearly if not quite insensible, are now capable either of rousing its efforts to the utmost violence, or of arresting them in the midst of full activity; and on the other hand, we see that where its action has been deranged or interrupted, it gives rise to serious affections of the nervous system, or even convulsions. With all this, it now displays peculiarities of function which strikingly distinguish it from all other organs of the body; in some cases it appears to annihilate or to absorb, by its all-pervading influence, the functional energies of other organs; and in spite of its increased nervous power and susceptibility to various impressions, it seems to possess the faculty of continuing its efforts, uninfluenced by general disease, unimpaired by exhaustion, and for a time almost independent of the life itself of the mother. In convulsions and paralysis, in general fever and inflammation of vital organs, its powers appear to be undiminished; on the contrary, where the patient, from whatever cause, is rendered incapable of assisting its efforts by the abdominal muscles, the uterus will take upon itself the whole task of expelling the child, which will be born apparently without a single effort on the part of the mother. We also observe that organs, the various conditions and derangements of which have exerted little or no influence upon the uterus in its state of quiescence during pregnancy, now affect it powerfully, and are capable of modifying its action very considerably. The stomach, the intestinal canal, and the skin, are remarkable instances of this, and seldom fail to disturb or prevent the natural efforts of the uterus whenever these organs deviate from a healthy condition. It will be, therefore, of the highest importance to watch their functions narrowly, in order that we may form a correct estimate of their effect upon the uterus."[1]

Dr. Rigby goes on to show that derangements in the contractile powers of the uterus itself may arise from various morbid states of the organ, both functional and mechanical, as from relative inactivity or debility, from derangement in the digestive organs, from affections of the mind, from age and temperament, from general plethora, or what is probably more frequent, from local uterine congestion, from simple or specific inflammation of the uterine tissues, from irregular or spasmodic contraction in its fibres, from organic disease in its fundus, body or cervix, &c. Each of these divisions receives a separate notice, and their nature, diagnosis, and treatment, are excellently commented on, but for their full discussion we must again refer the reader to the work itself.

In the general management of such cases Dr. Rigby makes one remark which we beg to extract, namely, that "the more carefully such cases are investigated, the less frequently will practitioners require ergot and other oxytoxic medicines." We quote this observation because we believe it strictly true, and because we have seen and heard enough to convince us, that from want of attention to it, the ergot is too often given under circumstances for which it is little or not at all adapted, and where its exhibition is attended with deleterious results both to mother and child. We do not repudiate the use of that drug; we would wish merely, so far as such an observation may serve, to guard against its abuse, and we fear that few medicines are actually more abused in practice. When employed, Drs. Davis and Ramsbotham both recommend the ergot to be given in the form of infusion, and in doses containing half a drachm of the powder, every ten or fifteen minutes. Dr. Davis alleges, that the ordinary mode of exhibiting this drug in America is in an infusion of seven or eight grains of the powder every ten minutes. We have not Prescott's Dissertation at hand to refer to, but assuredly either the dose is accidentally understated, or it is in itself far too little to be of any certain avail.[1] Dr. Rigby employs the powder of ergot in cinnamon water, a scruple or two of the former to an ounce and a half of the latter, and adds a few grains of borax. The cinnamon is an old oxytoxic, and long ago the borax was held in high repute by some practitioners upon the continent, for its supposed efficacy in exciting or increasing uterine contractions. Lamotte speaks of its employment, and in our own day its use has been again revived by Hufeland, L'Offler, and the late Professor Lobstein of Strasburg. Its alleged efficacy is surely a legitimate subject for direct experiments and observations.

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[1] Rigby's System of Midwifery, p. 205.

NOTICE OF A "PULVIS AD PARTUM"

IN THE FIRST EDINBURGH PHARMACOPŒIA.[2]

(From Edinburgh Monthly Journal of Medical Science, May, 1851, p. 493.)

By the older authors, many different medicines are described as being supposed to have an oxytoxic effect upon the uterus; or, in other words, as capable of either originating or increasing the parturient contractions of that organ. From the time of Dioscorides and Galen downwards, various and diversified plants, &c., have been described in medical works, as possessing these alleged virtues. The only two believed at the present day to have a decided oxytoxic effect, are ergot of rye and Indian hemp; and each of these plants, when used medicinally, seems to possess also in common, a depressing or sedative effect upon the spinal nervous system. Is their oxytoxic power dependent upon this action on the centre of the reflex system? Various mineral substances used to be reputed also oxytoxic in their properties, as eagle-stone, &c. Borax has long enjoyed such a reputation. Homberg[3] and Lamotte[4] long ago spoke of its oxytoxic properties. In the first edition of the Edinburgh Pharmacopœia, published in 1699, there is given a recipe for a "Pulvis ad Partum," containing borax as its first ingredient. The powder contains, besides, crocus and amber, substances to which oxytoxic powers were ascribed in olden times; and two animal ingredients that were endowed with the same properties. Its exact composition consists of Venetian borax, British crocus, the livers of eels dried with their gall, white amber, of each two drachms, and of horses' testicles dried in an oven, one ounce.

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[1] See Wood and Bache's United States Dispensatory, 1851, p. 323.

"The dose of the powder to a woman in labor is fifteen or twenty grains, to be repeated every twenty minutes till its peculiar effects are experienced, or till the amount of a drachm has been taken."—(Ed.)

[2] Extracted from Proceedings of Edinburgh Obstetric Society, March 26, 1851.

[3] Histoire de l'Academie Royale des Sciences, 1702.

[4] Lamotte's Midwifery, translated by Tomkyns, 1746, p. 237.

> "℞. Boracis Venetæ.
> Croci Britannici.
> Jecorum cum felle Anguillarum exsiccatorum.
> Succini albissimi, ana drachmas duas.
> Testiculorum Caballinorum in clibano siccatorum unciam unam.
> M. F. Pulvis s. a."

This last ingredient is almost a forestalling of the absurd modern therapeutic doctrine of isopathy. And the whole is perhaps not uninteresting as showing one of the resources officially recommended in tedious labor, in Edinburgh practice, some hundred and fifty years ago.

INDIAN HEMP AS AN OXYTOXIC.[1]

(From Edinburgh Monthly Journal of Medical Science, July, 1850, p. 91.)

Dr. Simpson stated, that, in the early part of the winter session, he had given Indian hemp (Cannabis Indica) in several cases of tedious labor, with the view of ascertaining if it possessed any oxytoxic effect, like ergot of rye, in increasing and exciting the parturient action of the uterus. He had been induced to try the effects, if any, of Indian hemp during labor, in consequence of Dr. Churchill[2] stating, that it possessed powers similar to those of ergot of rye in arresting hemorrhage, when dependent upon congested states of the *unimpregnated* uterus. In the few cases of labor in which it was tried, parturient action seemed to be very markedly and directly increased after the exhibition of the hemp; but far more extensive and careful experiments would be required, before a definite opinion could be arrived at, relative to its possession of oxytoxic powers, and the amount of those powers.

[1] Extracted from Proceedings of Edinburgh Obstetric Society, March 13, 1850.

[2] Diseases of Women, 3d edition, p. 113.

OBSERVATIONS REGARDING THE INFLUENCE OF GALVANISM

UPON THE ACTION OF THE UTERUS DURING LABOR.

(From Monthly Journal of Medical Science, July, 1846, p. 33.)

It has been repeatedly proposed and attempted to induce and increase the parturient contractions of the uterus, by the application of galvanism to the organ.

In 1803, Herder appears to have first suggested the employment of galvanism for this purpose, in cases of slow and tedious labor, in which the contractile action of the uterus offered to become insufficient and defective.[1]

Dr. F. Ramsbotham of London, when treating of "lingering labor," in the excellent Lectures upon Midwifery, which he published in the Medical Gazette in 1834, &c., observes—"I am inclined to think that electrical shocks—particularly derived from the galvanic battery—would excite the flagging powers of the uterus under labor, and perhaps even induce action *ab initio.* This is a means, however, of which I would not, in the present state of our knowledge, recommend a trial; and I only judge by analogy, in consideration of the influence the electrical fluid exerts over the nervous system generally, and, through that system, over muscular fibre."[2]

Some obstetric authorities are inclined to attribute no small portion of the good results derived from the use of the forceps in lingering labors, to the irritating effect of the instrument upon the contractile power of the uterus. They believe, that in addition to its action as a simple mechanical power or agent, the introduction and irritation of the instrument exerts a dynamical effect upon the uterus, by stimulating the organ to renewed and increased contractile efforts. Professors Stein and Kilian have in a particular manner supported this view; and, some years ago, Kilian constructed forceps, with the blades made of different metals, in order that the dynamical action of the instrument might be rendered greater and more equable, by being of a galvanic nature. In 1839, he published[1] the first case in which he employed this galvanic forceps. The labor had been progressing slowly from insufficient uterine action, and for two hours and a half the head of the child had scarcely, if at all, moved. The forceps were introduced; and, as soon as the blades were joined, the uterus appeared to the operator to contract with more power and energy, but still the contractions themselves were quite inadequate, without extractive force, to promote the expulsion of the infant. In the last edition of his learned work on Operative Midwifery, Kilian candidly states, in regard to his galvanic forceps, "After having now (1845) repeatedly employed them, the results have fallen short of my expectations, probably in consequence of the insufficient power of the instrument. I will, however," he adds, "at some subsequent time attempt to ascertain, in a more complete manner, the influence of a strong galvanic stream upon the uterine structure, and perhaps I shall then be enabled to offer something more precise regarding this subject."[2]

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[1] See his Diagnostische Praktische Beiträge zur Erweiterung der Geburtshulfe, Leipsic, 1803; and Meissner, Art des Accouchemens, au 19me Siècle, p. 175; Velpeau, Traité de l'Art des Accouchemens, tom. ii. p. 62.

[2] Medical Gazette for 19th April, 1834. See also the same opinion expressed by Dr. Ramsbotham in the same words, in the two editions of his well-known work on "The Principles and Practice of Obstetric Medicine and Surgery," p. 176 of second edit. 1844.

A case, in which galvanism was employed, in order to induce premature labor, was published in Germany[3] in 1844, by Drs. Hœninger and Jacobi. The os uteri was opened up by a sponge-tent, and ergot of rye exhibited, before the galvanism was applied.

In the Provincial Medical and Surgical Journal of the same year,[4] Dr. Radford of Manchester, published a lecture "on galvanism applied to the treatment of uterine hemorrhage." He proposed, by the aid of galvanism, to induce a state of tonic contraction of the uterus, in "extreme cases of exhaustion from hemorrhage." He further ingeniously suggested the use of the same agent "in hourglass contraction, and other forms of irregular uterine action;" "in tedious labor, depending upon want of power in the uterus;" "in cases where it may be considered necessary to induce premature labor;" and "in certain cases of menorrhagia in the ungravid state." Dr. Radford, at the same time, stated that he had practically ascertained its action. "Galvanism," he observes, "produces an effective and powerful contraction of the uterus; and not only so as regards its tonic contraction, but it has also the power of energetically exciting alternate contraction when applied at intervals. I can," he adds, "tell you most seriously and most solemnly, that it produces these two important changes upon the uterus, in such a degree as in my previous reflections on the subject I had no conception of. The alternate contraction excited by this agent is analogous to, and as powerful as, that which is observed in normal labor, and the tonic contraction is greater."

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[1] Medicinische Zeitung des Vereins für Heilkunde in Preussen, No. xii. 1839. Forbes, British and Foreign Review, vol. viii. 1839, p. 566.

[2] Operationslehre für Geburtshelfer, Hft. 4, 1845, p. 516.

[3] Neue Zeitschrift für Geburtskunde. Bd. xvi. Hft. 3, p. 424.

[4] Provincial Medical and Surgical Journal for December 24, 1844, p. 605.

The publication of Dr. Radford's observations had the effect of strongly calling the attention of the profession in this country to the subject; and several cases, intended to show the probable influence of galvanism upon the parturient action of the uterus, have latterly been published in different Medical Journals.

In conducting physiological or therapeutical experiments upon the *living* economy, where so many disturbing and deranging influences are ever present to perplex and embarrass our results, all are ready to acknowledge the extreme uncertainty which we generally encounter in attempting to trace a true chain of sequence and causation between applied agencies and their apparent or expected results. The difficulty of distinguishing between the *post* and the *propter hoc* in such inquiries is assuredly as great in regard to experiments upon the uterus, as in regard to similar observations upon any other organ of the body. In some instances, and especially where the process of parturition is lingering, the contractile efforts of the uterus occasionally diminish in power, or even altogether cease, and again more or less suddenly or gradually recommence, without our being able to trace out any appreciable cause—mental, vital, or physical—of a nature sufficient in any way to account for the variations which the labor-pains seem to undergo. Besides, the uterus, particularly during labor, is well known to be very readily, and sometimes very powerfully, influenced in its parturient action, by mere states and emotions of the mind alone; and through the medium of the excito-motory system of nerves, the simple application of cold, and other forms of chemical and physical irritation to the surfaces of the abdomen, vagina, cervix uteri, &c., is frequently and strongly capable of stimulating and increasing the contractile functions of this organ. In attempting, therefore, to ascertain the exact influence of galvanism itself upon the uterus during parturition, these and other sources of fallacy in such an investigation must be necessarily, as far as possible, guarded against. With this view, I some time ago planned and instituted a series of experiments, the results of which I now propose to state.

The experiments were begun for the purpose of ascertaining, as far as possible, the exact degree of influence which galvanism possessed over the contractile action of the uterus during labor, and consequently the amount of aid which we might expect to derive from this power, in any case in which we had recourse to its assistance. I did not in the first instance doubt, as I was afterwards led to do by the results of the experiments, the actual oxytoxic influence of galvanism. I believed in its actual existence, and was merely anxious to ascertain and fix its actual amount.

A little consideration will readily show, that any agent capable of increasing the parturient action of the uterus may produce that result in one of three ways. First, It may increase the *force* or *intensity* of the pains, without increasing their duration or frequency; Secondly, It may increase the *duration* of the pains, without increasing their intensity or frequency; and, Thirdly, It may increase the *number* or *frequency* of the pains that occur within a given period, without increasing either the intensity or the duration of the individual pains themselves.

We have evidently little or no power of measuring the existing amount of uterine action by any observations upon the supposed mere intensity of individual pains. The sufferings of the patient, as marking this intensity, would, no one can doubt, prove a most inadequate and fallacious guide in such an inquiry as the present. But we have the power of measuring, with the stop-watch, the *number* of pains that recur within a given time, and the actual *duration* of each pain as it occurs; and these two criteria certainly afford us ample and sufficient means for ascertaining and determining the actual amount of parturient action that may exist within any prescribed period.

In the experiments which I have to detail, we measured the amount of parturient action that was present at different limited periods,—first, by *length* or *duration* of the individual uterine contractions or pains; and, secondly, by the *number* or *frequency* with which they recurred within these periods.

But an explanation of the different data contained within the four different columns, into which the results of the experiments are thrown in the subsequent tables of cases, will illustrate the mode of procedure that was adopted, and its objects, much better than any more lengthened comment.

The *first column* shows the duration of the pains, and the length of the intervals between each, or, in other words, the frequency with which they recurred, for some time before either the galvanism, or the apparatus for it, was put in requisition. Hence this column displays in each case the duration and frequency of the pains, whilst they were still in no respect interfered with by the experimental arrangements subsequently adopted.

The *second column* marks, in a similar way, the duration of the pains, and of the intervals between them for some time after the galvanic apparatus was arranged about the patient, but before any galvanic current was passed through the wires. This method was pursued in order to avoid any mental or physical influence which the arrangement of the apparatus might possibly have upon the patient. Some minutes were generally required in thus adjusting and applying the machine and wires.

The *third column* denotes the duration of the pains, and the length of the intervals between (or their frequency), after the galvanic circle was completed, and the galvanic current thus allowed to pass through the patient.

The *fourth column* presents, on a similar plan, the length of the pains and intervals, for a limited period after the vaginal circle was entirely broken.

In applying the galvanism in the succeeding cases, the galvanic current was generally used of as great a strength as the patient could possibly be induced to bear. In some of the experiments an electro-magnetic coil machine, made by Messrs. Abraham and Dancer of Manchester, and recommended by Dr. Radford, was employed; in others we used an apparatus of a similar construction, and of equal if not greater power, that has long been manufactured for physiological and medical purposes by the Messrs. Kemp of Edinburgh.[1] With either instrument it was easy to excite a stronger galvanic current than the arms of the most determined person could bear for any length of time, when the extremities of the conducting wires were taken hold of by the two hands. In all the experiments, we employed the "vaginal conductor" devised by Dr. Radford, consisting of a strong brass stem, seven inches long, and covered with a nonconducting material up to near its uterine extremity. And the directions which, in the lecture previously cited, Dr. Radford has laid down for the employment of the galvanism, were strictly adhered to.[2]

The subjects of the first six succeeding cases were in-door patients of the Edinburgh Royal Maternity Hospital. The observations upon them, contained in the appended tables, were all noted and collected by Dr. Martin Barry, who was at the time acting as House Surgeon to the Hospital. And in thus mentioning Dr. Barry's name, I am sure that I offer a sufficient guarantee to my professional brethren as to the scrupulous accuracy and conscientious fidelity with which the experiments were conducted, and every item in the results of them carefully noted. They were witnessed by various pupils and others.

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[1] See a description of this instrument given by Mr. Kemp, in the Monthly Journal of Medical Science for November, 1845.

[2] "When the remedy is applied, the brass ball of the vaginal conductor is to be passed up to the os uteri, and moved about at intervals on to various parts of this organ. At the same time, the other conductor must be applied to the abdominal parietes, over the fundus uteri. Shocks may be also passed transversely through the uterus by simultaneously applying the conductor on each side of the belly."—Dr. Radford's Lecture, p. 608.

Case I.—Ann M'Pherson, æt. 20; first pregnancy. Labor came on about the seventh month, and the child was born in a putrid state; the head presented in the first position. The length of the first stage was 9 hours; the second stage only occupied 6 minutes. The labor began on the 6th of April, 1845, at 8 o'clock A.M.; she was delivered at 5½ P.M. The galvanism was used during the first stage. The following observations were begun at 11·13′ A.M., and were finished at 1·47′ P.M.:

Before Application of Wires.		Wires applied, but Galvanic Circle left Incomplete.		Wires applied, and Galvanic Circle Completed.		After Removal of Wires.	
Duration of Pains.	Duration of Intervals.	Duration of Pains.	Duration of Intervals.	Duration of Pains.	Duration of Intervals.	Duration of Pains.	Duration of Intervals.
Seconds.	Seconds.	Seconds.	Seconds.	Seconds.	Seconds.	Seconds.	Seconds.
30	30	40	80	45	75	45	165
33	267	38	22	40	170	37	368
31	209	40	50	35	145	45	300
33	147	38	172	30	120	40	95
35	205	45	135	45	195	30	105
30	150	40	140	32	163	25	95
35	325	35	205	30	195	28	182
35	175	50	190	33	117	35	145
32		42		40		28	
Avr. 33.	188	41	124	37	147	56	125

Case II.—Jane Young, æt. 22; second pregnancy; was delivered of a living child on the 12th of May, 1845. The head presented. The labor was very tedious; the first stage lasting 53½ hours, and the second, 20 minutes. The observations contained in the table below were made during the first stage, being commenced at 7·49′ of the 11th, and terminating at 41 minutes past 10 that morning. The labor began about 11 P.M. of the 9th, and she was delivered on the 12th at 4½ A.M.

Before Application of Wires.		Wires applied, but Galvanic Circle left Incomplete.		Wires applied, and Galvanic Circle Completed.		After Removal of Wires.	
Duration of Pains.	Duration of Intervals.	Duration of Pains.	Duration of Intervals.	Duration of Pains.	Duration of Intervals.	Duration of Pains.	Duration of Intervals.
Seconds.	Seconds.	Seconds.	Seconds.	Seconds.	Seconds.	Seconds.	Seconds.
70	305	40	170	30	90	40	230
60	255	40	215	33	162	43	227
60	210	38	112	30	105	35	205
45	135	39	171	33	87	37	263
40	170	40	200	31	59	37	188
45	180	42	258	34	71	36	219
40	140	41	244	35	25	36	204
45	150	39	201	30	150	37	263
50		37		34		38	
Avr. 51	193	40	196	32	94	83	225

Case III.—Isabella Bowman, æt. 25; first pregnancy; temperament excessively nervous; was delivered of a living child on the 11th August, 1845. The head presented, but the labor was very tedious. The first stage was protracted to 51 hours; the second stage was nearly 3 hours in length. The galvanic experiments were made during the first stage. Labor began at 12 A.M. of the 9th; she was delivered at 7 A.M. on the 11th. The succeeding series of observations were commenced at 9·31′ P.M. on the 10th, and were terminated at 8 minutes past 11.

Before Application of Wires.		Wires applied, but Galvanic Circle left Incomplete.		Wires applied, and Galvanic Circle Completed.		After Removal of Wires.	
Duration of Pains.	Duration of Intervals.	Duration of Pains.	Duration of Intervals.	Duration of Pains.	Duration of Intervals.	Duration of Pains.	Duration of Intervals.
Seconds.	Seconds.	Seconds.	Seconds.	Seconds.	Seconds.	Seconds.	Seconds.
60	210	35	55	37	83	60	150
65	205	35	85	40	380	20	160
30	210	23	7	33	207	36	234
68	202	40	260	40	140	75	225
40	140	50	190	40	140	60	300
60	120	30	180	35	25	70	230
58	62	60	240	30	210	47	193
30	90	43	197	70	230	60	60
55		73		60		37	
Avr. 51	155	43	152	43	177	52	194

Case IV.—Mary Macdonald, æt. 18; first (?) pregnancy; was delivered on the 28th August, 1845, of a premature child, which only gasped a few times after birth. The length of the first stage was 8½ hours; that of the second only three minutes. Labor began about 4 o'clock, A.M., and was completed about half-past 12 P.M. The galvanism was employed during the first stage. The observations were begun at 7·40′ A.M., and completed at 9·54′. The patient was not nervous in the slightest degree. The galvanism caused a continuous tremor. Breaking the connection of the wires produced starts, and nothing more; re-forming the connection had the same effect. It ought perhaps to be observed, that in this, as in the other hospital cases, I have altered the tables in one point—and in one point only—from the mode in which they stand in Dr. Barry's manuscript copies of them. I have omitted, and that merely for the sake of their abridgment and perspicuity, the exact date in minutes and seconds, which Dr. Barry carefully gives in all the observations of the commencement of each individual pain.

Before Application of Wires.		Wires applied, but Galvanic Circle left Incomplete.		Wires applied, and Galvanic Circle Completed.		After Removal of Wires.	
Duration of Pains.	Duration of Intervals.	Duration of Pains.	Duration of Intervals	Duration of Pains.	Duration of Intervals.	Duration of Pains.	Duration of Intervals.
Seconds.	Seconds.	Seconds.	Seconds.	Seconds.	Seconds.	Seconds.	Seconds.
50	115	25	175	45	90	45	45
40	145	58	42	50	85	40	65
65	65	45	90	52	98	55	50
45	75	44	81	59	31	62	98
63	42	59	71	35	70	61	79
64	41	54	111	57	48	60	105
76	74	38	97	52	78	52	83
52	53	35	85	*118	83	62	18
52		53		80	62	50	
				62	90		
				47	38		
Avr. 56	76	46	94	55	70	54	68

In this case the galvanic circle was immediately afterwards a second time completed and broken with the following results:

Connected Wires re-applied.		Wires again removed.	
Duration of Pains.	Duration of Intervals.	Duration of Pains.	Duration of Intervals.
Seconds.	Seconds.	Seconds.	Seconds.
55	70	62	63
57	138	56	109
62	18	60	30
45	53	49	101
55	35	65	55
58		62	
Average 55	63	59	72

Case V.—Mrs. Ross, æt 23; first pregnancy; lymphatic temperament; was delivered of a living child, born at the full time, head presenting, on the 20th November, 1845. The first stage was 23½ hours long; the second stage was completed in 35 minutes. Labor commenced at 11 A.M. of the 19th, and terminated at 11·35′ A.M. of the 20th. The galvanism was used in the first stage. The following observations were begun at 6·9′ A.M. At that time the os uteri was dilated to the size of a sixpence. It was about the size of a shilling when the observations were completed, at 8·53′ A.M.

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* "Probably two pains, with incomplete cessation of the first."—M. Barry.

Before Application of Wires.		Wires applied, but Galvanic Circle left Incomplete.		Wires applied, and Galvanic Circle Completed.		After Removal of Wires.	
Duration of Pains.	Duration of Intervals.	Duration of Pains.	Duration of Intervals.	Duration of Pains.	Duration of Intervals.	Duration of Pains.	Duration of Intervals.
Seconds.	Seconds.	Seconds.	Seconds.	Seconds.		Seconds.	Seconds.
65	205	60	210	65		70	110
70	80	70	65			65	160
40	80	50	205	After 23 minutes, *during which there was no pain*, the wires were removed, when, in the course of a minute and a quarter, the contractions returned according to the rate noted in the next column.		70	125
65	175	65	70			85	12
68	232	55	215			70	200
70	110	70	95			75	120
*105	135	70	155			55	140
80	130	80	220			80	235
65		80	100			70	
		55	185				
		75	105				
		70	65				
		55	80				
		75	255				
		80	70				
		65	70				
		40	20				
		65					
Avr. 63	143	66	129			71	152

Case VI.—Catherine Riley, æt. 24. Temperament sanguineous; was delivered on the 23d October of a living child, at the full time, the head presenting. The labor began about 11 o'clock P.M. of the 21st, and terminated at 2 A.M. on the 23d. The following observations were commenced at 47 minutes past 8 o'clock P.M. on the 22d, and terminated that evening at 10·23′.

Before Application of Wires.		Wires applied, but Galvanic Circle left Incomplete.		Wires applied and Galvanic Circle Completed.		After Removal of Wires.	
Duration of Pains.	Duration of Intervals.	Duration of Pains.	Duration of Intervals.	Duration of Pains.	Duration of Intervals.	Duration of Pains.	Duration of Intervals.
Seconds.	Seconds.	Seconds.	Seconds.	Seconds.	Seconds.	Seconds.	Seconds.
75	15	80	10	60	45	66	69
35	55	45	90	95	100	*185	85
45	60	75	105	90	15	78	57
95	85	95	10	*225	45	90	90
85	20	60	90	80	25	105	15
90	30	85	170	55	80	85	50
95	55	75	15	80	85	70	50
85	65	65	130	74	106	80	55
*160		170		63		75	
Avr. 77	48	75	77	82	63	83	59

At the commencement of the preceding observations in this case, the os uteri was about the size of a crown piece; it became

* "Probably two pains, with incomplete cessation between them."—M. Barry.

fully dilated, and, consequently, the first stage was terminated by 10 o'clock, before the observations were completed.

In all the six preceding cases the labor was more or less lingering and protracted. Such cases were selected because it was only in them that adequate time could be obtained for making the required series of careful and uninterrupted observations. For the same reasons, the continuance of the first stage was chosen as the proper time for conducting the investigation, the changes in the second stage, or at least in the latter part of it, being generally too rapid and great to allow of accuracy in such an inquiry. The results would doubtless have been more satisfactory if the trials and observations had been more extensive. But we met with no small difficulty in extending the inquiry among the hospital patients, even to the length that we have done. In a number of other instances a similar series of observations was begun, but sooner or later interrupted by various circumstances—more particularly by objections on the part of the patient, in consequence of the disagreeable pain and suffering which they underwent from the passage of the galvanic current through their bodies, without that current directly exciting increased uterine contractions.[1]

The data contained in the preceding tables prove, that in the circumstances under which the observations were made, neither does the duration of the pain always go on progressively increasing, nor does the duration of the interval go on progressively decreasing with the continuance of the labor, as some obstetric authorities have alleged.

Further, neither the frequency of the pains, nor their duration, appeared to be affected in any appreciable or direct manner by the employment of the galvanism. An examination of the mean or average results obtained in each case, during the four different periods indicated by the four different columns, will afford us evidence of the justice of this last remark. To accomplish more readily this analysis, I shall collect into a tabular form the mean or average results under each period in four of the cases (viz. the I., III., IV., and VI. cases). I shall immediately allude to the exceptional data afforded by the other two cases, the II. and V.

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[1] Indeed, if it were proved that the galvanism had a direct and useful oxytoxic effect, I believe it would be found no easy task to get patients to submit to the action of so formidable and painful a measure—not to speak of the niceties required in managing the machine, in setting it, and keeping it in proper action—and the difficulty of being able at all times to have such an apparatus speedily and at once within our reach when its services might be necessary, as in sudden hemorrhages.

General Mean Results of Observations upon the Contractile Action of the Uterus in four of the preceding Cases during the four stated Periods.

Cases.	Before Application of Wires.		Wires applied, but Galvanic Circle not completed.		Wires applied, and Galvanic Circle completed.		After Removal of Wires.	
	Duration of Pains.	Duration of Intervals	Duration of Pains.	Duration of Intervals	Duration of Pains.	Duration of Intervals	Duration of Pains.	Duration of Intervals
	Seconds.	Seconds.	Seconds.	Seconds.	Seconds.	Seconds.	Seconds.	Seconds
No. I.	33	188	41	124	37	147	35	182
No. III.	51	155	43	152	43	177	52	194
No. IV.	56	76	46	94	55	70	54	68
No. VI.	77	48	75	77	82	63	83	59
Average,	54	117	50	112	54	114	56	125

The analysis of the individual cases contained in the preceding table affords the following results, in regard to the periods at which in them, the pains were (1) most frequent in their recurrence, and (2) longest in their duration.

1. *Frequency of the Pains.*—In two cases (I. and III.) the interval between the pains was least, and the pains themselves were consequently most frequent during the second period in the observations, namely, when the apparatus was simply applied, and before the galvanism was used. In Case IV. they were most frequent during the period following that in which the galvanic action was put in force; and in Case VI. they were most frequent during the first period, or previously to either the apparatus being arranged or the galvanism applied.

2. *Duration of the Pains.*—In one case (I.), the pains were longest during the second period, when the apparatus was applied without the galvanism; in another (IV.), they were longest in the first period, or before any experimental applications were tried; and in two others (III. and VI.), they were of the greatest length during the period subsequent to the removal of the galvanism.

The last column in the above table shows that, on the whole, the length of the pains and of the intervals was changed and modified in only a slight degree, in these four cases, by the arrangements and applications adopted during the four periods. But in *not one of these four cases were the pains either most frequent in their recurrence, or longest in their duration, during the period of the employment of the galvanism, and whilst the galvanic current was passing through the uterus.*

The two remaining cases (II. and V.), were, as I have already stated, somewhat exceptional, and that in two opposite directions.

In Case II., during the period that the galvanism was applied, the pains were less in their duration than at any other of the three periods—but the intervals between them were also less; or, though shorter in length under the galvanic action, they were more frequent in their recurrence.

In Case V., the result was different from what it was in any of the other five instances—for, as I have already stated, subsequently to the completion of the galvanic circle, only one pain occurred, and then uterine action *entirely disappeared* during the twenty-three minutes that the galvanic current continued to be passed through the body of the patient. The result was rendered only the more remarkable by the fact, that the uterine contractions and pains were regular before the galvanism was applied, and again became regular as soon as the galvanic influence was removed.

In some of the cases above detailed, the labor was of a very protracted nature, and consequently presented so far, exactly one of those conditions in which, as suggested by Herder, and by Drs. Ramsbotham and Radford, the galvanism would probably be useful in strengthening and increasing the pains. We have seen this result unfulfilled. Last year, in two cases of very lingering and difficult labor, occurring, the one in consultation, and the other in my own private practice, I took a further opportunity of attempting to increase the defective action of the uterus by the influence of the electro-magnetic apparatus,[1] and in both instances as signally failed in producing any beneficial effect. I shall state one or two particulars regarding them.

CASE VII.—This case proved so protracted, after the os uteri was fully opened, and at last such local and general symptoms supervened, as to force me to use instruments for the delivery of the patient. Some time previously, I carefully applied, for upwards of an hour, the action of galvanism in the mode already described, but without making the infant's head descend, or increasing either the length or the frequency of the pains. I watched the result with a stop-watch in my hand—contrasting, as in the above tables, the duration of the pains and intervals before, during, and after, the application of the galvanism. And I employed the galvanic current as strong as the patient could be made to endure it. In fact, the skin of the abdomen became at last quite red and erythematic under the irritation of the external conductor.

CASE VIII.—The subject of this second case of protracted labor was deformed, and came from a distance to Edinburgh, in order to be confined under my care. The pelvis was very narrow at the brim, and when the child's head was at last expelled, it was found much compressed and flattened. The labor was extremely tedious, the intervals between the pains being long. The membranes had been ruptured for some hours, and the os uteri was well dilated when the galvanism was applied at 2 P.M. on the 10th August. At this time the pains were steady, and made the patient complain considerably, but they appeared to have no effect in forcing down the child's head. She had not slept during the previous night, but she was by no means weak or exhausted, and she had taken some nourishment. Her pulse was quiet, and the parts were soft and cool. My assistant, Dr. Keith, carefully noted the duration of the pains and intervals for a period before the apparatus was employed —their length and frequency after the apparatus was arranged, but the galvanic circle not completed—and the effects after the galvanic current began to pass through the patient. The results are stated in the following table:

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[1] Throughout these remarks, I have spoken of the power employed, as Galvanism, and in doing so, have conformed to general and conventional usage. Speaking in strictly technical language, it ought to be more properly, I believe, designated Electro-magnetism.

Before Application of Wires.		Wires applied, but Galvanic Circle not completed.		Wires applied, and Galvanic Circle completed.	
Duration of Pains.	Duration of Intervals.	Duration of Pains.	Duration of Intervals.	Duration of Pains.	Duration of Intervals.
Seconds.	Seconds.	Seconds.	Seconds.	Seconds.	Seconds.
90	390	75	390	90	390
60	330	90	420	75	360
90	360	*	300	100	420
150	300	120	300	80	
60	420	60	420	Pains subsequently ceased.	
75					
Average 87	340	86	366	86	390

The patient was not at all alarmed, but seemed rather to be amused with the operation of the galvanism. After the last pain of 80 seconds, noted in the table, there was no appearance of uterine contraction for 13 minutes, when one almost imperceptible pain occurred. The galvanism was continued for some minutes longer, but *no uterine action reappeared;* and from this time till the evening only three slight sensations of pains were felt. As the patient had long wanted sleep, and the passages were perfectly free from irritation, an opiate was then given. She slept well during the night. Uterine contractions, however, did not again occur till the afternoon, or till 24 hours after the use of the galvanism, when they again came on gradually, and increased in severity till the child was born. When born, the heart of the child was pulsating, but all efforts were ineffectual in establishing respiration. Its head, as I have already stated, was much compressed; and on opening it we found clots of blood effused both at the base and into the ventricles of the brain. The mother made a speedy recovery.

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* This pain was slight, and short.

The *general results* obtained from the employment of galvanism, in the eight cases which I have detailed, may be summarily stated as follows:—

In one instance (Case II.), the pains were more frequent in their recurrence, but shorter in their duration, during the application of the galvanism. In five other cases (Cases I., III., IV., VI., and VII.), the employment of the galvanism neither increased the average frequency of the pains, nor their average duration. In one (Case V.), the pains ceased whilst the galvanism was applied, and returned upon its removal. In the instance which I have last detailed (Case VIII.), the uterine action ceased while the galvanism was applied, and did not return upon the withdrawal of the galvanic action, nor for 24 hours subsequently. There was no reason whatever at the time to expect this as a probable occurrence, independently of the galvanism. But even admitting, for the sake of argument, that the cessation of the uterine action was not the result of the galvanic influence used, still the fact is amply sufficient to show that the galvanic current had not, at least, the power either of increasing the pains, or even of continuing and maintaining them when they offered to fail. It may be proper to add, that during the galvanic action, in none of the experiments did Dr. Barry or I find, in the intervals between the *clonic* uterine contractions or pains, any evidence whatever of unusual *tonic* contraction of the uterus, as shown either by any degree of hardness in the general uterine tumor, or by any degree of tension in the pressure of the bag of membranes or the child's head against the cervix uteri.

It would be hasty and logically incorrect to deduce from the preceding observations, that under no modification, and under no manner of application, does galvanism possess the power of directly exciting or increasing the contractile action of the uterus. Forms or methods of employing it may yet possibly be detected or devised, affording a different result. But I believe I am justified in inferring from the preceding inquiry, that as employed at the present time, and in its present mode, it is not a means which can be in any degree relied upon for the purpose in question; and is so far practically and entirely useless as a stimulant to the parturient action of the uterus.

In stating the above conclusions as the result of my own experience, I, of course, by no means wish to impugn, in any way, the validity of the observations made in one or two isolated cases by others, and in which an opposite effect was supposed to be obtained under the employment of galvanism during labor. Uterine contraction may certainly have become occasionally increased while the galvanism was being used, but I strongly question if that increase was the result of the galvanic agency. I have already hinted, towards the commencement of this communication, how much and readily the existing degree of uterine action may be occasionally modified by the state of the patient's mind, or by other still more unappreciable conditions, and how greatly it is in some cases influenced and excited by the application of cold to the surface of the abdomen, or, of mere mechanical irritants to the mucous surfaces of the vagina and cervix uteri. The use of so portentous an apparatus as the electro-galvanic machine, may certainly sometimes influence the uterus through the mind of the patient, by raising feelings of hope and confidence in the results; or the application, to the skin of the abdomen, of the wet sponge attached to the external conducting wire, may excite uterine contraction by reflex action, upon the same principle as cold thus used very generally leads to this effect, both during labor and after delivery. The mere galvanic irritation of the skin or muscles of the abdominal parietes may possibly produce the same result, without the uterus being itself directly influenced by this agency. Or, lastly, and principally, the mere introduction and mechanical irritation of the "vaginal conductor" upon the surface of the vagina and cervix uteri will, I doubt not, occasionally excite more strong and powerful uterine pains, in the same way, and upon the same principle, as the mechanical irritation of the fingers or of the lever or forceps certainly sometimes produces the same consequence. I have already alluded to Stein as attributing much of the good which may be derived from the forceps to their simple dynamical or irritating effect upon the uterine contractions.[1] He has thus shown, according to Kilian, that through the mere *dynamical* influence of the instrument, altogether independently of its mechanical action, "sometimes the irregular character of the pains is reduced to regularity—sometimes when exhausted, the activity of the uterus is reinforced—and sometimes when normal, its power becomes greatly increased."[2] Some years ago, the late Professor Lobstein of Strasburg illustrated this dynamical effect of the forceps by the following case: "Being one day," says he, "upon the point of terminating a labor by the forceps, where its application was indicated by a prolapsus of the cord, scarcely had I introduced the two branches of the instrument, when the contractions, which had already ceased, reappeared with a new force, and drove the head through the cavity, and outlet of the pelvis, with such rapidity, that I had not time to disengage the blades, &c. I attribute," he adds, "to this irritation exercised by an instrument upon the uterus a great part of that astonishing success which the partisans of the lever have obtained."[1] Long before this, Baudelocque expressed the same opinion with regard to the success of Hiberniaux with the lever. "I am fully persuaded," he observes, "that the lever has very little share in it, and that in his hands it is nothing more than a means of irritating the uterus and exciting it to contract with more energy, as we sometimes irritate it with the end of the finger conveyed under the edge of the orifice, and the hand placed on the belly."[2]

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[1] "On the Action of the Forceps," in the Gemeins-Zeitschrift für Geburtskunde, vol. iv. p. 374.

[2] Operationslehre für Geburtshelfer, p. 514.

It is almost unnecessary to point out how apposite these remarks are to the introduction and employment of the long vaginal conductor—as one possible and probable source of fallacy in any observations made, without due caution, respecting the supposed influence of galvanism upon the parturient action of the uterus.

I have already stated that the results at which I have arrived in the preceding inquiry are quite the reverse of those which I expected to obtain when I commenced the investigation. One reason which specially and principally led me to believe that I would find the uterus quite susceptible of galvanic influence, was the knowledge of the long ascertained and acknowledged fact, that all voluntary, and some involuntary, muscles are readily capable of being forced into contraction through its agency. But this *argument from analogy* did not, as we have seen, prove good in point of fact—and is probably not even correct in point of theory. Last autumn, when mentioning the results of some of the preceding experiments to Dr. Sharpey of London, that gentleman directed my attention to a paper published by Jordan,[3] upon the structure and action of the dartos in the male. The dartos, like the uterus, presents under the microscope a structure somewhat different from the ordinary tissue of involuntary muscles, though not exactly similar to each other. Like the uterus, the dartos contracts under the application of cold, and of different mental states, and other stimuli,[4] and like the uterus also, its contractions seem incapable of being excited by the agency of galvanism. Jordan submitted it experimentally to the action of a galvanic battery of sixty-five pairs of plates, without producing any effect whatever upon its contractility.

As a concluding remark, I would beg to take the liberty of suggesting, that perhaps the plan of investigation which I pursued in the present inquiry is one that might be usefully and successfully employed to test the validity, or fix the value, of other supposed oxytoxic measures besides galvanism.

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[1] Compte de la Salle des Accouchées, &c., 1804 to 1814, p. 17.

[2] Heath's Translation of Baudelocque, vol. ii. p. 431.

[3] Meckel's Archives für Physiologie, bd. i. 1834, p. 410.

[4] "The dartos," remarks Cruveilhier, "possesses the property of active contractility, as is seen in the contraction of the scrotum, and the vermicular motions observed in persons exposed to cold, or under the influence of great dread, or of the venereal orgasm, and also in the much more evident contraction of the scrotum after an irritating injection has been thrown into the cavity of the tunica vaginalis."—Cruveilhier's Anatomy, by Madden, p. 596.

ON THE SEX OF THE CHILD[1]

AS A CAUSE OF DIFFICULTY AND DANGER IN HUMAN PARTURITION.

(From Edinburgh Medical and Surgical Journal, October, 1844, p. 387.)

My object in the following observations is to show, that the Sex of the child has a very marked and demonstrable influence upon the difficulties and dangers of human parturition, in relation to the fate both of the mother and of the infant.

Two winters ago I had occasion to undertake some investigations into the causes of the difficulties attendant upon parturition in the white, as compared with the black tribes of mankind; and in the human female, as compared with the female of the lower animals. In consequence of these investigations I was led to conclude that the adaptation of the head of the fœtus to the maternal canals is so very close and accurate in the process of parturition, as it occurs in the female of our own race, that deviations of a very slight degree in the relative size of the cranium of the child, and of the pelvic passages of the mother, should, when viewed on the large scale, lead to differences of a very appreciable and notable extent in the relative safety or danger of the whole process.

On further considering the subject it appeared to me, that in the slight difference which is known to exist between the size of the heads of the male and female child at birth, and in the effects which this difference might or might not be traced to produce upon the results of parturition—a criterion existed by which the truth of the opinion in question might be fully tested. It was at the same time manifest that the subject was one of such a nature that the only evidence by which it could be settled obviously consisted of an appeal to an extensive body of statistical facts. The first difficulty was that of obtaining these required data. The numerous and excellent reports of instances of morbid and instrumental labor published by various authors are, however valuable in other respects, totally useless in regard to the present subject—in consequence of the sex of the child being rarely or never mentioned by them in the details which they give of their individual cases. The only exception to this general remark that I am acquainted with is to be found in the admirable Practical Treatise of Dr. Collins.[1] In his invaluable record of the morbid cases which occurred in the Dublin Lying-in Hospital during his Mastership,[2] Dr. Collins gives, at the end of the different chapters that treat of the individual difficulties and complications attendant upon labor, a table showing, amongst other items, the *sex* of the infant in each case. Dr. Collins himself properly suggests, that "an extensive record" of the kind which he has published might "materially assist the physician in obscure investigations."[3] For the data forming the grounds of the following calculations I am, as will be seen in the sequel, principally indebted to the facts recorded in Dr. Collins' work; and while the inferences I am about to draw form one instance of the fulfilment of his own suggestion, regarding the utility of such data as he has collected, my deductions will, I doubt not, be regarded as the more valuable and trustworthy, seeing that the facts on which they are chiefly founded, were originally collected and published by Dr. Collins, without apparently any view whatever to the special inquiry upon which we propose to enter.

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[1] Read before Edinburgh Obstetric Society, January 10, 1843.

Having made these preliminary remarks, I shall now proceed to show that the sex of the child exerts a manifest influence upon the general maternal mortality accompanying human parturition, upon the frequency and fatality of its individual morbid complications, and upon the safety and life of the infant itself, both during birth and for some time subsequently to it. In all of these respects the birth of male children is attended with much greater danger than the birth of females. In proof of this, I have to adduce different series of corroborative details. These details will probably be exhibited with the greatest clearness, in the form of such a series of propositions as the data which I have to bring forward may seem to warrant. And I may further arrange these propositions into more general heads or chapters, so as to show the effect, during parturition, of the sex of the child as bearing—1. Upon the safety and life of the mother; and—2. Upon the life and safety of the infant itself; 3. I shall enter into some investigations with the view of developing the cause of the greater comparative dangers accompanying the birth of male children; and, *lastly*, I shall allude to some practical inferences, and attempt to point out the extent to which the mortality in childbirth and early infancy is influenced by the mere sex of the offspring.

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[1] "A Practical Treatise on Midwifery, containing the result of 16,654 births occurring in the Dublin Hospital, during a period of seven years, commencing November, 1826. London, 1836.

[2] "*Master* is the title of the physician to whose care this hospital is intrusted for a period not exceeding seven years." See Dr. Collins' preface.

[3] Ibid. p. 1.

THE DANGERS AND DIFFICULTIES OF PARTURITION ARE GREATER TO THE MOTHER IN MALE THAN IN FEMALE BIRTHS.

The greater danger and difficulties which the mother incurs in connection with male births may be demonstrated both by an appeal to the general mortality among parturient mothers, and by a reference to the sex of the infant in a variety of the most formidable complications that are found to accompany the process of labor. One or two propositions, and the statistical proofs on which they are founded, will illustrate this remark.

FIRST PROPOSITION.

Of the mothers that die under parturition and its immediate consequences, a much greater proportion have given birth to male than female children.

During Dr. Collins' term of mastership 16,414 women were delivered in the Dublin Lying-in Hospital. Of these, 164 died. Of the 164 that died, 7 had been delivered of twins, and 157 of single children. In searching for the effect of the sex of the infants in relation to the maternal mortality, I shall on this, as on other occasions in the following remarks, reject from the calculations the twin cases, because the plurality of the offspring is an element that would obviously disturb and pervert, in different ways, the accuracy of any investigations like the present; for the fœtuses, in compound births, are frequently of opposite sexes—they are in general considerably below the average size and weight—and the expulsion of the second of them, when once the maternal canals are dilated, is not attended with such obstacles, nor hence with such danger, as the passage of the first or the transit of a single child. If we confine, then, our attention to the 157 cases in which the mothers died in connection with the birth of a single child, we will find that in 3 the sex of the infant was not noted; in 105 it was male; and in 49 female. The number of maternal deaths was therefore much higher after male than after female births. But, to show with more clearness and precision the relative excess of mothers that died after giving birth to male, as compared with those that died after giving birth to female children, I shall, on this as on other occasions throughout the present essay, compute the casualties connected with female births as equal to 100, and calculate the proportion of those connected with male births in reference to this fixed standard. In this manner the absolute numbers and relative proportion of maternal deaths, after male and female births, contained in Dr. Collins' returns, would, according to the above data, stand as follows:

Total maternal deaths.	In these the sex of child.		Or in proportion of male to female as
	Male.	Female.	
154	105	49	214 to 100

It is proper further to remark, that 38 out of 157 cases appear to have died from "causes not the result of childbirth." I have not been able, from the details given by Dr. Collins, to separate these 38 cases from the general number, but if they were so subtracted, the remaining 119 cases would not improbably show a greater proportion of male births in connection with those maternal deaths that thus directly resulted from parturition or its immediate effects.

SECOND PROPOSITION.

Among labors presenting morbid complications and difficulties, the child is much oftener male than female.

This proposition may be proved by a reference to many of the more formidable accidents that are found to accompany labor, as convulsions, laceration of the uterus, post-partum hemorrhage, instrumental cases, &c.

Convulsions.—Of 28[1] cases of convulsions described by Dr. Collins, in 17 the offspring was male, and in 11 female; of the male children, 8 were still-born; of the female, 4.

Rupture of the Uterus.—This formidable accident occurred in 34 instances during Dr. Collins' mastership. In 23 cases the rupture was in connection with the birth of male children, and in 11, or in less than the third of the whole, it occurred in connection with the birth of female children.[2]

Puerperal Fever.—From November, 1826, to November, 1833, 88 patients were attacked with puerperal fever in the Dublin Lying-in Hospital. Among these 88 women, 54, or five-eighths, had given birth to male, and 34, or three-eighths, to female children.

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[1] Here, as in most other cases, our calculations include only, as already stated, the results of uniparous births.

[2] Dr. Collins adduces 20 cases of rupture of the uterus mentioned by Dr. M'Keever, and out of these, in 15 the child was male, and female in the remaining 5, or in about one-fourth of the whole.

Post-Partum Hemorrhage.—This complication is known to occur especially after those confinements in which the contractile powers of the uterus have been unusually taxed and exhausted during the previous stages of the labor. Dr. Collins reports 44 cases of hemorrhage after the expulsion of the placenta. In 31 of these the child was of the male, and in 13, or less than one-third, it was of the female sex.

Tedious and Difficult Labors.—The duration of the labor was noted by Dr. Collins in 15,580 cases. In 264 of these cases the process was prolonged beyond 24 hours. Dr. Collins has not, in his chapter on tedious labors, published the particulars of all these 264 instances, nor given such data as to enable us to ascertain the sex of the children in them. These data would, in all probability, have shown a great overproportion of male infants—more especially in that class of instances in which the delay was produced by protraction of the second stage of labor, in consequence of want of proper relative size between the fœtal head and the maternal passages.

In another part of his work, Dr. Collins details a set of facts that may in a great measure fill up the deficiency to which we allude. In his chapter on "Still-born Children" he notes a variety of important circumstances, relative to 1121 cases in which the child was dead at birth. He further observes,[1] "In 106 of the 1121, the labor was extremely severe, and in nearly half of these the patients had been *one*, *two*, *three*, days ill, or even more, before admissión into the hospital, and most of them grossly mismanaged." He gives a short outline of 49 of the 106 cases of tedious labor in the General Table on Still-born Children, and marks out by asterisks 60 additional instances, which were, to use his own words, "similar in most respects to those detailed."[2] These two sets of cases of protracted labor with death of the infant, amount in all to 109 instead of 106.[3] Out of this number the sex of the child was male in 65, and female in 44.

Forceps Cases.—The forceps were used in 24 cases during Dr. Collins' mastership. Of the children thus delivered, 16 were males, and 8 females.[4]

Crotchet Cases.—"Of the 16,654 births which occurred in the hospital, in 79," says Dr. Collins,[5] "delivery was effected by lessening the head, on account of extreme difficulty in the labor, or where the child was dead and interference necessary for the patient's safety." In the section on Still-born Children, 41 of the 79 are recorded, and references given to other 33 of the cases.[1] Among these 74 crotchet cases that are thus noted, the offspring was male in 50, and female in the remaining 24 instances.

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[1] Practical Treatise, p. 462.

[2] Ibid. p. 485.

[3] In 97 of these 109 instances of tedious labor, the number of hours during which the patients were in labor is stated. Five were under 12 hours in labor; 14 were from 12 to 24 hours; 19 were from 24 to 36 hours; 27 from 36 to 48 hours; 21 from 48 to 60 hours; 10 from 60 to 72 hours; and 1 was 90 hours ill.

[4] Ibid. pp. 11 and 15.

[5] Ibid. p. 486.

If we attempt to throw the proofs that we have collected of our present proposition under the seven preceding heads into a tabular form, and, taking again, for the facility of comparison, the female births at the fixed standard of 100, calculate the proportion of males to them, we shall find the result to be as follows:

Nature of Complication.	Total Cases.	No. Male Children.	No. Female Children.	Proportion of Males to Females as
Tedious Labors,	109	65	44	148 to 100
Convulsions,	28	17	11	153 to 100
Puerperal Fever,	88	54	34	161 to 100
Rupture of Uterus,	34	23	11	207 to 100
Post-Partum Hem.,	44	31	13	240 to 100
Forceps Cases,	24	16	8	200 to 100
Crotchet Cases,	74	50	24	208 to 100
Total,	401	256	145	165 to 100

It is unnecessary to qualify the proof which the above table affords of the proposition that we have laid down, by venturing to offer any comments. It is proper, however, to add that here, as elsewhere throughout the calculations of the present essay, it should be held in recollection that about 6 per cent. ought always to be deducted from the column of the male children, in consequence of there having occurred, in concurrence with an acknowledged law in human statistics, that proportion of male over female births during Dr. Collins' term of mastership in the Dublin Hospital. He noted 8548 male births, and 8068 female births, being in the proportion of 106 males to 100 females.

Whilst it is requisite to correct our arithmetical results and tables to this extent, it will, at the same time, at once be seen that the required deduction is so slight as not in any material degree to interfere with the essential evidence of our various propositions.

THE DANGERS AND ACCIDENTS FROM PARTURITION AND ITS RESULTS ARE GREATER TO THE CHILD IN MALE THAN IN FEMALE BIRTHS.

The increased danger to infantile life dependent upon the sex at birth, may be demonstrated by a reference to the sex of the children in those cases in which the mothers die from labor or its consequences. It may be proved still more strongly and easily, by an appeal simply to the sexes of those children that are dead at the moment of birth, without any regard to the fate of the mother; and by the consideration of the comparative number of accidents and deaths among male and female infants consequent upon delivery.

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[1] Practical Treatise, p. 487.

We proceed to illustrate each of these points in the form of two or three propositions and their proofs.

THIRD PROPOSITION.

Amongst the children of the mothers that die from labor or its consequences, a larger proportion of those that are still-born are male than female; and, on the contrary, of those that are born alive, a larger proportion are female than male.

This proposition may evidently be considered under two heads. In the first place, we shall show that a greater proportion of male than female children are found among those infants that were brought forth still-born, by mothers who themselves died from labor or its consequences.

During Dr. Collins' mastership there occurred, as we have formerly seen, 154 cases in which the mother died after the birth of single children. The 154 mothers produced 105 male and 49 female children.

The result of the mortality of these infants at birth relatively to their sex, may be reduced to the following short tabular form:

Total male births 105; of these, born dead 50, *or* 49 *per cent.*
Total female births 49; of these, born dead 16, *or* 34 *per cent.*

In the second place, we remark, that, of those children who were born alive by mothers that died from labor or its consequences, a greater proportion were females than males.

Here we must again take into our calculation the number of mothers who died after uniparous births in connection with the absolute number of male and female children which they produced, when the result, as far as regards those children who were born alive, may be stated in the following form:

Total male births 105; of these, born alive 55, *or* 52 *per cent.*
Total female births 49; of these, born alive 33, *or* 67 *per cent.*

The general proposition may be further proved and illustrated by throwing the results we have just given into one such common table as the following:

Sex.	Total Number.	Of these, dead.	Of these, living.	Proportion of dead to living, as
Male,	105	50	55	95 to 100
Female,	49	16	33	48 to 100

Or to state it conversely:

Sex.	Total Number.	Of these, living.	Of these, dead.	Proportion of living to dead, as
Male,	105	55	50	110 to 100
Female,	49	33	16	206 to 100

We thus observe, that, amongst the children of mothers dying after uniparous labors, males are born *dead* in the ratio of 95 to 100 living, while with females the ratio amounts only to 48 dead to 100 living. But we find, on the other hand, under the same circumstances, that males are born *living* in the ratio of 110 to 100 dead, while among females the average amounts to the ratio of 206 living to 100 dead.[1]

FOURTH PROPOSITION.

Of still-born children a larger proportion are male than female.

It has already been stated, that, including the whole births in the Dublin Hospital, the male children born were, to the female, in the ratio of 106 to 100. By extended statistical returns, a similar observation has been proved to hold good as a general law in human reproduction, at least among European females.[2] M. Bickes, whose results are founded on seventy millions of observations, has shown the proportion of male births to female births to vary in European nations from 104 to 108 boys for every 100 girls.[3]

All writers on human statistics seem further to acknowledge, that among still-born children, males occur in a much greater ratio than females. This ratio exceeds very considerably the normal disproportion between the sexes at birth, and is therefore not explicable by it.

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[1] It was not till after the preceding calculations and remarks had been written that I met with the following confirmatory statement in Dr. Clarke's able letters to Dr. Price.—(See Philosophical Transactions for 1786, p. 349.) Speaking of the maternal mortality in the Dublin Lying-in Hospital from 1757 to 1784, Dr. Clarke observes—"I found that of 214 women dead of single children, 50 were delivered of still-born males, and 15 of still-born females; 76 of living males, and 73 of living females." Hence, calculating upon these data of Dr. Clarke, it would appear that, during the above period, male children were born dead in the Dublin Hospital in the ratio of 66 to 100 living, while with female children the ratio amounted only to 20 dead to 100 living; and, on the other hand, the males were born living in the ratio of 152 to 100 dead, whilst the females were born living in the much higher ratio of 487 to 100 dead.

[2] For an exception among the free population residing at the Cape of Good Hope see Hawkins' Medical Statistics, p. 51. It is not unworthy of remark, that, among twin children, there seems to be more females than males born. In the Edinburgh Medical and Surgical Journal for January, 1844, p. 113, I have reported the sexes of the children in 788 twin cases. Among these there were 756 males and 820 females, or the males were to the females in the proportion of 92 boys to 100 girls. See page 291 of this volume.

[3] Die Bewegung der Bevölkerung mehrerer Europ. Staaten.

The excess of male still-births and the different degrees of it in different returns, may perhaps be exhibited most simply in the form of a table, such as we have used in speaking on this subject in our lectures, and which we here insert.

Locality.	Proportion, among Still-born Children, of Males to Females.	Reporters.
In Amsterdam,	120 to 100	Lobatto.
Geneva,	125 " 100	Mallet.
Wurtemburg,	127 " 100	Riecke.
Prussia,	135 " 100	Hoffmann.
Halle,	140 " 100	Guete.
London,	140 " 100	Bland.
Berlin,	142 " 100	Süssmilch.

During Dr. Collins' superintendence of the Dublin Hospital, 1121 children were born dead. The number of males to females among these 1121 children stood as follows:

Still born Males.	Still-born Females.	Or in proportion of Males to Females, as
614	507	122 to 100

In Dr. Collins' reports, the over-proportion of still-born males, though strongly marked, is not so great as in some of the other returns we have quoted. We believe that probably the variation in this respect among the evidence adduced in the preceding table, may be partially explained by the supposition, that in some of the returns, premature children, and perhaps in others those that were putrid at birth, have not been included. At all events, whenever these are omitted, the number of still-born males in proportion to females will be found to increase. In other words, if we limit our calculations to infants still-born at the full time, and if we consider only those that die during the process of labor, we shall find among them the ratio of males to females to be greatly higher than the average is among still-born children taken indiscriminately, and without reference to the time and cause of their death.

We shall devote our next proposition to the proof of this.

FIFTH PROPOSITION.

Of the children that die during the actual progress of parturition, the number of males is much greater than the number of females.

It will easily be seen that the data required in proof of this proposition are very difficult to be obtained. Indeed, they could only be accurately ascertained by a series of minute observations instituted through the medium of the stethoscope. This has not yet been done, so far as we know, by any observer. From the want of such evidence, we are under the necessity of approaching as nearly as possible the proper proof, by taking advantage of a mark entered in the elaborate tables of Dr. Collins. He has placed a note of "p" (signifying putrid), opposite to all those children that were born in that state. These "putrid" children had evidently all, or almost all,[1] died from causes that had operated during intra-uterine life, and before the commencement of parturition. We may, therefore, probably reject such infants from our present calculations, inasmuch as these calculations refer only to children dying *during* the continuance and progress of the parturient process. Of the 1121 children that were still-born, as many as 527 were found putrid at birth. The sexes of the remaining 594 stand in the relation shown by the following table:

Still-born Children not putrid.	No. of Males.	No. of Females.	Proportion of Males to Females, as
594	357	237	151 to 100

In the general rate of infantile mortality at birth, we have already seen, under the previous proposition, that in Dr. Collins' cases the proportion amounted to about 122 males for every 100 females. In the present instance, and when the children that had died some time before labor began, are excluded, the ratio rises as high as 150 boys to every 100 still-born girls, or as 3 to 2. We might further from the 594, exclude 62 premature still-born children that were not putrid, viz., 37 males and 25 females, and retain only those that had reached the full time. This, however, would not alter our results above 1 in the column of 151 males.

But the ratio of these male still-births would probably have been made somewhat greater, and our present proposition still more strongly borne out, had we been able, on the one hand, to take into the calculation those children only who died subsequent to the commencement of the process of parturition, and to exclude, on the other hand, all those that perished, however shortly before, under the influence of other morbific agents than the effects of labor. The fact of the still-born children being "putrid," shows generally that death has occurred several days at least before birth. It is a condition which has enabled us therefore to exclude most, but not all, of those who had died antecedently to the commencement of actual labor. I repeat, that if we had possessed the power of excluding all—and thus so far improving our data—the proportion of male to female children who died during labor, high as we have shown it to be, would, in all probability, have been found raised still higher.

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[1] For two remarkable exceptions see Dr. Collins' treatise, pp. 470 and 483, cases 461 and 1058. In a paper on Peritonitis in the Fœtus (Edinburgh Medical and Surgical Journal, October, 1838, p. 390), I have attempted to show that a large number of those children that are still-born and putrid have died of peritoneal inflammation. See, also, vol. ii. of this work.

SIXTH PROPOSITION.

Of those children that are born alive, more males than females are seen to suffer from the morbid states and injuries resulting from parturition.

Among the accidents and injuries to the child more immediately resulting from parturition, no one has, of late years, been made the subject of such minute pathological investigation as the "bloody tumor" or "ecchymosis" of the head—the *Cephalæmatoma neonatorum* of Naegele, Valleix, Dubois, Walshe, and various other modern authors. "The unequal pressure," says Velpeau, "which the cranium experiences, and the tendency that its bones have to override one another, during most labors, when the head is traversing the pelvis, and whether the labor be natural or artificial, is incontestably the most common cause of this tumor."[1] No author, that I am acquainted with, has given any statistics regarding the sexes of the infants affected with cephalæmatomata, with the exception of Burchard. The data which he adduces are not so extensive as could be wished, but still they furnish strong and indubitable proof of our present position. In his essay, "De tumore cranii recens-natorum sanguineo," he mentions that out of 43 cases, which he had observed at the Breslau Hospital, the child was male in 34 and female in 9 of the instances, so that the relative frequency of the disease in the two sexes stood thus:

Male Children.	Female Children.	Proportion of Males to Females.
34	9	377 to 100

I am not aware that there have been hitherto published such data regarding serious swellings of the infant's scalp, the *caput succedaneum* of the older authors, or the asphyxia or apoplexy of children at birth, or any of the other accidents to which they are subjected during labor, as would enable us to make similar calculations with respect to them. As they are all confessedly more or less direct results of the physical pressure which the infant's head suffers during parturition, the calculations, derived from one, would probably apply with greater or less force to each of the others. Since additional data from *individual* morbid states and injuries are thus wanting in support of the proposition we have laid down, we shall appeal, in confirmation of it, to some calculations we have made from Dr. Collins' data regarding the *general* mortality observed among children immediately after delivery. In his chapter on "Children dying in the Hospital," Dr. Collins observes, "thirty-two children born at the full period died a few minutes after birth; in six of these, respiration could not be established, though the heart's action continued for some time."[1] Dr. Collins does not offer any clue by which these 32 cases can be discovered in the table attached to this chapter, and thus their sexes ascertained. I find, however, in that table, that 17 single children, born at the full period, are marked as dying within the first half-hour after birth. They hence probably perished from morbid states the more immediate results of labor. Of the 17 children, 9 died within five minutes after birth; 1 in ten minutes; 4 in fifteen minutes; and 3 in thirty minutes. One of the 17 children was female, and 16 of them males. If we might venture to make the same form of calculation as we have hitherto used, of the respective sexes of the children from these very small and insufficient data, it would stand thus:

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[1] Traité Complet des Accouchemens, tom. ii. p. 594.

Male Children.	Female Children.	Proportion of Males to Females, as
16	1	1600 to 100

SEVENTH PROPOSITION.

More male than female children die in the earliest periods of infancy; and the disproportion between the mortality of the two sexes gradually diminishes from birth onwards till some time subsequently to it.

This proposition follows as almost a necessary corollary from those that have preceded it. If, in consequence of the pressure and greater injuries to which male children are subjected during birth, more male than female infants perish during labor; and if, again, among those born alive more males than females are found to suffer under such morbid states as are the immediate results of parturition, it might justly be a priori expected that the same causes which produced these results during delivery and immediately after it, would so far *continue* to affect the male constitution, for some time subsequently, as to predispose it more to disease, and likewise render the diseases which did occur in it more dangerous and fatal in their course than those that affect the female. Further, if this greater liability to morbid action, and its greater intensity and fatality in the male, as compared with the female infant, were the consequence of the male being subjected to greater physical injuries at the time of parturition, the pathological characteristics in question should be observed to diminish more and more in the male system from the moment of birth onwards, because the morbific effects, resulting from any cause or causes operating during birth, would thus progressively diminish, and ultimately pass away. At last, therefore, at some date in early infantile life, the mortality among male and female children, though very different at first, should become nearly or entirely equal. And such, indeed, is the actual state of facts, when the mortality during infancy is investigated upon a large scale. Thus in his observations regarding the "influence of sexes" upon mortality, Quetelet, in his elaborate "Treatise on Man," remarks, as a matter of statistics, that among male as compared with female children, "the ratio of deaths before [during] birth is as 3 males to 2 females; during the first two months after birth the ratio is as 4 to 3; during the third, fourth, and fifth months, as 5 to 4; and," he adds, "after the eighth or tenth month a difference scarcely exists."[1]

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[1] Practical Treatise, p. 501.

I shall endeavor to bring forward some statistical evidence in support of the preceding statements.

Dr. Collins has given in his Treatise,[2] a table containing a variety of particulars regarding 284 children that died in the Dublin Hospital within a few days after their birth. Excluding twins and premature children, the date of the death of 148 of these infants is noted in the table referred to. Six of them died on the 8th day after birth; two on the 9th; and one on the 10th. These nine included 5 boys and 4 girls. With regard to the remaining 139 infants that died during the first seven days after birth, the following table, which has been compiled with considerable care, will show the periods of their demise, and the relative proportion which the mortality among the male and female children presented at different dates during the first week of life.[3]

Period of Death.	Males.	Females.	Proportion of males to females.	Ratio of Excess of male mortality.
Within first half hour, .	16	1	1600 to 100	1500
Within first hour, . .	19	2	950 to 100	850
Within first 6 hours, .	29	7	414 to 100	314
Within first 12 hours, .	34	15	226 to 100	126
Within first 18 hours, .	36	19	191 to 100	91
Within first day, . .	49	28	175 to 100	75
Within first week, . .	80	59	136 to 100	36

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[1] Treatise on Man, Chambers' English edition, p. 50.

[2] Dr. Collins' Treatise, p. 519, &c.

[3] The time the mothers remained in the institution, in most instances, was for "a period of eight, nine, or ten days after delivery."—Ibid. p. 500.

The last column in the above table shows in the most striking manner both the great proportion of male over female deaths in the first few hours and days of extra-uterine existence, and the rapid diminution which takes place from the moment of birth onward in the relative superabundance of the male over the female mortality. The principal and strongest objection that may be urged against the table is the small number of data which is made in it the basis of such interesting statistical deductions. I have given it, however, such as it stands, in consequence of the want, so far as I know, of any more numerous or complete series of similar facts bearing upon the relative male and female mortality in the first week of life. Against the table which I have next to bring forward, the same objection cannot be urged, as the individual data are sufficiently ample, and yet the results are exactly the same in their nature, and equally confirmatory of the proposition we are discussing. For the data upon which I have constructed the table, I am indebted to an elaborate extract of deaths at different ages, contained in one of the invaluable reports of the Registrar-General of England. At pp. 144 and 145 of his Fifth Annual Report, the last published, are given, in two separate tables, the numbers, ages, and sexes of all the individuals that died in England and Wales in the year 1841. In that year 134,563 died who had not passed the age of five; namely, 71,595 males and 62,968 females. In the following table I have arranged these data regarding the male and female deaths, and made calculations from them of such a kind as bear upon the objects of our present proposition.

Total number and relative proportion of male and female deaths, during the year 1841, *in England and Wales, within the first five years of life.*

Ages.	Males.	Females.	Proportion of males to females.	Ratio of Excess of male mortality.
0 to 1 month, . .	13,351	9,741	137 to 100	37
1 to 2 months, . .	4,858	3.703	131 to 100	31
2 to 3 months, . .	3,313	2,676	124 to 100	24
3 to 6 months, . .	8,008	6,451	122 to 100	22
6 to 9 months, . .	6,341	5,182	110 to 100	10
9 to 12 months, . .	5,573	5,013	105 to 100	5
1 to 2 years, . .	13,987	13,281	100 to 100	0
*2 to 5 years, . .	16,164	15,941	101 to 100	1

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* In England the proportion of living males and living females does not become equal till about the twentieth year of life. After that period, an excess of the living population is female; before it, a small excess of it is of the male sex, there being originally born 105 males for every 100 females.—Fourth Report, p. 12. Hence, other circumstances being equal, a slightly greater number of males than of females should necessarily be found in the mortality bills in early life, as seen in the last two lines of the table.—See the Register's Fifth Report, p. 23.

A different arrangement of the facts included in the preceding table may probably show still more strikingly the great proportionate male mortality in the period immediately following birth. During the second year of life, that is, from the twelfth to the twenty-fourth month, 13,987 males and 13,281 females died, or nearly an equal number of the two sexes, the excess on the part of the males amounting only to about 700. During the first month of life, that is, from birth up to the twenty-ninth day following it, there died of male infants a number nearly similar to that of those who perished in the whole course of the second year, namely, 13,351. But the corresponding female mortality during the same period was very much less, amounting only to 9741. Whilst the difference, therefore, between the mortality of the two sexes, during the second year, did not, in the numbers given, show an excess of much more than 700 on the side of the males, this excess ran to upwards of 3500 during the single first month of life, or was five times greater in amount. A tabular arrangement of these figures will point out this contrast more plainly.

Deaths in England in 1841.	Of male Children.	Of female Children.	Excess of male mortality.
During first month of life,	13,351	9,741	3610
During second year of life,	13,987	13,281	700

If it were necessary, and our space permitted, it would be easy to adduce additional tables constructed from the Registrar's Returns of other years for England and Wales, and from the reports of the mortality in other countries, showing, in correspondence with our proposition, the two facts—1st, that the male mortality, as compared with the female, is much greater immediately after birth, and 2d, that the male excess of deaths gradually and regularly diminishes from that time onwards, to about the end of the first year of life. The only new and remarkable circumstance that would be proved by these additional tables is, that the rate of the decrease of the male mortality is everywhere nearly the same.

In illustration of this last remark, I shall content myself with giving the male as compared with the female infantile mortality in England during 1841, and in Western Flanders from 1827–30. The Flanders mortality is calculated upon a return of 37,203 deaths for the first five years of life, including 21,198 males and 16,005 females.[1]

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[1] See Quetelet on the Natural History of Man, p. 30. The town and country mortality are given separately by Quetelet. I have added both together, and computed the proportions in the table from their conjoined sums.

Ages.	Male mortality in		Female mortality.
	England.	Flanders.	
0 to 1 month, . .	137	139	100
1 to 2 months, . .	131	128	100
2 to 3 " . .	124	126	100
3 to 6 " . .	122	122	100
*6 to 9 " . .	110	117	100
*9 to 12 " . .	105	104	100
1 to 2 years, . .	100	103	100
2 to 5 " . . .	101	99	100

CAUSES OF THE GREATER MATERNAL AND INFANTILE DANGER AND FATALITY ACCOMPANYING THE BIRTH OF MALE CHILDREN.

On this point, Dr. Clarke, in his excellent "Observations on some Causes of the Excess of the Mortality of Males above that of Females," published in the Philosophical Transactions for 1786, justly remarks that it may be safely asserted, "anatomy has not hitherto detected any internal difference between the animal economy of the male and female, which can be supposed to account for their difference of mortality, more especially in early infancy."[1] The same author then suggests two sets of causes in explanation of the higher mortality among male children, namely, *first*, their larger size and consequent more difficult delivery; and, *secondly*, their greater liability during their intra-uterine life to disease and debility, from their requiring in consequence of their size, more actual nourishment from the mother than smaller female children stand in need of. We shall discuss this latter opinion before attempting to show the greater truth and correctness of the former one.

"As the stamina of the male are naturally," says Dr. Clarke, "constituted to grow of a greater size, a greater supply of nourishment in utero will be necessary to his growth than to that of the female. Defects in this particular (nourishment), proceeding from delicacy of constitution or diseases of the mother, must, of course, be more injurious to the male sex."[2]

"It appears beyond doubt," says Quetelet, "that there is a particular cause of mortality which attacks male children by preference, before and immediately after their birth." "It will be interesting," he further observes, "to investigate the causes of a circumstance which is so unfavorable to the male sex." "If," he continues, "we were desirous of guessing at this point, we might say with those who suppose that a male conception requires a certain excess of energy in the woman, that this excess of energy was absent or wanting during the growth of the fœtus, and, that energy failing, the child would suffer more from it if a boy than if a girl. Hence the disproportion of dead births between the sexes," &c.[1]

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* The dates of the two original returns, from the 6th to the 12th month, do not precisely correspond. In the English tables the returns are from the 6th to the 9th month, and from the 9th to the 12th; in those of Flanders the corresponding returns here calculated upon are from the 6th to the 8th month (not the 9th), from the 8th to the 12th. The 8th month is not given separately in the published returns, for if so, we should have been enabled to equalize the two exactly.

[1] Philosophical Transactions, vol. lxxvi. p. 352.

[2] Ibid. p. 353.

In a review of Dr. Collins' Treatise in the Dublin Medical Journal, Dr. Graves makes several remarks on the same subject. After showing that Quetelet and Caspar had found respectively in Flanders and Prussia the number of still-born males to still-born females to be 140 to 100, and that in Paris the ratio was 122 to 100, Dr. Graves adds, "In Dublin, Dr. Collins' numbers give the proportion very nearly the same as that of Paris, so that here we have the additional fact corroborative of those already brought forward, to prove that a *greater* mortality before birth prevails among males than females—a most curious result, well calculated to puzzle both physiologists and philosophers."[2]

I shall not attempt to solve, as Clarke and Quetelet have done, the problem broached by Dr. Graves, but will content myself with showing, that the observations on which it is founded are in themselves erroneous, and that the tables of Dr. Collins, appealed to by Dr. Graves, prove, when studied minutely, that, contrary to the general opinion entertained on this point, a greater mortality does *not* prevail before birth among male than female infants. With this view we bring forward the following as our

EIGHTH PROPOSITION.

Of the children that die in utero, and before the commencement of labor, as large a proportion are female as male.

I have already had occasion to state, that, among the 1121 still-born children so often already referred to as occurring during Dr. Collins' mastership, 527 were putrid, and hence had perished before the supervention of parturition.

If we contrast together the proportionate number of males and females among these 527 children, as well as the sexes of the infants among them that were born at the full time, but putrid, and the sexes of all those that were still-born prematurely, whether putrid or not, we shall find the result to be as follows:

[1] Treatise on Man, English edition, pp. 25 and 30.

[2] Dublin Journal of Medical Science, vol. viii. pp. 518–19.

State of the Children.	Total cases.	Male.	Female.	Proportion of Male to Female.
No. of still-born putrid children,	527	257	270	95 to 100
No. of still-born at full time and putrid, ...	296	148	148	100 to 100
No. of premature still-births, .	293	146	147	100 to 100

This table seems particularly valuable and instructive in one respect. It demonstrates satisfactorily, that the intra-uterine morbific agencies, whatever they may be, which act fatally on the fœtus before birth, act equally on the female as on the male child; and that it is to other agencies than these that we are to look for the remarkable proportion of male over female deaths which is observable among still births.

In premature births the comparative compressibility and flexibility of the bones of the cranium is so decidedly greater as to offer no such marked difference between the size and resistance of the male and female head, as we shall immediately see to exist at the full time. Now the last column of the table shows, that, in this class of still-births, the premature, the number of dead-born male and female children was exactly equal. The second column testifies, that the same fact holds true of children born at the full time, and in a state of putridity. Further, in looking to the evidence afforded by the first column, we may there observe, that of *all* the children that had perished from intra-uterine causes, and before the commencement of labor (as demonstrated by their putrid state), the females were even more numerous than the males.

The whole series of facts proves, contrary to what is generally alleged, that the proportion of male children that die *before* birth is not greater than the proportion of females. Indeed, if we deducted the usual 6 per cent. for the normal over-proportion of males, the ratio of girls dying before parturition would be found to be greater than that of boys—a conclusion which the first column would seem to go far to corroborate and strengthen, for in that computation the number of the dead females distinctly and considerably exceeds that of the dead males.

This result with regard to the equality of the sexes among putrid and premature still-born children, becomes only the more striking when we couple and contrast it with the fact which we have already brought out, that of the children who are still-born, and *not* putrid, as many as two out of every three are boys. In other words, among the infants that die *before* labor, the females are equal, if not greater in number than the males. Among the infants that die *during* labor the males are raised to the high proportion of 150 boys for every 100 girls. For this sudden and surprising increase during labor, of the male infantile mortality over the female, there must be some cause or causes traceable in the conditions of the process of parturition. Let us now see, in accordance with what we have already proposed, whether the cause or causes in question be not referable to the greater size of the male infant, and the effects of this size upon the compression of the cranium and its contents. And in order to understand more thoroughly the investigation, let us inquire, in the first place, into the actual average difference between the volume and weight of the male and female at birth.

RELATIVE WEIGHT AND SIZE OF THE MALE AND FEMALE AT BIRTH.

In the Dublin Hospital, Dr. Clarke long ago endeavored to discover the relative weights of the new-born male and female child, by observations made upon 60 children of each sex. In his paper in the Philosophical Transactions of 1786, to which we have repeatedly referred, he gives both the absolute and average weights of these 60 male and 60 female infants.

The 60 males weighed in all 442 lbs.[1]

The 60 females weighed in all 404½ lbs.

The average weight of the male was 7 lbs. 5 oz. 2 dr.

The average weight of the female was 6 lbs. 11 oz. 2 dr.

The average difference between the weight of the male and female child, as calculated from these 120 instances, thus amounted to about nine ounces.

In the Edinburgh Lying-in Hospital 50 male and 50 female children, born during the latter months of 1842 and the earlier part of 1843, were weighed by my friend and assistant, Dr. Johnstone.

The 50 males weighed in all 383 lbs. 11 oz. 4 dr.

The 50 females weighed in all 342 lbs. 12 oz. 4 dr.

The average weight of the male was 7 lbs. 9 oz. 1 dr.

The average weight of the female was 6 lbs. 12 oz.

The difference between the weight of the male and female child, as calculated from these 100 cases, thus amounted to about ten ounces on the average.

The respective lengths of these 50 male and 50 female children were also carefully ascertained in the Edinburgh Hospital.

The total length of the 50 males was 1020½ inches.

The total length of the 50 females was 990½ inches.

The average length of the male child was 20 inches 5 lines.

The average length of the female child was 19 inches 10 lines.

The average difference between the length of the 50 males and

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[1] The Troy or Apothecaries' weight is there used.

50 females thus amounted to 7 lines, or somewhat upwards of half an inch.

These results are sufficient to prove, that at birth, as at other subsequent periods of life, the male is usually of a greater weight and size than the female. The general volume, however, of the *body* of the child is not, in relation to the mechanism of parturition, a matter of such immediate importance as the size of the *head* itself, the facility or difficulty of the process being principally dependent upon the relative size of the latter. The greater weight and volume of the male than the female child at birth might, a priori, entitle us to calculate that the head of the male would, in correspondence with the other parts of the body, be larger than the head of the female. In such an inquiry, however, as the present, it is better to refer to direct arithmetical facts than indirect though probable inferences, and Dr. Clarke has left us a series of measurements of the heads of male and female children, at birth, that are valuable in giving us more precise ideas upon this point. His observations were made upon the 120 male and female children whose respective weights we have already detailed. "For measuring their heads, I made use," he observes, "of a piece of painted or varnished linen tape, divided into inches, halves, and quarters. I took *first* the greatest circumference of the head, from the most prominent part of the occiput, around the frontal sinuses; and, secondly, the transverse dimensions, from the superior and anterior part of one ear across the fontanelle to a similar part of the other ear. These data appeared to me the most likely to afford data for determining the respective sizes of the brain in the different sexes."[1]

The result of Dr. Clarke's measurements may be exhibited in the following manner.[2]

	Absolute dimensions in		Average dimensions in			
	60 males.	60 females.	60 males.		60 females.	
	Inches.	Inches.	Inches.	Lines.	Inches.	Lines.
Circumference of head, ...	839	817	13	11⅘	13	7⅖
Dimensions from ear to ear, . .	445¾	433¼	7	5⅐	7	2¾

The differences brought out in the preceding table, between the male and female head, may appear more precise if we reduce them to decimal figures.

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[1] Phil. Trans. lxxvi. p. 358.

[2] Since these remarks were sent to press, I have incidentally met with the following remark in Dr. Forbes's Quarterly Medical Review, vol. x. p. 492. "M. Nevermann gives us the results of the measurements of the heads of 384 children by Professor Thulstrup of Christiana, which fully bear out Dr. Clarke's statements."

	Average circumference of head.	Average dimensions from ear to ear.
	Inches.	Inches.
In male child,	13·983	7·429
In female child,	13·617	7·221
Difference,	0·366	0·208

According to these observations upon new-born children, it would appear that—

1. The head of the male infant, when measured across from ear to ear, over the fontanelle, is about $2\frac{5}{7}$ lines, or nearly two-eighths of an inch greater than that of the female.

2. In circumference, the head of the male is $4\frac{2}{5}$ lines, or almost precisely three-eighths of an inch greater than that of the female. Hence—

3. The *transverse* diameter of the male head is nearly one-eighth of an inch greater than the transverse diameter of the head of the female child.[1]

The preceding difference between the absolute size and weight of the male and of the female child at birth, and between the relative dimensions of the male and female infantile head, may appear to some to be on the whole so slight, as not to afford in themselves any sufficient explanation of the differences that we have seen to exist, between the dangers and mortality incident respectively to male and female births. The general greater size of the head and body of the boy at birth may not seem adequately to account, by any influence traceable to it alone, for the general greater peril and fatality accompanying the birth of male as compared with female children. The alleged cause may appear unequal to the production of the alleged effects. Let us state, therefore, the reasons that induce us to hold the contrary opinion, and which impress us with the belief that the difference between the size of the male and female head at birth, inconsiderable as it may seem, is the true cause of that greater danger and fatality to mother and child, which accompanies the birth of male infants.

Before doing so it may be proper to prêmise, that under our fifth and eighth propositions I have already shown, on the one hand, that the explanation of the cause of the greater number of male than female still-born infants proposed by Clarke, Quetelet, &c., as depending upon intra-uterine agencies, is totally incorrect, and disproved by statistical facts; and, on the other hand, we have seen that the increased mortality of children of the one sex over those of the other at the time of birth is a result of some circumstance or circumstances connected with, or at least only in operation during, the process of labor. Whilst this holds true as regards the child, it is of consequence, at the same time, to recollect that the complications and dangers on the part of the mother, which we have found so much more frequently attendant upon male than female births, are all of such a nature as to be confessedly the direct or indirect consequences of causes acting during parturition. Further, it will, we imagine, be granted by every accoucheur, that whatever circumstance or circumstances may, by operating as a cause of obstruction, lead to the greater danger and fatality of the male infant during the progress of labor, will, at the same time, in all probability, equally explain that greater peril and fatality to the mother during the process, which is incident to the birth of males.

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[1] Calculating upon the accuracy of Dr. Clarke's linear measurements of the fœtal head, it would appear that the *surface* of the cranium of the male infant, *above* the circumferential line of measurement, is about 27·8 square inches; that of the female about 26·3 square inches. The arch of the male cranium, at birth, is therefore, superficially, upwards of one square inch greater than that of the female. To state it in other words, the proportion of the surface of the head of the male new-born child to that of the female is nearly as 19 to 18—or the surface of the head of the female is one-nineteenth part less than that of the male.

For the solution of the present problem, both as regards the fate of the mother and of the infant, we believe that we ought to look alone to the greater size of the head of the male infant, and that for a variety of reasons which we shall now endeavor to state *seriatim*, adding such illustrative remarks and evidence as they may seem to require, and throwing the more important deductions that may seem to flow from the statistical proofs brought forward, into the shape of additional propositions.

REASONS FOR CONSIDERING THE GREATER SIZE OF THE HEAD OF THE MALE CHILD AS THE CAUSE OF THE GREATER NUMBER OF COMPLICATIONS AND CASUALTIES ACCOMPANYING MALE BIRTHS.

We shall begin our enumeration of these reasons by stating that—

FIRST, *For the very marked differences existing between the difficulties and perils of male as compared with female births, there is no other traceable cause in the mechanism of parturition than the larger size of the head of the male child.*

Parturition, at the completed term of pregnancy, is a process consisting of a combination of mechanism, intended for the extrusion of the full-grown infant from the cavity of the uterus. For the perfect action of this mechanism, three sets of physical conditions are essentially necessary. *First*, A certain degree of mechanical expulsive power is required, and this is supplied principally by the vital contractions of the uterus. *Secondly*, An adequate degree of capacity and dilatation is necessary on the part of those maternal passages through which the child is to be expelled. *Thirdly*, The body which is to be expelled (viz., the infant) must be of such size, and be placed in such a position or positions, as to allow of its sufficiently easy entrance and progress through these maternal passages. A perfect and equitable adjustment between these three different conditions is necessary to the constitution of natural labor; and on the other hand, a deviation—absolute or relative—in any one or more of these conditions, leads on to tedious and difficult parturition, and its results. In which of these three essential conditions could a deviation be possibly effected by the sex of the child, so as to account for the greater dangers and complications attendant upon male births?

The mere sex of the child, and the circumstance whether it was a male or female, could evidently produce no primary effect upon the vital muscular contractions of the uterus, so as to alter them in any morbid manner. Consequently we may exclude from our consideration the first set of physical conditions that we have above named.

We may do equally the same with the second, because it is impossible to conceive that the measurements of the pelvis and maternal passages could be directly altered in any way by the child which has to pass through them being male rather than female.

In searching for a possible cause of deviation in the mechanism of parturition, explanatory of the facts which we have traced, we are thus, by a method of exclusion, compelled to look to the third set of physical conditions of labor, viz., the position and size of the infant.

The consideration of the position of the infant will afford us no clue to the problem, because in most of the cases from which we have drawn our data, the presentation and position were natural. When it was otherwise, the deviation was as frequently on the part of the female as of the male child.

The only item, therefore, that is left in which we may trace any distinction whatever between the mechanism of labor in male and in female births is the relatively greater size of the infant of the male sex, and this relatively greater size could only influence labor or its consequences in so far as it affected the dimensions of the *head* of the child—the increased volume of its body never, unless when excessive in degree, retarding or complicating, to any extent, the progress of the labor in cases of cephalic presentations. The difference between the male and female infant's head is indeed slight, but we are forced, at this stage of the inquiry, to believe it to be the probable origin of the greater dangers and more numerous complications accompanying male births, simply in consequence of our being utterly unable to trace any other appreciable difference, or imagine any other possible circumstance, in the conditions of parturition, which could afford the most distant explanation of the series of phenomena that we are discussing. It is the only circumstance which we can detect as an apparent and constant antecedent to the consequences we have described; and hence seems to be that particular condition which we ought to look upon as the cause of these effects.

In the foregoing remarks we have arrived at the conclusion that has just been stated by a kind of reasoning by exclusion. The argument we have been considering is in a great measure negative. But others of a more positive character are not wanting in confirmation of the same view. For—

SECONDLY, *An increase in the effects may be shown to be connected with an increase in the alleged cause.*

"When," observes Dr. Hamilton, "the head of the infant is pushed foremost, and the labor is not completed within twenty-four hours from its actual commencement, the case is styled laborious, and it may terminate in one of three ways. Thus, the natural powers may at last complete the delivery; or, though these fail, it may be possible for the practitioner to complete the delivery by mechanical means, with safety both to mother and child; or it may be impossible to draw the infant alive through the natural passages. These *three* several terminations constitute *three different orders of laborious labors.*"[1]

Now, if it were granted us, for the sake of argument alone, that the greater obstacles and perils attendant upon male births are attributable to the larger size of the male infant's head, it would almost necessarily follow, that just in proportion as the difficulties connected with the above three different orders of laborious labor progressively increased over one another in intensity, so the amount of male children connected with these labors would, in like manner, probably progressively increase in number.

Let us interrogate the data to be found in Dr. Collins' reports, in order to test practically the truth of an opinion which is so far merely theoretical.

Under the second proposition, we have already had occasion to state that, during Dr. Collins' mastership in the Dublin Lying-in Hospital, among the 16,414 labors which he has reported, the sexes of the infants born were in the proportion of 106 males to 100 females.

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[1] Outlines, p. 45. "It is," says Dr. Churchill, "peculiar to midwifery operations (and the same remark applies equally to the different orders of laborious labors), that they form an ascending series, increasing in gravity from the simplest to the most severe—no two being equal."—Researches in Operative Midwifery, p. 12.

Out of 109 tedious and difficult labors, in which Dr. Collins has recorded the sex of the infant, and that occurred among these 16,414 deliveries, 65 of the children were male and 44 female.

In 24 forceps cases met with by Dr. Collins, the proportion of male to female children was still greater, the males being 16 and the females 8 in number.

The ratio of male infants mounts yet higher, when we turn to the cases in which the labor was so difficult as to require craniotomy. Among 74 instances in which the crotchet was used by Dr. Collins, and where the sex of the child is mentioned, the infant was male in 50 instances, and female in 24.

A tabular arrangement of the above results will show the proportion of male to female children under these different classes of head presentations.

Nature of the Labor and Complication.	Total Cases.	No. of Male Children	No. of Female Children.	Proportion of Males to Females.	Ratio of Excess of Males.
Labors generally, .	16,654	8548	8069	106 to 100	6
Tedious labors, . .	109	65	44	148 " 100	48
Forceps cases, . .	24	16	8	200 " 100	100
Crotchet cases, . .	74	50	24	208 " 100	108

The preceding data prominently show, that as the classes of labors increase in severity and difficulty, the proportion of male to female children in them increases in a corresponding degree. Evidence of exactly the same kind, in support of this opinion, may be gathered from a tabular extract of the cases that occurred in the Dublin Hospital during the mastership of Dr. Clarke (viz., from 1787 to 1793), published by that gentleman in the first volume of the Transactions of the King's and Queen's College of Physicians of Ireland, p. 400. In this abstract Dr. Clarke states the sex of those infants that were still-born in the three classes of 1. ordinary, 2. tedious, and 3. laborious labors.

During Dr. Clarke's mastership there occurred 10,199 cases of single or uniparous births. Among these, 340 children were still-born though the labor was natural, that is, was terminated within 24 hours, and with the head of the fœtus presenting. Of these 340 still-born children, 170 were male, and 170 female. The ratio of the sexes shows this perfect equality in consequence probably of this list including principally putrid and premature children, among whom we have already found (see the eighth proposition) the number of still-born boys and girls to be nearly alike, and the over-proportion, if any, to be rather on the side of the females.

Under the term "Tedious Natural Labors," Dr. Clarke includes those which exceeded 24 hours in duration, but where there was no such disproportion between the head of the fœtus and the mother's pelvis, as to render *destructive* instruments necessary. This class of labors is produced either, he observes,[1] "by causes weakening the expelling powers of the mother, or increasing resistance to the passage of the fœtus." In Dr. Clarke's report, 134 cases are referred to this division, including 16 forceps cases. In these 134 labors, the child was still-born in 41 cases. Among these 41 still-born children, 26 were male and 15 female.

Dr. Clarke further reports the sex of the child in 48 cases of what he terms "Laborious Natural Labors," and where, to use his own words, "the disproportion between the head of the fœtus and the pelvis was so great that it became necessary to diminish the bulk of the former to save the life of the latter." Among these 48 crotchet cases, 32 of the children belonged to the male, and 16 to the female sex.

Of the 429 children that were thus still-born, in these different classes of natural or head presentations in Dr. Clarke's practice, the proportion of the sexes was therefore in the following ratio:

Nature of the Labor and Complication.	Total No. of still-born Children.	Of these were Male.	Of these were Female.	Proportion of Males to Females.	Ratio of Excess of Males.
Ordinary Natural labor, . .	340	170	170	100 to 100	0
Tedious Natural labor, . .	41	26	15	173 " 100	73
Laborious Natural labor (Crotchet cases), . .	48	32	16	200 " 100	100

The preceding data, from Dr. Collins' and Dr. Clarke's returns, will be probably admitted to be sufficient to prove the point that we have in view, and to warrant the following inference as a

NINTH PROPOSITION.

In "laborious labors," with the head presenting, in proportion as the order of labor rises in difficulty, the amount of male births in them rises in number.

To the proof that I have just given of the truth of this proposition, from the reports of Drs. Collins and Clarke, it gives me much satisfaction to add the following general confirmatory statement, contained in an analysis of Riecke's elaborate obstetric statistics of the kingdom of Wurtemberg, published in the Archives Générales.[2]

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[1] Dr. Clarke's Paper, p. 371.

[2] Archives Générales de Médecine, tom. xx. p. 76.

As one of the results of these statistics (the most extensive hitherto published in midwifery),[1] it is stated that "the proportion of boys to girls is much greater in 'artificial' than in ordinary labors, for it includes 7 boys for every 5 girls (or 140 male children for every 100 female). There is the same fact observable among the sexes of those children that are the products of artificial births, and who are either dead-born or die shortly afterwards, for among them the proportion of boys to girls is as 8 to 5 (or as 160 male to every 100 female infants). Often enough," it is added, "it happens that the same woman cannot, without aid, be delivered of a boy, who, at all her other confinements, when the child was female, required no assistance."[2]

THIRDLY, *A diminution of the cause leads to a diminution of the effects ascribed to it.*

We have alleged the cause of the increased casualties connected with male births to be the greater size of the head of the male than of the female infant.

In premature infants there does not exist so great a difference, as in those born at the full time, between the size of the head of the male and female child. The fœtuses of the two sexes approach one another more and more in physical and other characters, the earlier the period at which we compare them. Besides, the cranium of the premature child is so compressible, from its deficiency of ossification, as to be much more easily reduced in size under the pressure to which it is naturally subjected in the process of parturition; and hence the slighter difference which may actually exist between the dimensions of the heads of male and female premature infants is still more diminished in its effects and operation during the course of labor.

Possessing such physical conditions, the birth of premature children should, from the diminished size and diminished resistance offered by their heads, give rise to fewer of those casualties in labor that we have attempted to trace to the influence arising from the greater size of the head of the full-grown male child.

I have no evidence to bring forward with a view of showing the influence of the birth of premature children or their sex, on the difficulties of the labor in reference to the safety and life of the mother, because the labors with them are seldom or never so difficult as to induce any marked maternal complications. But we may derive equally good evidence in illustration of the point we are discussing by studying the effects of the labors with premature children —not upon the *mothers* but upon the *infants* themselves. With this view I shall collect, from the data contained in Dr. Collins' report, the number of male and female children expelled *prematurely* and not putrid, in order to contrast them with the proportion of males and females among still-born children expelled at the *full time* and not putrid. Of the former there were born in all 62, of the latter 532. The ratio of the sexes in each series stands thus:—

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[1] They include 219,353 deliveries which occurred in Wurtemberg, during the four years, 1821–25. The artificial deliveries (*accouchemens artificielles*), or those requiring some special aid or interference, amounted to 7949. In these 7949 cases, 630 of the mothers and 3754 of the children were lost. The generalization in the text seems to be founded upon the sex of the infants in these 7949 labors, and 3754 still-births.

[2] Ibid. p. 88.

Non-putrid Children still-born.	Total.	Males.	Females.	Proportion of Males to Females
At full time, .	532	320	212	151 to 100
Prematurely, .	62	34	28	121 to 100

The comparison of the two columns in the above table shows that, among the children who died immediately before or during labor, and who had reached the ninth month of utero-gestation, the proportion of males to females lost was considerably greater than among still-born children born prematurely—and, as we believe, for this reason, that in the latter (the premature still-births), there is not, as we have pointed out, so great a distinction between the male and female heads as among children at the full time. The comparison of the proportion of males to females who died within ten days after delivery among, 1st, children born at the full time, and 2d, those born prematurely, leads to the same inference, viz., that as in the latter the alleged cause of distinction between the relative size of the male and female head is diminished, the alleged effect upon the differences displayed between the mortality of the two sexes is also proportionally lessened. The following table is intended to illustrate this point. The calculations are from the data in Dr. Collins' chapter on children dying in the hospital.

Children dying within 10 days after birth.	Total.	Males.	Females.	Proportion of Males to Females.
Born at full time,	171	106	65	163 to 100
Born prematurely,	102	56	46	121 to 100

The remarks that we have made regarding the heads of premature children should apply equally to those of twins. Twin children, like those born prematurely, are in general below the standard size and weight, and, in the same manner, have their heads less ossified, and hence more compressible than those of single infants at the full time. There is, consequently, also a less marked difference between the relative size of the male and female head, among twin children, than among single children at the full time; and for the same reason, if our views are true, there should be a similar less marked difference between the mortality of the two sexes among twins, at birth and for some time after it. That such is in reality the case I shall allow Dr. Clarke to state in his own words. Speaking of the relative male and female infantile mortality in the Dublin Hospital, he observes,[1] "it is worthy of observation that though *double* the number of twins die and are still-born, compared to single children, yet the proportion of male twins lost to females is *less*. Only one-fifth more of the male sex die than of the female, and only one-third more is still-born. Whereas of single children, whose proportional mortality is one-half less, *one-fourth* more of the male sex die, and near double the number is still-born. To what, then, are we to attribute this lessened mortality in favor of male twins? Probably to their brain and nervous system suffering less during delivery, on account of their heads being much smaller than those of single children."[2] In other words, the cause of the higher mortality, or the relative size of the male over the female head, being diminished, the effects which we attribute to it, viz., the higher infantile mortality among male children, is also proportionately diminished.

FOURTHLY, *In those morbid complications in labor in which the cause is in abeyance, the effect is also absent.*

There are some morbid complications during labor which are independent of the presence, and hence of the sex or size of the infant. I allude especially to those complications which occasionally occur during the third stage of labor, and consequently after the total expulsion of the child. It would afford no small corroboration of the preceding remarks and inferences, if in these cases, where all agency from the conditions of the infant was for the time being excluded, the morbid complications which may take place were found to occur as frequently in connection with the birth of female as of male infants. And such, we believe, may be shown to be the fact.

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[1] Philosophical Transactions, vol. lxxvi. p. 354.

[2] In the Dublin Hospital, from the years 1757 to 1784, it appears from Dr. Clarke's tables that among the twin-births, 29 males and 20 females were still-born, and 116 male and 91 female children died within a fortnight after delivery; on the other hand, among the uniparous or single births 602 males and 351 females were still-born; and 1656 males and 1247 female children died within a fortnight after birth. These data form the ground of the inferences drawn by Dr. Clarke in the passage quoted in the text.

The two principal complications that occur during the third stage of labor are morbid retention of the placenta and hemorrhage.

Dr. Collins reports 70 cases of retention of the placenta, 35 of them after male, and 35 after female births.

Hemorrhage took place during the third stage of labor, or between the birth of the child and placenta, in 71 cases during Dr. Collins' mastership. In 36 of these 71 labors the child was of the male, and in 35 of the female sex.

These observations, when thrown into a tabular form, would consequently stand thus:

Nature of complication.	Total cases.	With male children.	With female children.	Proportion of males to females.
Retention of placenta,	70	35	35	100 to 100
Hemorrhage during third stage,	71	36	35	100 to 100

The evidence afforded by the above table is probably sufficient to entitle us to add to those inferences that we have already laid down, the following deduction as a

TENTH PROPOSITION.

Of the morbid accidents that are liable to happen in connection with the third stage of labor, as many take place with female as with male births.

To prove that the slight increase of size of the male over the female infant's head is the true antecedent or cause of the greater number of casualties accompanying male births, we may so far change the ground of direct proof which we have been hitherto attempting to pursue, and proceed to show, as a matter of strong though indirect evidence, that—

FIFTHLY, *Similar effects upon the mother and child are produced by other causes similar in their character and amount.*

If the larger dimensions of the head of the male infant be the immediate cause of the larger number of accidents and deaths attendant upon male births, its greater degree of *size* could only possibly lead to the results in question, by offering a greater proportionate degree of impediment to the passage of the fœtus through the pelvic canals. Its effects, therefore, upon the mother and child should be such as are produced by obstructed labor. That the various consequences which we have previously traced are all of that description, it is unnecessary for us to point out to the obstetric pathologist. It might be considered, however, as confirmatory of this fact, and, at the same time, as the strongest correlative evidence that could be adduced of the power of *slight* obstructions, during labor, to lead to these effects, if we could prove that in any other extended series of cases, in which obstructions of the same amount existed, the same or similar effects resulted both to the mother and child. Our evidence would only be the stronger if, in this class of cases, we could so far reverse the state of matters as to transfer the existing slight obstruction from the body passing to the passages themselves. The circumstances connected with first labors offer the exact conditions which we seek.

In first labors, taken as a class, there is a greater obstruction to the transit of the child than in subsequent deliveries, in consequence of the maternal passages being less dilatable than afterwards. The two causes of obstruction, viz. the increased size of the fœtal head, and the diminished size of the maternal passages, though different in their seat, are nearly analogous in the nature of their influence, and in the amount of that influence.

In male births, the body passing (the head of the infant) is slightly *greater* than in female births, whilst, *cæteris paribus*, the maternal passages are the same. In first labors the capacity or dilatability of the maternal passages is slightly *less* than in subsequent labors, whilst, *cæteris paribus*, the head of the infant remains the same, there being at least an equal number amongst them of males and females. Having stated these premises we shall go on to point out, as another reason for supposing the size of the male head to be the sole cause of the danger of male births, that, in accordance with our proposition, most of the complications and accidents, which have been already described as arising in male, as compared with female births, from the comparatively slight increase in the dimensions of the male head, occur also, in first, as compared with future labors, in consequence of the comparatively slight diminution of the dimensions or dilatability of the passages in these labors.

We shall give the proofs of this as an illustration of another, or

ELEVENTH PROPOSITION.

More dangers and deaths occur both to mothers and children in first than in subsequent labors.

Among the 16,414 women delivered in the Dublin Hospital during Dr. Collins' seven years' mastership, 4987 were confined for the first time. First deliveries occurred, therefore, in the proportion of about 30 in every 100 cases. If, however, in studying the various morbid complications which have been statistically reported by Dr. Collins, we find the proportion of these complications in first labors to be greatly above the ratio of 30 per cent., it would tend to show that there is a stronger liability to their occurrence in first than in future deliveries. To gain some data in this question, let us examine into the facts which may be gathered from Dr. Collins' returns in regard to the proportion of these morbid accidents arising from obstruction during parturition, which were met with in first as compared with subsequent labors.

Convulsions.—Of 30 cases of convulsions described by Dr. Collins, 29 occurred in women who were pregnant for the first time.[1] Thus they were found in connection with first pregnancies in the ratio of 96 in 100.

Crotchet cases.—Of the 79 cases which Dr. Collins mentions as requiring craniotomy, the number of the pregnancy in each can be made out only in 75. Of these, 51 were first labors;[2] or out of every 100 cases in which the crotchet was used, 68 were first deliveries.

Forceps cases.—The forceps were employed in 24 instances. Of these, 18 were first labors.[3] Hence out of every 100 instances in which this instrument was used, the proportion of first labors, according to this average, would be 75.

Tedious labors.—Under our second proposition I have already stated that Dr. Collins has given so far the history of 109 instances of tedious labors.[4] Of the 109 cases, 75 were first confinements;[5] thus giving for every 100 cases a proportion of 69 as pertaining to first deliveries.

Puerperal fever.—Out of 88 women attacked with puerperal fever, 44 were confined for the first time.[6] Thus this complication supervened upon first labors in the ratio of 50 in every 100 cases.

Maternal deaths.—We have already seen, under the first proposition, that of the 16,414 women delivered in the Dublin Hospital during Dr. Collins' charge of it, 164 died. Of these 164 mothers, 86 had given birth to first children;[7] or of every 100 women who died, 53 had been confined for the first time.

Still-births.—Under this head we exclude from our calculation all those children who were born putrid, either prematurely or at the full time. Of 1121 still-births in all, observed by Dr. Collins, 594 were born not putrid. Of these 594 children, 260 were the product of first confinements; or in other words, they were found in connection with first pregnancies in the ratio of 45 to 100.

Infantile deaths during the first eight or ten days after birth.—The mothers and their infants generally, as we have seen, remained for that time in the hospital after delivery. During it 284 of the children died. Of these, 170 were single children born at the full time.[1] Of the 170, 75 were the offspring of primiparæ; or infantile deaths, during the first ten days, among single children born at the full time, occurred in connection with first pregnancies in the ratio of 44 in every 100.

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[1] Practical Treatise, p. 201.
[2] Ibid. Table, p. 490, et seq.
[3] Ibid. p. 15.
[4] Ibid. p. 462, et seq.
[5] Ibid. Table, p. 490, et seq.
[6] Ibid. p. 284
[7] Ibid. p. 363.

The preceding series of facts, if arranged together into a general tabular form, would stand as follows. In reading these arranged results, it is to be recollected that the *standard proportion of first pregnancies, in the general sum of the labors, amounts to* 30 *per cent.*; whilst, as shown by the table, the proportion was much higher in the various complications under-mentioned.

Complication.	Total Cases.	1st Pregnancies.	Proportion of 1st pregnancies.
Convulsions, ...	30	29	96 in 100
Forceps cases, ...	24	18	75 in 100
Crotchet cases, ...	75	51	68 in 100
Tedious labors, ...	109	75	69 in 100
Puerperal fever, ...	88	44	50 in 100
Maternal deaths, . . .	164	86	53 in 100
Still births, not putrid, .	594	260	45 in 100
Infantile deaths during 1st 10 days; born single and at full time, . . .	170	75	44 in 100

The results which the above table is intended to display might perhaps be brought out more strongly by another arrangement, if our data for it were more complete. Dr. Collins mentions the number out of the 16,414 patients that were delivered for the first time, viz., 4987; of these 72 were twin births. Unfortunately, however, for our present purpose, he has omitted to give the exact numbers of those that were confined in their second, third, and other subsequent labors. But an approximation, sufficiently near for the purpose we have in view, may perhaps be made without any probability of a very serious error. To have the required standard of comparison, I shall take it for granted, for reasons which it is unnecessary to dwell upon, that the percentage of first and future labors in the Dublin Hospital was nearly as follows: Of all the cases, about 30 per cent. were first pregnancies; 22 per cent. second pregnancies; 15 per cent. third pregnancies; 11 per cent. fourth pregnancies; 8 per cent. fifth pregnancies; 5 per cent. 6th pregnancies; 3 per cent. seventh pregnancies; 2 per cent. eighth pregnancies; 1 per cent. ninth pregnancies; and 3 per cent. belong to women who were confined for the tenth time or upwards.[2] If we compare with this given standard the proportion of instances in which the different complications we have been considering occurred in different pregnancies, we shall see in a marked degree the great over-proportion of them in connection with first deliveries. I have constructed the following table with this view.

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[1] Practical Treatise, Analysis of Table, p. 519, et seq.

[2] This calculation of percentages proceeds on the assumed probability that, of the 16,414 women delivered in the Dublin Hospital under Dr. Collins, 4926 were cases of first labor; 3611 were of second labors; 2462 third labors; 1806 fourth labors; 1313 fifth labors; 826

Table showing the percentage of cases of natural and morbid labors belonging to different pregnancies.

Percentage of Pregnancies.	1st	2d	3d	4th	5th	6th	7th	8th	9th	10th and subseq.
Total No. delivered in Hosp.	30	22	15	11	8	5	3	2	1	3
Convulsions,	96	4	0	0	0	0	0	0	0	0
Tedious labors,	69	9	7	3	2	3	2	1	3	1
Forceps cases,	75	?	?	?	?	?	?	?	?	?
Crotchet cases,	68	10	7	4	0	3	?	0	2	4
Puerperal fever,	50	19	10	7	8	0	2	4	0	0
Maternal deaths,	53	16	7	7	5	4	1	3	1	3
Still-births,	45	17	11	8	5	5	4	3	4	8
Infantile deaths for 10 days after birth,	44	15	11	8	8	7	2	3	1	2
Post-partum hemorrhage,	33	16	10	9	9	5	4	6	3	6
Rupture of uterus,	21	18	18	6	6	15	6	3	3	10

In regard to the two complications which we have placed at the bottom of the above table, viz., post-partum hemorrhage and rupture of the uterus, it will be observed that our present proposition does not hold out so strictly with regard to them, and that they do occur comparatively more frequently in first than in subsequent labors. But this does not detract in any way from the previous view which we took of them, as occasional results of the partial obstruction offered by the male head. For there are sufficient reasons why they should occur more rarely in first labors, though these labors are more obstructed than those that occur subsequently. In regard to rupture of the uterus, for example, it is now well known to obstetricians that two causes may give rise to its occurrence, namely, *first*, over-action of the uterus, in consequence of impediment to the passage of the child;[1] and, *secondly*, any such diseased condition of a portion of the uterine parietes as renders that portion less resistant, and hence more easily lacerated, than the

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sixth labors; 495 seventh labors; 328 eighth labors; 164 ninth labors; and 491 were instances in which the women were delivered for a tenth or later time.

[1] "There is one fact," observes Dr. Collins, "which clearly shows disproportion to be a frequent cause, namely, its being oftener met with in the expulsion of male children. Thus, of thirty-four cases which I am about to state, twenty-three of the children were males, &c. This is satisfactorily accounted for by the greater size of the male head, as proved by accurate measurements made by Dr. Joseph Clarke."—Chapter on Rupture of the Uterus and Vagina, in Practical Treatise, p. 242.

other parts of the uterine walls. The first of these two causes is in much stronger operation in male than in female births, and in first than in future labors; but the second cause comes more and more into action with the frequency of the previous parturitions, the uterine tissues being more and more liable to disease under the strain of each successive pregnancy and labor. Hence, though lacerations from obstruction are more common in first confinements, the same accident, as a result of disease of the uterine structures, is far more frequent in subsequent and later parturitions, so much so indeed, as to cancel any argument that might be derived in favor of its origin from mere obstruction, by the study of it in primiparous mothers.

Again, post-partum hemorrhage, though more apt, as we have already stated, to occur in cases in which the uterine action has been protracted, and the contractile powers of the organ morbidly exhausted, is not so liable to appear after tedious first labors as after tedious subsequent labors, because it is a well-known and acknowledged fact, that altogether the uterus contracts more perfectly and securely after first labors than after others. Hence the well-known rarity of after-pains subsequently to first parturitions. "A woman," says Dr. Power, "experiences little or no after-pain with her first parturition, because the parietes of the uterus, not having been weakened by previous distension, contract more perfectly and permanently, so as to obliterate and empty the cavity thoroughly."[1]

In reference to the bearing of some of the results included in the above tables upon the question we are discussing, it is proper also to add, that the ratio in which some of the complications occurred in first pregnancies, is greater in degree than can be accounted for upon the doctrine of obstruction, and is partially dependent upon other co-existing causes. I allude more particularly to the case of puerperal convulsions. It is well known that in the practice of all accoucheurs this complication is met with, principally in first, and comparatively rarely in future deliveries.[2] The mere amount

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[1] Treatise on Midwifery, p. 190.

[2] The following table will show this fact better than any lengthened commentary:

Reported by	Total No. of Puerperal Convulsions.	No. of these occurring in first labors.
F. Ramsbotham, .	59	45, or 57 per cent.
J. Ramsbotham, .	22	15, or 68 "
Lee, ...	46	30, or 65 "
Merriman, ...	48	36, or 75 "
Clarke, ...	19	15, or 79 "
Johns, ...	9	8, or 89 "
Collins, ...	30	29, or 96 "
Total,	233	178, or 76 per cent.

of obstruction present in first labors is not the sole explanation of the large percentage of convulsions occurring in first confinements, for impediments in the maternal passages in future deliveries are not followed in any such degree by the same consequence; and, further, obstetric pathologists are now well aware that in almost every case of puerperal convulsions, a predisposition to the affection is given by one or more particular pre-existing morbid states of the system. Various authors, as Demanet,[1] Osiander, Chailly, Johns, &c., have correctly described the most common pre-existing morbid condition as one marked by dropsical effusions in the face and elsewhere. Of late years the pathology of these cases has been advanced a step farther, and the complication of convulsions shown to occur only, or almost only, in women who have such confirmed or temporary renal derangement as is marked by an albuminous state of the urine. Dr. Lever suggests that this albuminous state of the urine, the common state predisposing to puerperal convulsions, is produced by pressure of the enlarged uterus upon the renal veins. If this were the explanation, this complication should occur as frequently in subsequent as in first labors, because the renal veins are not more especially compressed in the one than in the other. But to discuss any such question at present would be wandering from our subject. We have said enough to show that, in regard to puerperal convulsions and their frequency in first labors, we must take other points into consideration than the mere degree of obstruction in order to account for their high over-proportion in first pregnancies.

Before we proceed farther, let us pause for a minute, and simply recapitulate the reasons that we have already given for considering the greater size of the head of the male child as the cause of the greater number of complications and casualties connected with male births. *First*, I have shown that for the very marked differences existing between the difficulties and perils of male, as compared with female births, there is no other traceable cause, in the mechanism of parturition, than the larger size of the head of the male child. We have seen, *Secondly*, that an increase in the morbid effects may be proved to be connected with an increase in this reputed morbific cause; *Thirdly*, that when the alleged cause is diminished, the effects are diminished; and *Fourthly*, that in those morbid complications in labor in which the cause is in abeyance, the effect is also absent. *Lastly*, we have found that similar effects upon both mother and child are produced by other causes which are similar in their character and amount.

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[1] "Regarde l'anasarque comme une de leurs causes *essentielles*."—Recueil Periodique de la Société de Médecine, tom. ix. for 1800–1, p. 110.

Now, if in conducting any investigation into the explanation or mode of production of a number of ascertained and acknowledged facts, we found, amidst the assemblage of phenomena submitted to our study, one supposed antecedent condition or probable proximate cause perfectly answering the various reasons and tests to which, in the preceding pages, we have already subjected the increased size of the male over the female infant's head as the alleged cause of the reputed greater danger and mortality attendant upon male births, the evidence, in support of that circumstance as the *vera causa* of these consequences, would be deemed of a very strong and decided character. The evidence would become still more conclusive if it could be shown, that though this *vera causa* appeared at first sight to be in itself an agent quite inadequate for an explanation of the various important effects traced to it, yet, when studied in its full and proper relations, it could be proved to have necessarily sufficient power and influence for the production of the results. Such we shall now endeavor to prove to be the case with the slightly increased volume of the head of the male over that of the female infant, as the cause of the greater number of the casualties accompanying the birth of male children.

SIXTHLY, *The greater size of the male than of the female infant's head is sufficient in itself to explain the greater dangers attendant upon male than female births, when we consider it in relation to its absolute and cumulative effects.*

"No arguments are required," as Dr. Denman observes, "to prove that a small body will pass through a small space with more facility than one that is large; the size of the body being supposed to bear any reasonable comparison to the dimensions of the space. Of course it may be presumed that the larger the head of the child is at the time of birth, with the greater difficulty it will be expelled." Nor does the well-known compressibility of the human cranium at birth alter this view in regard to the effects which an increased size of the head is calculated to produce. "For though nature has, with admirable wisdom," I quote the words of Dr. Osborne, "by means of sutures and fontanelles, so constructed the head of the human fœtus that, in the passage through the pelvis, it may suffer the form to be altered and the volume to be considerably diminished without such injury to its contents as shall necessarily destroy life; yet as there is a volume beyond which each fœtal head cannot suffer compression with safety—so there is another and still smaller into which it cannot be compressed at all."[1]

No one will be inclined to doubt the fact, that when the fœtal head is *much* increased beyond the common size at birth, its expulsion will be attended with more than the usual degree of difficulty. But exactly the same effect may be produced by the infantile head exceeding the required dimensions in only the most trifling degree, provided it happens, from the particular conformation of the mother, that the maternal pelvis and fœtal cranium are otherwise very nearly and accurately adapted to one another. In such instances a difference in the size of the head, of the smallest amount, may involve and change the whole question of the physical possibility or impossibility of the passage of the infant. Dr. Hamilton, in his "Practical Observations," when speaking of the occasional difficulty of determining in individual cases in practice, whether the maternal passages are or are not of such dimensions as to allow the child to pass, and whether the infant's head may or not require to be diminished by craniotomy, strongly remarks, "that in this question even a miscalculation of the *sixteenth part of an inch* might be fatal to the life of an infant." "The author," he adds in a footnote, "has been accustomed to illustrate this practical remark to his pupils by a very simple mechanical demonstration. He first shows the smallest possible aperture through which the fœtal head of ordinary size can be squeezed, and he then covers the head with a common towel, and proves the utter impossibility of its then passing through the same aperture. He does not believe that the addition of a common towel can increase the diameter of the head more than the sixteenth part of an inch."[1]

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[1] Essays on the Practice of Midwifery, p. 188.

If Dr. Hamilton's calculation regarding the increase of the diameter of the head in this experiment be true, viz., one sixteenth of an inch, then here we have the transit of the fœtal head entirely prevented by an increase of size which is not more than half of that difference (one-eighth of an inch), which we have seen to exist between the diameter of the standard male and the standard female head at birth.

In relation to the question of the greater difficulties attendant upon male births, it ought further to be always held in view, that when the fœtal head is notably above the medium standard, the infant is, in a great majority of instances, of the male sex. Among the 120 infants whose heads were measured by Dr. Clarke, only six were above 14½ inches in circumference. These six were all males. Twenty-nine of the children were as high as 8 lbs. and upwards in weight. Of these 29, as many as 19 were of the male, and 10 only of the female sex. Such facts require no comment.

But let us throw altogether out of consideration the more extreme cases to which we have adverted, and look upon the question of the more numerous dangers attendant upon male births as the result merely of the average slight increase of the standard male over the standard female head at birth. Even under this limited view the normal difference in size between them (about one-eighth of an inch in diameter), inconsiderable as it may at first appear, will, we believe, afford us a perfectly sufficient explanation of the statistical facts which we have brought forward in the preceding pages, provided we consider what the *accumulated* mechanical effects of this difference in measurement would be when operating over a very wide and extended range of cases of parturition. A few remarks will illustrate our meaning. We take for granted, that the act of parturition consists of the dilatation of the female passages, and of the expulsion of the fœtus through them. Before the process can be terminated, a very considerable amount and continuance of expulsive and dilating force is in general required. During it, the parietes of the maternal passages are necessarily subjected to pressure from the advancing fœtal head, and in turn the wedge-like fœtal head itself is compressed by the resisting passages. The whole process is rendered the more tedious and difficult in consequence of the size of the head of the fœtus being nearly proportioned to the size of the passages of the mother. What, then, will be the effects upon this whole process, of such a difference in the dimensions of the fœtal head as holds good with regard to the relative sizes of the male and female at birth, supposing the mother's pelvis and passages to be always, *cæteris paribus*, the same in their capacity? If the female infant's head, when of the standard size, require for its transit a certain extent of dilating and expulsive force—if it necessarily produce, during its egress through the maternal passages, a certain amount of pressure upon their walls in order to overcome the resistance which they offer—and if, in turn, the head of the child be, under the same action, compressed itself to a certain degree;—then the standard male infant's head (as being an eighth of an inch greater in diameter, and consequently demanding more actual space for its transit), will require proportionately a greater exent of dilatation in the maternal passages, and a greater expenditure of force to effect this greater dilatation; it will hence also produce, during its egress through the maternal canals, a greater amount of pressure upon their parietes, and be itself reciprocally compressed to a greater degree. The difficulties and dangers to child and mother entailed by this latter state of matters might well *à priori* be supposed to be exactly those that we have shown them really to be under the various propositions that we have previously laid down.

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[1] Hamilton's Practical Observations, Second Edition, p. 254.

But that the birth of the male, as compared with the female child is accompanied with more obstacles and delay, need not be an inference resting solely, as it does above, upon abstract though satisfactory reasoning—for it is a matter which should admit of direct statistical and arithmetical proof. If our data were sufficiently precise and extensive, we ought to be able to prove, that, as a

TWELFTH PROPOSITION

The average duration of labor is longer with male than with female children.

Dr. Collins has given a table[1] showing the number of his patients that were delivered in "one quarter of an hour," "in one hour," "in two hours," &c., from the commencement of labor. The sexes, however, of the children are not distinguished, so that the data are of no avail in regard to their bearing upon the proposition we have just stated. Nor am I aware that there has been as yet anywhere published such a series of facts as would enable us to prove statistically that the birth of the male child is absolutely longer than that of the female, in the time required for its completion. The average duration of labor in the European female is about four or five hours. The average duration of male births will probably be found, when sufficient evidence is collected, to be about a quarter of an hour or half an hour longer than that of females. I venture to make this allegation, in regard to the common run of labors, upon the evidence afforded by an assemblage of cases, now lying before me, in which the process was somewhat protracted beyond the usual standard. The cases I allude to were collected under the following circumstances. In 1836 a separate register was begun in the Edinburgh Lying-in Hospital of those labors in which the date of the commencement of the process and the comparative length of each successive stage could be ascertained with sufficient accuracy. Those cases in which the labor was very speedy, and indeed all that were less than three or four hours ill, were not entered, because in most of them the duration of the labor and its stages could not be noted with the accuracy required; many of these patients only entering the hospital a few minutes before delivery. About half of the labors in the Dublin Hospital appear to have been terminated within the first two or three hours,[2] so that our Edinburgh register, as wanting that proportion of the more rapid cases, cannot show the average length of the process in ordinary instances. The records, however, which it contains of the cases protracted beyond the period mentioned have, I know, been kept with adequate care,[3] and seem capable of furnishing us with sound and unprejudiced evidence upon the present topic, inasmuch as they were originally noted without any view to such an inquiry as this into the comparative duration of male and female births. In analyzing the register I find, that, from 1836 to 1841 inclusive, 249 male and 178 female births are entered, with notes of the precise length of the labor in each. The following table will show the absolute and average duration of the labors with male and female children in these 427 cases.

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[1] Practical Treatise, p. 22.

[2] Of the 15,850 cases noted by Dr. Collins, 7063 were terminated within two hours, and 9550 were within three hours from their commencement.

[3] A number of the entries were made by myself when annual pupil at the hospital in 1836–7, and have been continued by others in that office.

Labors.	Absolute duration of the whole cases.		Average duration of each labor.	
	Hours.	Minutes.	Hours.	Minutes.
With male children (249 in number),	2646	33	10	38
With female children (178 in number).	1702	29	9	34
Average greater length of the male birth,			1	4

In those classes of labor in which the process becomes morbid from tediousness or other complications and difficulties of a more serious character, the difference between the duration of the male and female births would seem to be even greater than the above table displays. At least such an inference appears deducible from the analysis which I have made, from Dr. Collins' tables, of the comparative length of the male and female births in the records which he has given of 507 still-born children (including the putrid and premature), of 97 morbidly tedious labors, 68 crotchet cases, and of 130 instances in which the mother died in connection with delivery. The preceding numbers include *all* the cases, under these several heads, in which the exact duration of the labor is mentioned in Dr. Collins' tables. The result is this:—

Labors.	The average duration of the male births was		The average duration of the female births was		The average greater length of the male births was	
	Hours.	Minutes.	Hours.	Minutes.	Hours.	Minutes.
With still-births (310 males and 197 females),	14	27	12	10	2	17
Morbidly tedious (56 males and 41 females),	43	18	40	12	3	6
Requiring crotchets (44 males and 24 females),	42	22	38	20	4	2
Leading to death of the mother (86 males and 44 females),	18	9	12	58	5	11

The evidence afforded by the foregoing table appears almost enough to warrant us in qualifying the proposition we have laid down, to this extent, that the "average duration of labor is longer with male than female children, and the difference in this respect between male and female births, becomes increased in length when the labors become more severe and dangerous in their character."

The remarks and proofs that we have now offered may tend to remove the difficulty which the inquirer may at first entertain regarding the supposed inadequatenesss of the slightly increased size of the male head to produce all the greater number of casualties and complications that we have traced to be connected with male births. For it will probably be allowed that the principal, or indeed sole obstacle, which the mind has to contend with, in allowing the very small size of the male over the female infant's head to be the cause of the remarkable differences which we have traced between the results of male and female births, consists in the difficulty of at first supposing so slight an apparent cause to be the agent by which such remarkable consequences are brought about. But in considering this and the analogous questions, it must be held in recollection, that in all processes, whether vital or physical, in which, as in parturition, an established relation of mechanical conditions is required, any disturbance in these conditions, however small and trifling in itself will, ultimately, and when followed out through numerous and extended series of observations, be found to lead to results the magnitude of which could scarcely have been previously surmised. These results may not be appreciable when we confine ourselves to the study of the agency in an individual case, or in a small number of cases merely, but they become more and more marked in proportion as the number of our instances become more and more extensive. The effects cannot be distinctly seen when we look for them in a limited series of data, but they may be evoked with all the force of a mathematical demonstration, when we prosecute our calculations for them among large and accumulated masses of observations. From any study, however minute and accurate, of a limited number of cases of labor, no man would probably feel himself entitled to conclude that male are in any notable degree more difficult and dangerous than female births; but this, as we have seen, becomes a demonstrable and strongly marked fact, when we direct our inquiries after the truth of it into the records of hundreds or thousands of carefully reported observations, such as we have made use of in the course of the preceding inquiry.

The remarks we have hitherto made upon the male sex of the child as a cause of delay, difficulty, and danger during labor, have referred principally to the larger size of the male infant's head or cranium. In that condition, and its effects, we have so far found a satisfactory explanation of the numerous complications and casualties to which the mother herself is subjected in male births. One or two additional observations may be required to show more specifically in what manner the safety and life of the male child during labor, and for some time after it, is endangered by the same circumstance. We believe that the larger size of the contents of the male, than of the female cranium, or, in other words, the larger size of the male brain or encephalon at birth, and the consequent greater compression and injury to which it is subjected during birth, affords us the proper clue to the explanation of these peculiarities in the male infantile mortality.

The weight, and hence the size of the brain, is now well ascertained to be absolutely more in the adult male than in the adult female. From measurements of the interior of numerous crania, Sir William Hamilton[1] computes the male adult encephalon as weighing 3 lbs. 8 oz. Troy, and that of the adult female as 3 lbs. 4 oz. Professor Reid carefully weighed in the Edinburgh Infirmary the encephalon of 53 adult males and 34 adult females. The average weight of the male brain in these observations was 3 lbs. 2 oz. 3¼ drachms; that of the female, 2 lbs. 12 oz. 8½ drachms; and the average difference in favor of the male encephalon thus amounted to 5 oz. 11 drachms.[2] This difference in weight between the encephalic contents in the two sexes exists also at birth. In an elaborate paper on the weights of the brain in the different sexes, &c., by Professor Tiedemann,[3] founded on numerous observations, that distinguished anatomist states that "the female brain weighs on an average 8 ounces less than that of the male, and this difference is," he adds, "already perceptible *in a new-born child.*" In one of his tables Tiedemann adduces the weight of the brains of two male and two female infants at birth. The average weight of the two male brains was 13 oz. 6 drachms 20 grains; the average weight of the two female, 10 oz. 44 grains; and the average excess of size of the encephalon of the male infant, as deducible from these four observations, 3 oz. 5 drachms 36 grains. But this result is probably much higher than a more extended number of data would give, in consequence of one of the female children in the table being evidently below the standard size and weight of the girl at birth.

There is another way by which, in the present deficiency of direct facts bearing upon this point, we may arrive at the same conclusion, with equal or greater certainty. "The weight of the brain," says Tiedemann, "of a new-born child is relatively to the size of the body as one to six." The male infant at birth is, as we have seen, on an average, about nine or ten ounces heavier than the female. If the above ratio between the brain and whole body holds equally true (as there is every reason to believe) of the two sexes, the male brain should, on an average, be one-sixth part of this heavier than the female brain at the time of delivery. Hence the male encephalon is, in the infant at birth, about 1½ ounces heavier than the female encephalon, and the male brain and cerebellum altogether about one-ninth or one-tenth greater in weight, and consequently in size, than the brain and cerebellum of the female.

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[1] Monro's Anatomy of the Brain, 1831, p. 12.

[2] London and Edinburgh Monthly Journal for 1843, p. 322. See also Dr. Sim's paper in the London Medico-Chirurgical Transactions, vol. xix., for similar results.

[3] London Philosophical Transactions for 1836, p. 502.

The immediate effects, during parturition, of this much greater size of the male encephalon upon its own delicate structures are evident. For as, *cœteris paribus*, the pelvic passages are always the same in their dimensions, the larger and heavier male brain will, in its forcible transit through the given standard space of the maternal canal, of necessity suffer more physical compression and injury than the smaller female head and brain.

Connected with the structural economy of the brain of the new-born infant, there are some circumstances which are calculated to render such degrees of injury as we have adverted to, more extensive in their influence, and more permanent in their results, than similar amounts of injury of these same parts at other later periods of life. For, *First*, the brain of the infant at birth is still so soft and almost semifluid, and its vessels so large, as to be more easily lacerated than years afterwards under the same extent of pressure. *Secondly*, the brain at that particular period of life, and for ten or twelve months afterwards, is undergoing great and rapid normal changes from its imperfect embryonic state to the anatomical consistence, form, and organization which it is to present during the remainder of life. "From birth," observes Billard, "to the first year, the brain of the infant is in a true state of transition, so that this organ, which is scarcely formed at first, arrives, towards the ninth month or first year, at the organization proper to the brain of adults. Ought we not," he adds, "to attribute to this modification supervening in the brain of infants, the frequency of cerebral affections at the age of which we speak?"[1]

But there is a *third* peculiarity, probably even more important than the two others. The human brain is very much larger in proportion to the body in the infant, than it is in the adult. In a full-grown adult the proportion of the brain to the body is as about 1 to 40; in the infant it is, as we have already stated, about 1 to 6, or nearly seven times greater. The influence of this immense difference is important both physiologically and pathologically.[1] The nervous system in the infant is thus rendered naturally much more susceptible and sensitive of physical and sympathetic morbid impressions than at a more advanced age;[2]—pathological irritations and disturbances of other parts are far more readily and strongly reflected upon the nervous centres;—and, for the same reason, any direct lesion or disease of the brain itself comes also in turn to exert a more potent and extended effect upon the general and individual functions of the body, than at other later periods of life. All these conditions are heightened and increased in the male as compared with the female infant at birth, both, *first*, by the greater absolute size of the male brain; and, *secondly*, by the greater lesions and injuries to which it is subjected during the process of labor. I have collected some statistical data to show that the particular diseases which destroy so many more male than female children immediately after birth and for a short time subsequent to it, and which are therefore the evident and demonstrable causes of the excess of the male over the female mortality at that early period of life, are all such as are capable of receiving an explanation of their more marked frequency, severity, and danger upon the above principles, in conjunction with the consideration of the increased size and increased injuries, at birth, of the male as compared with the female encephalon. To adduce evidence of this to the amount that might be necessary, would require us to extend this essay far beyond the limits prescribed to it, and these it has already far overstepped. I may, on another occasion, return to the subject; and, in the meantime, shall only add, that the particular affections alluded to, as producing the excess of the male over the female mortality, which is so remarkable at birth, and progressively decreases from that time onwards, are almost all referable in their origin or course to morbid states and disturbances of the nervous system,[1] and to functional derangements and inflammatory action in it and other organs of the body.

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[1] Billard, Traité des Maladies des Enfans, p. 335, Brussels ed.

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[1] Tiedemann, on whose authority I have given the respective proportion, stated in the text, of the whole brain in the infant and in the adult, observes, in relation to this point, and his words apply equally to the morbid as to the healthy states of the system, "The different degrees of susceptibility and sensibility of the nervous system seems to depend on the relative size of the brain as compared with that of the body. Children and young people are more susceptible, irritable, and sensitive than adults, and have a relative larger brain. In diseases which affect the nourishment of the body, the susceptibility increases as the patients grow thinner. The susceptibility and sensibility decreases on the other hand, with persons recovering from a long illness gradually as they regain their strength. The degree of sensibility in animals is also in proportion to the size of the brain. Mammalia and birds have a large brain, and are more susceptible than amphibious animals and fishes."—London Philosophical Transactions, vol. cxxvi. p. 503.

[2] "The nervous system in infants is naturally in a very susceptible condition; it is consequently easily excited on the application of the slightest impressions. This constitutional irritability is characteristic of the infant state . . . . The nervous sensibility is in excess at this age, and whatever be the disease with which the infant is affected, a morbid excitability and irregularity of action in the nervous system are more or less its attendants."—Stewart's Practical Treatise on the Diseases of Children, New York, 1841, p. 494. See also Maunsell and Evanson on the Diseases of Children, p. 13, &c.

PRACTICAL INFERENCES, AND EXTENT OF INFLUENCE EXERTED BY THE MALE SEX OF THE INFANT UPON THE GENERAL MATERNAL AND INFANTILE MORTALITY DURING PARTURITION AND FOR SOME TIME SUBSEQUENT TO IT.

The subject of the preceding inquiry is certainly not without interest in a physiological point of view. Nor is it devoid of importance in a pathological and practical light. The results of the whole investigation show us with demonstrative force that the adaptation of the fœtus to the maternal passages in human parturition is, in general, so close and perfect, that a deviation in their relative size of the slightest possible extent, is capable of altering immensely, when the subject is viewed on a large scale, the consequences of the process as regards both the immediate safety and life of the mother and infant, and their subsequent welfare. We may be hence led to see more strongly than previously the advantages attendant upon the presentation of the head of the infant in its most natural position, and consequently in its smallest diameter, and the disadvantages accompanying any deviations in its presentation, seeing that all these deviations offer a larger circumference than the normal parietal case. The same inquiry illustrates well the importance of saving, and particularly in cases in any degree tedious, or in any slight malpresentation, the space of the pelvic passages as much as possible, by keeping the rectum and bladder empty, by carefully guarding against congestion and tumefaction of the soft tissues lining the pelvis, and by promoting, by all appropriate means, the dilatability of the maternal passages. We can easily, from the same data, understand how greatly the dangers of the mother and child are increased by organic contractions, however slight, in the pelvic passages and bones themselves, and by increased volume on the part of the infant, whether that increased volume be the effect merely of excessive growth or the result of actual disease.

The object of our investigation becomes one of still more practical moment when we consider it in relation to the extensive character of its operation and influence.

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[1] "The diseases of the nervous system," observes Mr. Farre, "are *twenty-three per cent.* more fatal to males than females, *the chief difference arising from the diseases which affect children.*"—Registrar-General's Second Annual Report, Appendix, p. 4.

The importance of any cause of human disease not so much depend upon the immediate intensity and danger of the morbid action which it is calculated to excite in single individuals, as upon the frequency and extent of its operation, and the consequent amount of its effects upon the general community. In this way a slight disease or slight cause of disease, which acts upon a great proportion of the population, may become as important in its ultimate and practical results as a malady of the most formidable character, provided that malady is much more narrowed and limited in its attacks. "The cholera," observes Quetelet,[1] "and the influenza are diseases which differ greatly from each other; the one is a dreadful scourge, which manifests itself in the most dreadful manner; the other, in its ordinary external appearance, resembles a catarrh or common cold; and yet the tables of mortality prove, that, although the latter disease is not so deadly, it nevertheless, in consequence of its universality, and in consequence of the sufferings it causes, produces results nearly as extensively fatal as cholera. Facts serving to confirm this opinion may be found in the excellent work published by Dr. Gluge on the History of Influenza."[2]

The remark which we have applied to diseases and the causes of disease holds equally true with regard to morbid complications in midwifery and their causes. Those special states which, in obstetric practice, are seen to lead to some of the most formidable causes of difficult labor, such as tumors in the pelvis, malpositions of the infant, inertia of the uterus, &c., are certainly often most disastrous in their immediate effects, but yet, upon the whole, they are comparatively so rare in their occurrenee as not individually to lead, upon a large scale, to consequences of so severe and fatal a character, as disturbing agencies of a far slighter, but far more general kind, such as that inconsiderable enlargement of the male over the female head, which we have been considering in the preceding pages. A few computations will best illustrate and enforce the truth of this abstract remark. These computations I shall found on the idea that the bases of calculation offered by the data of the Dublin Hospital are sufficiently correct to serve for a ground of analysis of other analogous statistical results. To avoid as far as possible any probability of great errors, we shall keep our calculations considerably within the limits that our apparent standards might seem to warrant.

With this explanation, let us proceed to inquire to what extent the cause of difficulty and danger during labor, which forms the topic of our present essay, fatally influences each single year in

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[1] Natural History of Man, p. 112.

[2] Die Influenza oder Grippe, u. s. w. Minden, 1837, 8vo.

England, or during a succession of years, the results of parturition, as respects the fate both of the mother and infant. Some facts, recorded by Mr. Farre, in his admirable contributions to the different annual reports of the Registrar-General, will serve as matters of comparison on these points with the Dublin Hospital returns.

Number of maternal deaths referable annually in Great Britain to the influence of the comparatively great size of the head of the male infant.

In the returns of Drs. Clarke and Collins, we have reports in the Dublin Hospital of the sex of the child in 368 cases in which the mother died from labor or its consequences. In 231 instances the child was male; in 137 cases it was of the female sex. The proportion of maternal deaths, after male and female births, was therefore as follows:

Total deaths.	With male children.	With female children.	Proportion of males to females.
368	231	137	168 to 100

If we venture, then, to compute from this data, that out of 250 mothers that die in childbirth from parturition or its effects, 150 have given birth to males and 100 to females, as the above table would seem to show, then we have in every 250 maternal deaths an *actual excess* of 50 cases of loss of the mothers after male births, for which, for reasons which we have previously stated at length, we can find no other explanation than the greater comparative size of the male head, and we have already attempted to prove that this explanation is in itself logically and amply sufficient to account for the consequences which we have attributed to it. In other words, among every 100 parturient mothers that die, we have 40 perishing after producing female children, other 40 perishing after producing male children, and the remaining or additional 20 in the 100 perishing also after the birth of males, but so far forming a regular and constant *excess* of 20 per cent. of deaths in connection with male births, traceable to no other cause than the sex and consequent size of the infant. One in every 5 maternal deaths, or 20 in every 100, and 200 in every 1000, are so far the direct or indirect consequences of the greater dimensions of the head of the male infant. Now, at the present day in Great Britain, upwards of 3000 women die every year from childbirth or its immediate effects. Hence, according to the above computation, 600 of these 3000 cases,[1] as forming the *excess* of male over female births, are more or less immediately the results of the sex and size of the male infant. In Great Britain, therefore, the valuable lives of 500 mothers, to speak within terms, are annually lost in childbirth through the influence and agency of the cause in question.

Number of infantile deaths occurring annually in Great Britain during labor, and referable to the sex and size of the male infant.

We have seen under our fourth proposition, that in the Dublin Hospital there died, during the process of parturition, and probably as a consequence of the injuries to which they were subjected, 151 male children for every 100 females. According to the mode of argument followed in the preceding paragraph, there was thus an *excess* of 50 male deaths among every 250 children, or 20 in every 100, referable to the greater size of the head of the male infant. Further, we may take it for granted that, on a low computation, 1 in every 50 children born dies during labor, about 1 in every 25 cases being a still-birth. To be certain, however, not to overstep our limits, let us reckon only one in every 75 children to die during parturition, and 1 in every 5, or 20 per cent. of those that thus perish, to be formed by that excess of the mortality of males over females which we can trace to no other cause than the influence of the greater dimensions of the male head. In England and Wales about 500,000 births take place annually.[2] By the above computation more than 6500 of the offspring of these births die during labor, and one-fifth of that number are lost in consequence of the sex and size of the male child. In Great Britain, therefore, the lives of 1500 infants are annually lost in childbirth from the operation of this agency.

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[1] During the four years from 1838 to 1841, there died in childbirth in England and Wales 11,722 mothers. In 1841, the last year of which the returns are yet published, 3007 women died in childbed in England and Wales.—Fifth Annual Report of the Registrar-General, p. 380. If to these we add 500 deaths as occurring in childbed in Scotland, we will be much within the limits in computing 3000 maternal deaths to take place annually in Great Britain from labor or its immediate consequences. I reckon 500 deaths as occurring annually in Scotland in childbed on the calculation which seems to hold in regard to England, that nearly 200 deaths happen every year from this cause in every million of the general population.

[2] "In 1841, 512,158 births were registered, 1840,502, 203, and in 1839, 492,574."—Registrar General's Fifth Report, p. 8. It seems doubtful if all the births that occurred were registered, as this, like the other branches of registration, could not at once be made perfectly efficient. Certainly it is to be greatly lamented that there is no official return of the still-births. The omission is a most extraordinary one, and surely ought to be corrected.

Number of infantile deaths occurring annually in Great Britain within the first year of life, referable to the influence of the sex and size of the male child during labor.

In 1841 there died in England and Wales, within the first year of life, 41,444 boys and 32,766 girls. There was thus an excess of 8678 deaths of male infants within the first year, and 3610 of this excess occurred within the first month after birth. In considering this subject under our seventh proposition, I have already stated our reasons for considering the excess in question of male over female deaths, in very early infantile life, as owing to the greater injury sustained by the head and nervous system of the male child in its passage through the pelvis during delivery. I have further commented on the same subject in speaking of the excess of size of the male head and encephalon as the probable cause of the greater difficulties attendant upon male births. Under these points enough has been stated to show that a large proportion, if not the whole, of the remarkable excess of the male over the female mortality, which is observable for some time after birth, is traceable to the greater dimensions of the male head and encephalon, and their consequent greater compression and injury during delivery. Supposing these views to be perfectly correct, there die annually in Great Britain upwards of 5000 children within the first year after birth, whose death is referable to the influence of the sex and greater size of the male head during labor.

If we add together the three series of observations that have been stated under the preceding heads, the result is as follows: *upwards of* 7000 *deaths in all, namely, above* 6500 *of the deaths of infants during and after birth, and* 500 *of the deaths of mothers in childbed occurring annually in Great Britain, are referable to the direct or indirect agency of the cause that we have been discussing, viz., the sex and larger size of the head of the male child.*

In using here the term *cause* in reference to the slightly greater size of the male head as the source of the results that we have above ascribed to its influence, it may be proper to state the precise meaning which we attach to this term. Amidst all the uncertainties of medical language, there is no phrase which has had more vague and contradictory significations attached to it. Except in the cases in which disease and death are produced by the immediate action upon the body of severe physical injuries, or strong chemical agents, it rarely or never happens that serious and fatal morbid actions are excited in the economy through the operation of one single morbific cause. In most, if not in all, other instances, a number of causes are found to have acted either in concourse or succession towards the production of the disease that may be present, and the effect to which it leads. According to the order which these several causes may occupy in the series or chain of antecedent sequences, and according, in some instances, to their supposed relative importance and intensity in the production of the existing morbid action, they are individually described by pathologists as accessory or determining —remote or immediate—predisposing or exciting—in relation to the disease that has been originated by their combined influence. Hence a cause which may be termed predisposing in one case may be more properly designated exciting in another, or the reverse, and consequently different authors often describe the same cause under opposite heads.[1] Further, it seldom occurs during the course of a fatal disease that the same cause which had especially excited the original malady continues, by the intensity of its own immediate action, to lead to the fatal result. An example will illustrate our meaning. All pathologists look upon excess in eating and drinking as a common cause of disease. If the body be debilitated by previous morbific agencies, a fit of intemperance may suddenly excite in it some serious and deadly morbid action, as extensive effusions or inflammation, severe fevers, &c., and thus act as an *exciting* cause of these diseases; or when the powers of the constitution happen at any time to be impaired by intemperance, the application of an excess of cold or heat may lead to similar consequences, and then the cause we are considering (the intemperance), would be regarded as a *predisposing* one. Further, it rarely happens that intemperance in eating or drinking leads to death by the mere excess of their own action and influence on the body. Few men, in other words, die of direct gluttony, or direct intoxication; but many die from intemperance in eating and drinking, leading directly or indirectly to a variety of maladies, such, according to Dr. Copland, as "plethora, inflammatory complaints, functional and organic diseases of the stomach, liver, and bowels, gout, apoplexy, paralysis," &c.[2]

The view which we have stated as generally taken by pathologists with regard to intemperance as a cause of disease and death, applies exactly to the greater size of the male head as a cause of the greater number of complications and casualties connected with male births. In some instances this excess of size acts as a predisposing cause to the accidents and diseases that are consequent to it, and, without its antecedent or concurrent agency, these accidents and diseases would not be produced. In other instances, it acts as a determining or exciting cause of the morbid conditions and complications resulting from its operations. We will take an illustrative example from an accident which we have already spoken of in a previous page, viz., rupture of the uterus. When rupture of the uterus occurs in a patient who has already borne a large family, and where the uterine parietes are, from some pre-existing disease, much more weak and lacerable at one part than another, the greater delay and difficulty of the labor in connection with the larger sized male head may give rise ultimately to the complication in question, when the shorter and more easy passage of a smaller fœtal head would not have led to the same disastrous consequence. Here the previously diseased state of the uterus acts as the predisposing, whilst the larger size of the male head acts as the exciting or determining cause of the lesion. On the other hand, when rupture of the uterus occurs in a first labor in consequence of the larger size of the male head calling forth a morbid and inordinate degree of uterine contraction for its expulsion, we have this inordinate uterine contraction acting as the exciting or determining cause of the injury, and the excess of size of the male head standing now in the relation of the predisposing cause to it. Further, like other pathological causes, the excess of size of the male head does not so often lead to accidents and death by the mere influence of its own intensity, or by the effects of direct compression upon the maternal passages or infantile head, as by leading to the supervention and excitement of various other formidable and fatal complications, such as morbid delays in the labor demanding instrumental interference, eclampsia, post-partum hemorrhage, puerperal fever, &c., on the part of the mother—and convulsions, nervous and inflammatory diseases, &c., on the part of the infant.

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[1] For instance, Drs. Copland and Craigie, two of the latest British writers on practical medicine, speak of intemperance in food and drink under two different heads. "Amongst the most frequent *predisposing* causes to disease is," says Dr. Copland, "intemperance in food and drink."—Dictionary of Practical Medicine, Part ii. p. 562. Dr. Craigie enumerates the same morbific agency among his exciting or occasional causes of disease.—Elements of the Practice of Physic, p. 18. Dr. Williams, again, describes excess of aliment among his exciting causes of disease, and habitual excess in stimulating drinks among his class of predisposing causes.—Principles of Medicine, 1843, pp. 9 and 24.

[2] Medical Dictionary, Part ii. p. 562.

Under the signification and reservations which we have thus stated, we repeat that the greater size of the male fœtal head is the more or less immediate *cause* of a large number of those maternal and infantile deaths that take place in connection with labor, or as a result of that process. In illustration of this remark, I shall venture to add another observation—one which may at first seem sufficiently startling to those who have not practically directed their attention to the subject, though it is but a simple and direct deduction from the facts we have so imperfectly brought forward in the preceding pages. It is this:

The official returns of the mortality of England and Wales have only, as yet (1844), been collected for somewhat upwards of seven years, viz., from 1st July, 1837, to the present date. If the calculations we have already given are accordant with truth (and we believe them to be much within the limits), there have been lost in Great Britain during that limited period, as a consequence of the slightly larger size of the male than of the female head at birth ABOUT 50,000 LIVES, INCLUDING THOSE OF ABOUT 46,000 OR 47,000 INFANTS, AND OF BETWEEN 3000 AND 4000 MOTHERS WHO HAVE DIED IN CHILDBED.

Since the appearance of the preceding memoir, additional and confirmatory statistics have been given to the profession. In France, M. Chereau, whose attention was called to the subject by the perusal of Dr. Simpson's paper, has analyzed the official returns of stillbirths in Paris from 1817–36, drawing deductions from them similar to those expressed in the propositions laid down by Dr. Simpson. His paper will be found in the Annales d'Hygiène Publique, 1846, tom. xxxvi. p. 65.—(*Ed.*)

ON HEAD PRESENTATIONS,

WITH THE FOREHEAD ORIGINALLY DIRECTED FORWARDS OR TOWARDS THE PUBIS.[1]

(From the Northern Journal of Medicine, April, 1846, p. 216.)

At the Maternity Hospital, you have had repeated opportunities of watching cranial presentations in which the face or forehead of the child was primarily directed forwards, or towards the pubis. The Hospital records amply testify to the frequency with which some of you have met with this position of the infant's head. At the same time, you must permit me to remark that, for obvious reasons, I do not place any great reliance upon the perfect accuracy of the reports of positions entered in these records by the younger pupils. In fact, one of their principal objects in Hospital and Dispensary instruction is, to acquire, among other matters, a practical knowledge of the subject of positions—and to consider them always accurate on this point, is to presuppose them already proficients in a difficult department of clinical observation. But Dr. Martin Barry (our invaluable House Surgeon, and a gentleman to whose talents, and zeal, and humanity, the hospital is deeply indebted for its prosperity) has been so kind as to furnish me with a table showing the positions of the head in 335 cranial presentations, that he had himself carefully observed and noted among the patients of the Institution, and on which, therefore, we may place implicit confidence. I shall classify these 335 presentations according to the four positions and numerical nomenclature used by many of the German schools.

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[1] A Clinical Lecture.

Table showing the Positions of the Head in 335 Cranial Presentations at the Edinburgh Maternity Hospital.

I.—OCCIPITO-ANTERIOR POSITIONS.	
First Position; or occiput directed to left foramen ovale, in . . .	256 cases.
Second Position; or occiput directed to right foramen ovale, in . .	1 "
II.—OCCIPITO-POSTERIOR POSITIONS.	
Third Position; or occiput directed to right sacro-iliac synchondrosis, in	76 cases.
Fourth Position; or occiput directed to left sacro-iliac synchondrosis, in	2 "
Total,	335

It is to this latter division of cases (*the occipito-posterior*), that I wish particularly to call your attention at our meeting to-day. And I do so, in order to point out to you at some length their mechanism and management—to show you the strange and instructive errors that have been fallen into by most British authors, in the study and description of them—and to enable you to examine more fully for yourselves, into the actual phenomena of these cases when they happen to occur in your future hospital and dispensary practice.

NOMENCLATURE AND NATURE.

You will find the class of cases to which I advert discussed by different writers under a variety of different appellations. Dr. Clarke has described them as cases in which "the face of the child presents to the os pubis." Various English authors speak of them as cases in which the forehead, instead of the occiput, of the child, is turned or inclined forwards to the pubis, or to either groin. They form the *fronto-cotyloid* positions of some foreign authors, the *occipito-sacral*, and *occipito-sacro-iliac* of others. They constitute, as you are aware, the third and fourth positions of those German authorities, who use the numerical nomenclature, illustrated by the preceding table.

In order that you may understand more precisely the nature of the cases to which I wish at present to direct your attention, allow me to recall to your memory, that anatomists and obstetricians describe four diameters of the brim of the pelvis, viz., 1, the conjugate; 2, the transverse; 3, the left oblique; and, 4, the right oblique; that it very rarely, or indeed, almost never happens that, at the commencement of labor, the long diameter of the child's head enters the pelvis in either of the first two or direct diameters; and that of the last two or oblique diameters, the left, or that marked by a line running from the left sacro-iliac synchondrosis to the opposite or right foramen ovale, is comparatively seldom occupied by the head, because behind, its length is curtailed by the presence of the rectum. In fact, in 99 out of every 100 cases of cranial presentation, the long diameter of the head of the infant is found placed, at the commencement of parturition, in the direction of the *right* oblique or diagonal diameter of the brim, or in a line running from the right sacro-iliac synchondrosis to the left foramen ovale. In the living subject, this diameter—the right oblique—is the longest. The child's head is of an ovoid form, and, consequently, has a long and short diameter; and the longest diameter of the child's head is, as an almost invariable rule, found placed by nature in this, the longest diameter of the brim of the mother's pelvis, in order that it may pass through it with the least possible degree of obstruction, and, consequently, with the least possible degree of difficulty and danger.

In considering, therefore, the mechanism of normal cranial presentations, we hold it as an important and established principle, that the line of the long diameter of the child's head is, on entering the pelvis, placed in the line of the right oblique diameter of the brim; that, at the commencement of labor, it is comparatively seldom parallel with the line of the shorter or left oblique diameter; and that still more rarely, and, in fact, only when the pelvis is misshapen, or the child very small, is it ever found situated in the direction of the conjugate or transverse diameters.

In proof of this somewhat abstract statement, I may appeal to the table of cases which I have already shown you as the result of Dr. Barry's observations; or I may illustrate my remarks by the following evidence published by the younger Naegele, with regard to the ascertained positions of the head in 3491 cranial presentations observed at the Heidelberg Hospital, from 1827 to 1841.

Table showing the positions of the head in 3491 *Cranial presentations at the Heidelberg Lying-in Hospital.*

I.—OCCIPITO-ANTERIOR POSITIONS.	
First Position; or occiput to left foramen ovale, in	2262 cases.
Second Position; or occiput to right foramen ovale, in	4 "
II.—OCCIPITO-POSTERIOR POSITIONS.	
Third Position; or occiput to right sacro-iliac synchondrosis, in . . .	1217 cases.
Fourth Position; or occiput to left sacro-iliac synchondrosis, in . . .	8 "
Total,	3491

In none, I believe, of these 3491 cranial presentations was the head found lying in the transverse or conjugate diameter of the brim; in 12 only (viz., in the four cases of the second position, and the eight cases of the fourth position) was it found placed in the left oblique diameter of the brim, and in all the remaining 3479 it was primarily situated in the right oblique diameter.

To prevent, therefore, unnecessary complications and repetitions, I beg you to observe that in my future remarks in this lecture I shall leave entirely out of view those rare, and, from their rarity, almost anormal cases in which the head enters the brim in the left oblique diameter. This omission will simplify our subject for us.

Next, observe this other point—either end of the axis of the long diameter of the fœtal head—that is—either its occiput or forehead—may be placed at either end of the long or right oblique diameter of the brim of the pelvis. Most frequently we find the occipital extremity of the infant's head placed forwards, or situated at the anterior extremity of the right oblique diameter. In other words, most frequently the occiput is placed behind the left foramen ovale; and this constitutes the *first* position of most authors who use a numerical nomenclature. But the arrangement is sometimes exactly reversed; that is to say, occasionally the frontal extremity or forehead of the infant is placed forwards or behind the left foramen ovale, and consequently the occiput is, under this cranial position, directed backwards, and to the right sacro-iliac synchondrosis. To recapitulate;—in this last variety of cranial position (the immediate object of our investigations), the long diameter of the child's head lies as usual at the commencement of labor, parallel with the left oblique diameter of the brim, but with the face or forehead, instead of the occiput, placed behind the left foramen ovale; or in other words, pointing forwards to the left groin. Dr. Hamilton and others have properly stated, that these are not strictly cases of presentations of the forehead "to the pubis," but of presentations of the forehead to one or other groin. And I have already shown you why the groin to which the forehead points is so far more frequently the left than the right, that for the present, I leave the latter variety out of consideration, and apply my subsequent remarks entirely to that cranial position in which the forehead points to the left foramen ovale or left groin. This position is the *third* position of the head in the two tables of cases which I have already shown you.

Having premised these dry but necessary details, let me now solicit your special attention to several more important and practical points connected with the clinical history of this important class of cases. I have repeatedly remarked to you, that, in its theoretical and practical results, the study of the mechanism of labor in some cranial and other presentations forms a sad satire upon the general accuracy of obstetric observation. I could not easily adduce a better clinical example of this remark than the whole study of occipito-posterior positions of the head will now afford us. And first let us inquire, when, at the commencement of labor, the occiput is situated posteriorly in passing through the brim of the pelvis, in what relative position does it emerge from the outlet? In other words, in occipito-posterior positions,

DOES THE OCCIPUT MAINTAIN THE SAME RELATIVE POSITION TO THE BACK OF THE PELVIS IN PASSING THROUGH THE BRIM, CAVITY, AND OUTLET.

In most of your text-books you will find this question answered in the affirmative. For instance, Denman, Merriman, Blundell, Hamilton, and others, allege, that in the natural mechanism of this class of cases the head retains throughout the same position relatively to the parts of the mother—that it enters the brim of the pelvis, passes down into the cavity, and makes its exit through the outlet in the same position: that is, with the forehead or face always directed anteriorly, and consequently the occiput always pointing posteriorly. Long ago, Solayres showed distinctly that this was by no means the common and natural course of events in instances of this presentation, but his observations were unattended to. Since Naegele, however, wrote his admirable essay on the Mechanism of Parturition, the whole subject has been much more studied; and we now know, from abundant and accumulated evidence, that the ideas generally held with regard to the mode in which labor proceeds in occipito-posterior positions are quite incorrect. For it has been ascertained, beyond the possibility of doubt and cavil, and you shall have constant opportunities of confirming it by your own observations, that in almost all cases in which the occiput is originally placed posteriorly, the head, upon descending down upon the floor of the pelvis, so far changes its position as to rotate round, so that at last the occiput, and not the forehead, emerges anteriorly under the arch of the pubis; and consequently, the face, which looked forwards, or towards the pubis at the commencement of labor, is turned backwards towards the sacrum or perineum at the conclusion of it. In short, in reference to cranial presentations, we may lay it down as a *general law*, admitting of few and occasional exceptions only, that in whatever position the head is found entering the brim, whether with the occiput directed anteriorly, or directed posteriorly, it will ultimately pass through the outlet and vulva with the occiput placed under the arch of the pubis, and the forehead and face gliding over the perineum. When the forehead is primarily situated behind the left foramen ovale, as occurs in the cases forming the subject of my remarks, the head, in passing through the lower pelvic apertures, rotates to the left and backwards a quarter of a circle, and at last comes out at a position at right angles to that in which it entered; the occiput, which was originally placed opposite the right sacro-iliac synchondrosis, rotates to the right and forwards in a corresponding degree, so as to be placed opposite the foramen ovale of the same side, and emerges under the arch of the pubis; and the long diameter of the infant's head, instead of remaining parallel with the right oblique diameter of the pelvis, is latterly born in parallelism with the left oblique diameter of the outlet.

But what evidence have we of the actual occurrence of this curious change in the relative positions of the occiput and forehead at the brim and outlet? I shall state to you the principal

PROOFS OF THE ROTATION OF THE OCCIPUT FROM THE POSTERIOR TO THE ANTERIOR PART OF THE PELVIS.

1. You may have constant opportunities of proving to yourselves the occurrence of the rotation in occipito-posterior positions, by observations of this kind. Keep your forefinger, in such a case, steadily upon the anterior fontanelle during the whole passage of the head down into the cavity, and through the outlet of the pelvis, and each of you will, by this form of direct and personal observation upon the living subject, readily convince yourself that the forehead of the infant does rotate round from the anterior to the posterior segment of the pelvis when the head is about to pass through the vulva, or that it makes a rotation from the left foramen ovale round to the left sacro-iliac synchondrosis. The evidence amounts to this—you can actually *feel* the rotation take place.

2. Experiments made by pressing a dead child, placed in an occipito-posterior position, through the pelvis of a dead mother, give the same result. In this experiment, which I have tried in several instances, we can *see* the head placed in the brim of the pelvis in one position, viz., with the occiput directed posteriorly, emerge from the outlet in another position, viz., with the occiput directed anteriorly. This experiment will even so far succeed with the dried fœtal skull and the dried pelvic bones, provided you select a skull and pelvis adapted in size to each other, and apply the impelling force in a proper direction. Thus [*showing the experiment*] this fœtal skull cannot, for want of space and adaptation, be forced through the outlet of this pelvis in an occipito-posterior position; but as soon as the occiput turns forwards, it not only passes easily, but even admits, in addition, of one or two fingers passing out along with it.

3. I have had occasion to show to you more than once at the hos-

pital, that, in natural or artificial footling cases, when the toes, abdomen, and face point, as they sometimes do, anteriorly, the child rotates round in the course of its expulsion, and at last is born with the toes, abdomen, and face pointing posteriorly. The rotation is of the same extent and nature as the rotation of the head itself in occipito-posterior positions; but in these footling cases we have actual *visual* evidence of the turning—an observation which, in the living subject, cannot of course be made in the corresponding cranial positions.

But in a few cases, the head in occipito-posterior positions may be observed by you to pass out with the occiput still placed posteriorly. In other words, you will from time to time see

EXCEPTIONS TO THE GENERAL RULE OF THE ROTATION OF THE OCCIPUT FORWARDS.

Out of 76 cases of occipito-posterior positions, carefully watched by Dr. Barry at the Edinburgh Maternity Hospital, within the last year, in 74 the occiput turned forwards and emerged from under the arch of the pubis; and in 2 the occiput retained its primary posterior position, so that the forehead at last passed out under the arch of the pubis. In 96 cases noted by Naegele, he found the head to rotate as I have described in 93; and in the remaining 3, no rotation occurred. These important facts may perhaps be expressed more strongly in a tabular form thus:

Proportion of occipito-posterior positions in which the occiput ultimately passed through the vulva anteriorly or posteriorly.

	Total cases.	Occiput at last posterior.	Occiput at last anterior.
Heidelberg Hospital, ...	96	in 3	in 93
Edinburgh Hospital, ...	76	in 2	in 74

The exceptions to the rotation occur, therefore, not more frequently than once in thirty cases;—and further, in the exceptional instances you will generally find either the head unusually small, or the pelvis of the mother unusually large. Yet, as I have already stated, almost all obstetric authors describe the exception as the rule—and the rule as the exception. Now,

BY WHAT POWERS OR MECHANISM IS THE ROTATION EFFECTED?

That the rotation of the head in labor is not produced by vital actions on the part of either the infant or the pelvic canals, is demonstrated by this one simple fact—that, as I have already stated to you, the rotation may be seen to occur when, in imitation of labor, the experiment is tried of forcing the body of a dead infant through the pelvic canals of a dead mother. Here, as during life, the rotation of the fœtal head upon the floor of the pelvis is, on a contracted scale, like that of the spindle of a screw upon a nut, provided with one-half of a single or broken thread. If relatively the spindle is too small, or the nut too large, the former may pass through the cavity of the latter without any rotation—as in the exceptional cases just now alluded to;—but if they are properly adapted in size to each other, the spindle can only make its transit by its helix rotating upon the inclined plane of the corresponding helix of the imperfect nut.

Further, it appears to me that very incorrect and erroneous opinions are generally held by obstetric authorities as to the mechanism of the partial rotation of the head in occipito-posterior positions, and at the same time I am inclined to look upon it as a physical problem of no very difficult solution. In order that you may understand the explanation of it which I have to propose to you, it is necessary to hold in view the following axioms: 1. That the impelling power of the uterus is transmitted to the head of the infant along the line of the spine; 2. That the spine is articulated to the head nearer the occiput than the chin, and consequently (more especially after the flexion of the chin upon the breast) the expulsive force of the uterus bears most strongly and directly upon the occipital region; and 3. That the mobility of the smooth and lubricated surface of the fœtus upon the smooth and lubricated surface of the uterine and vaginal cavities is such, that if the occiput is forced to rotate in any degree, the occiput will, in turn, produce a secondary but corresponding rotation upon the forehead and other parts of the infant's head, and consequently upon the body also, unless it happen to be morbidly and tonically grasped by the uterus, which is rare. Observe one point more—the direction in which the power of the contracting uterus first impels the body and head of the child, is in the line of the axis of the brim of the pelvis—that imaginary line which is usually represented as running from the umbilicus to the lowest point of the sacrum. In the common occipito-posterior position, the impelling power of the uterus thus forces the *right* side of the occiput (for the head is placed obliquely) against the concave floor of the pelvis, and its further progress downwards and backwards in that direction is arrested by the resisting structures of that floor. The occiput is thus submitted to two forces meeting at an obtuse angle, viz., to the impelling power of the uterus from above, and to the resisting power of the pelvic floor from below—and these two powers act upon it with similar force—physical action and reaction being always equal. The necessary result is, that the part acted upon, viz., the occiput, moves forward and to the right with a screw-like movement—sometimes rapidly—sometimes slowly, and in a line diagonal to the lines of the forces acting upon it—exactly as if it were impelled by a single force. Or, as mathematicians express it, it moves in a line diagonal to a parallelogram, two of the sides of which are formed by the lines of the two existing forces. The diagonal or resultant line of motion of the occiput, at this point in the labor, is in a line running anteriorly in the direction of the axis of the outlet, and the movement of the occiput forwards in this direction is further greatly promoted and facilitated by this being, at the same time, the direction in which it encounters incomparably the least resistance to its further progress.

What part of the floor of the pelvis serves as the resisting plane upon, and by the aid of which, the occiput is thus made to rotate forwards?—In some—any, and in others, apparently every successive, portion of the concavity of the floor of the pelvis seems to serve this purpose. But the spines of the ischia contribute far less than is generally believed. I have repeatedly observed the rotation, when, in fact, the occiput was so low as to be beyond the influence of the bones and ligaments, and where the required resistance was apparently afforded by the soft tissues alone—as in the very last throes of labor. And I have seen the concavity formed by the prolapsed posterior lip of the uterus serve the same purpose before the head could be much or at all influenced by the counter-pressure of the pelvic floor. Generally, however, the rotation occurs on the concavity of the floor of the pelvis itself;—and I consider it of great moment that you should particularly note this fact, as demonstrating that if we try to turn the head, as some recommend, whilst it is still high in the pelvis, we do not imitate the mechanism of nature in this position, but so far act in direct opposition to the laws with which she has regulated this mechanism.

The best evidence which I can offer you of the correctness of that explanation of the rotation which I have stated, is afforded by this sufficiently rude and simple experiment. Place a full-sized infant's skull in the common occipito-posterior position, and try to force it through the outlet of a well-formed bony pelvis, in which the counter-pressure of the soft parts is, for the time being, imitated by the application of your hand, placed, as it were, so as to form a continuation of the concavity of the sacrum. Apply the requisite impelling force to the fœtal skull by passing a wooden or metallic rod through the foramen ovale, so that the sum of the momentum employed be, as it is in nature, directed to the occiput. The occiput will become thrown forwards under the pressure acting upon it from above, and the counter-pressure acting upon it from below. No obstetric author has, so far as I am aware, shown what I have attempted to point out to you, that it is the movement of the *occiput* that we are to study in this position, as being that part, the movement of which regulates the whole. We must leave the forehead out of the question of the mechanism of the rotation, as its motions are *secondary* and entirely the result of those of the occiput; and, without holding these facts in view, it will, I believe, be found difficult or impossible to give a rational explanation of the phenomena.

But let us pass on to another and less difficult inquiry, viz.:

ARE OCCIPITO-POSTERIOR POSITIONS OF THE HEAD FREQUENTLY MET WITH IN PRACTICE?

In former times, and up, indeed, to the present day, this position has been generally represented as exceedingly rare. Thus Dr. Clarke of London, in a special essay on the "management of cases in which the face of the child presents to the os pubis," almost congratulates himself upon having had the good fortune to observe 14 cases in the course of several years of his very extensive practice. A few years ago, Wigand of Hamburgh, who is said to have seen more midwifery than almost any other European accoucheur, states that he had only met with six or seven cases. Dr. Collins has recorded 12 cases as having occurred from 1825 to 1832, out of 16,000 deliveries in the Dublin Lying-in Hospital.

In contradistinction to the preceding statements, I beg to observe that, so far from seeing such cases rarely and at long intervals, I find that in one out of every three or four cases, among my private patients, I meet with this position of the head, or with the forehead looking forwards to the left foramen ovale or left groin. It is so very frequent, that I have repeatedly seen two or three instances of it occur in succession. Among the patients of the Maternity Hospital you will find this position as frequent as I have just stated it to be. And Dr. Barry's results, as to the frequency with which this position was observed among the patients here, contrasts strangely with the frequency with which the same position was observed among the patients of the Dublin Lying-in Hospital, as given by Dr. Collins in his invaluable report. The difference may be correctly expressed as follows:

Absolute number and proportion of Occipito-posterior Positions in the Dublin and Edinburgh Hospitals.

	Total Head Presentations.	Occipito-posterior Positions.	Proportion.
Dublin Hospital, ...	16,041	12	or 1 in 1336
Edinburgh Hospital, ...	335	78	or 1 in 4

If you search the records of obstetric statistics, you will find an infinite discrepancy upon the relative proportion in which this class of cases was noted by different observers. The following table, illustrative of this point, requires no comment:

Frequency of Presentations of the Face to the Pubis, or of Occipito-posterior Positions, as given by different Practitioners.

Collins	reports them as	1 in	every	1336	labors.
Bland	“	1 in	“	374	“
Baudelocque	“	1 in	“	346	“
Lachapelle	“	1 in	“	171	“
Cusack	“	1 in	“	151	“
Maunsell	“	1 in	“	121	“
Merriman	“	1 in	“	67	“
Lobstein	“	1 in	“	18	“
Gregoire	“	1 in	“	14	“
Murphy	“	1 in	“	7	“
Villeneuve	“	1 in	“	7	“
Barry, Joseph Bell, Rigby, Dubois, Stoltz, Naegele, &c.,		1 in	every	3 or 4	labors.

I have already stated that my own experience is perfectly in accordance with that of those practitioners whom I have placed last upon the preceding list. Indeed, I feel the most certain conviction that if any of you arrive at a different conclusion when you examine cranial presentations upon a large scale, it will not be from the facts being different, but from your interpreting them differently; and do not attribute your results, if they vary from that which I have last stated, to defective powers of observation on your part, but rather to a defective use of those powers which you do possess.

But the question naturally presents itself, how has it happened that, if occipito-posterior positions are so frequent, they should be reported and represented as so rare by various authors? The solution of this problem is easy when we consider the

FALLACIES COMMITTED IN JUDGING OF THE POSITIONS OF THE HEAD IN LABOR.

Many practitioners report merely the position of the head as it is observed to emerge ultimately from the outlet, and not as it may be found higher up in the pelvis. But we have seen, that in occipito-posterior positions the occiput almost always rotates forwards, before it passes the vulva. Hence one enormous source of error. For all, or nearly all those cases which were originally occipito-posterior positions come to be thus inadvertently reported among the number in which the occiput is situated anteriorly.

The following table will enable you to understand another great and common source of fallacy. In constructing and explaining it, I use the numerical nomenclature of positions which I have employed in giving Dr. Barry's observations at the beginning of the Lecture:

Relative Proportion of the four most common Positions of the Head given by different Observers, and calculated to a standard of 1000 *Cases each.*

	A.—Occiput to left side of Pelvis.		B.—Occiput to right side of Pelvis.	
	Positions.		Positions.	
	1st.	*4th.*	*3d.*	*2d.*
Naegele,	698	3	298	1
Dubois,	702	6	259	3
Barry,	763	4	228	3
Bell,	750	4	240	7
Halmagrand,	700	4	240	50
Murphy,	632	46	161	161
Boivin,	800	5	50	190
Lachapelle,	770	4	70	200

Now, in examining the returns collected into this table, you will observe that all agree as to the frequency of the first position of the head, or that in which the occiput is directed forwards to the left foramen ovale; and further, that they represent the fourth, or that in which the occiput is directed backward, to the left sacro-iliac synchondrosis, as being of very rare occurrence. But of the proportional numbers of the other positions, namely, those in which the occiput looks to the *right*, and either anteriorly or posteriorly, there are very discordant statements. I shall easily, however, be able to show you how to reconcile this difference.

One set of authors, among whom we have Lachapelle and Boivin, make the second position, viz., that with the occiput directed forwards to the right foramen ovale, common, at the expense of the third, or that with the occiput directed backwards and to the right sacro-iliac symphysis, and which they account much rarer. The others, among whom we have the names of Naegele, Dubois, Barry, and an able country practitioner, Mr. Bell of Barrhead, give a contrary account, for they correctly describe the third position as a frequent primary position, and the second as, on the other hand, the very reverse. This incongruity arises entirely from the former class of authors not adverting to the fact, that the second position is an ulterior stage in the progress of cases which originally belonged to the third. And, as they either omitted to make early and careful examinations, or preferred trusting entirely to observations, more easily made in a later period of the labor, so we are not to be surprised that they range and enumerate under the second position, cases which ought to have been referred to the lists of the third.

Every careful observer, however, who takes the pains to examine early, will find the cases of the third, or common occipito-posterior position, to be of common occurrence, while the second position may be almost excluded from our accounts of the mechanism of parturition, unless as a mere *stage* in the course of the other. Probably one of the most fallacious of all marks, and yet one sometimes relied upon, for determining the position of the cranium at the brim, is to observe the direction of the face after the head has passed the outlet. It is alleged, that if the face turn, during the passage of the shoulder, to the mother's left thigh, it marks that it has been a third or occipito-posterior position, and the reverse if it turns to the right. But such a criterion would constantly lead you into error. For the face of the infant sometimes does turn to the right after it has passed the outlet, even though it were originally an occipito-posterior position; and, in fact, you will often find, that if you abstain from all interference in the passage of the body, the head will occasionally revolve round to a greater or less extent, so that the face turns more or less anteriorly in correspondence as the body turns in a half spiral mode, in consequence of the shoulders making a screw-like motion upon the floor of the pelvic canal, as they become forced down upon and through the pelvic and vaginal outlet.

The occipito-posterior are also perhaps frequently confounded with occipito-anterior positions in the earlier stages of the labor, in consequence of the distinctive marks between them not being attended to and appreciated. To avoid this error, let us next then consider the

DIAGNOSIS OF CASES IN THE OCCIPITO-POSTERIOR POSITION, OR WITH THE FOREHEAD TO THE PUBIS.

There are three modes by which we may arrive at a diagnostic knowledge of the usual occipito-posterior position of the head with the forehead behind the left foramen ovale, viz., 1st, the locality of the fœtal movements as felt by the mother; 2d, the situation of the fœtal heart as determined by auscultation; and 3d, the tactile examination of the fœtal head itself. The first two may afford us a greater or less degree of certainty as to the position of the head before labor begins, or in the first commencement of it before the os uteri is much opened. It is to the third, however, that we principally or almost solely trust as our guide after labor has fully set in. I shall offer you a few details regarding each of these signs.

I. *The movements of the Fœtus.*—The fœtal movements are often felt by the mother much more on one side of the abdomen than on the other. The movements which she feels are principally the movements of the hands and feet, or rather the extremities of the infant. Hence the side of the uterus on which they are felt, indicates the side to which the extremities, abdomen, and face are turned. Consequently the back and occiput of the child look to the opposite side. Recollecting then, that the infant's head is almost always placed in the right oblique diameter of the brim, we may feel pretty confident that the occiput is placed to the right and anteriorly, if the fœtal movements are principally or only felt on the left side; and, on the other hand, that the occiput is placed to the left and posteriorly, if the fœtal movements are principally or only felt on the right side. The degree of distinctness with which the sensations of the fœtal movements are felt by the mother, confined to one or other side, will principally determine the degree of reliance that we may, in individual patients, place upon this sign. Many mothers, however, are not aware of the movements being greater in one part than another, and in such instances we must depend upon other aid.

II. *The sounds of the Fœtal Heart.*—When using the stethoscope, the fœtal heart is most distinctly heard over the left scapular region; and, if the fœtus is in the occipito-anterior position, the sounds of the heart will be heard most distinctly above the left groin of the mother, or in the left iliac region. If the fœtus is in the occipito-posterior position, or with the face towards the pubis, these sounds will be heard most distinctly, on applying the stethoscope to the right iliac region of the mother. Thus, in most cranial cases, the use of the ear gives you a strong presumption of the position, by marking whether it be placed with the occiput anteriorly and to the left —or posteriorly and to the right.

III. *The tactile examination of the Child's Head.*—Both the signs which I have already mentioned, the sensations of the fœtal movements, and the seat of the sounds of the fœtal heart, yield in value to the more direct and more certain evidence derived from actually feeling and touching the fœtal head with the fingers in a vaginal examination. When the finger is introduced, you feel the sagittal suture of the infant's head crossing obliquely the opening of the os uteri, in a line parallel with that of the right oblique diameter of the pelvis. Both in the common occipito-anterior, and common occipito-posterior position, this is the direction of the sagittal suture —so that the mere direction of the suture will not afford you a distinctive diagnostic sign. Run your finger, however, along the suture to either of its extremities—say to its anterior extremity, which is generally easily reached, and you may at once fully determine the position of the head, by ascertaining which of the two fontanelles is placed at the anterior or pubic extremity of the suture. If the fontanelle which you touch in this situation is four-limbed and quadrangular, it is of course the anterior fontanelle, and necessarily shows the forehead to be placed anteriorly; and consequently the occiput must be directed to the posterior part of the pelvis. But always be perfectly certain, that it is the anterior and not the posterior fontanelle which you are touching, that is, that it is a space or bregma quadrangular, and not triangular in its figure, and formed by the meeting of four, and not of three sutures. If you are not, especially in early practice—strongly upon your guard in relation to this source of fallacy—you will be exceedingly liable to fall into error on the point. But with due care and caution, you may readily surmount such a difficulty. In no case, I believe, will you find it necessary to force your finger up so far as to touch the ear of the child, as some authorities advise. But your ear may, by the medium of the stethoscope, add to the certainty of the tactile diagnosis by ascertaining, in the mode I have pointed out, the exact locality of the fœtal heart.

One subject more remains for our consideration in reference to cases of the occipito-posterior position of the head, viz., their

TREATMENT.

Upon this subject, let me, in the first place, state to you this strong and important fact—in not one of Dr. Barry's 77 cases—and in not one of the other 200 or 300 cases of head presentation, with the forehead directed forwards, which have occurred among the patients of the Maternity Hospital since its first institution—in none, I repeat, has any kind of aid or interference by the hand, or forceps, or other instrument, been required or given. In stating this as the result of our observations and practice here, I state what I believe should be the result of your observations, and the line of your practice, in your future professional experience. In a vast majority of occipito-anterior positions you require to offer no unusual aid or interference. In exactly the same way, in a vast majority of occipito-posterior positions—in 99 out of the 100—you require to offer no unusual aid or interference of any kind or description whatever. You must equally look upon the one position and the other as constituting a perfectly natural labor—so far as in each the mode and mechanism of parturition are concerned.

You will find, however, doctrines and practices very different from this, taught in regard to the treatment of occipito-posterior positions in most obstetric works and text-books, modern as well as ancient.

For very many advise and practise interference of an active character in all such cases;—and then, when the case ends favorably, as it generally does, they unhesitatingly ascribe the result solely to their interference—forgetful of the fact, that nature, as I have just shown you, does generally conduct the mechanism of the labor to this safe termination—and that too, sometimes in opposition to, and despite of the supposed assistance of art. In precisely the same way, in many of those diseases, the natural history of which is, that they will terminate favorably if time is allowed, and quietude and proper regiminal means be enforced—many practitioners will insist on inflicting upon their patients a continual system of drugging, and then subsequently arrogate entirely the patient's recovery to the influence of their drugs, allowing nothing to where everything perhaps is due, viz., the curative influence of nature herself.

In the treatment of occipito-posterior positions, the mode of interference recommended is sometimes most active. And one class of practitioners seem to believe that without interference nature could never, by her own unaided mechanism and efforts, bring a case of this kind to a successful termination.

Thus, a few years ago, Capuron read before the French Academy, and afterwards published in the Journal Hebdomadaire, a paper with this significant title, "On the impossibility of natural delivery, and the necessity of applying the forceps, in occipito-posterior positions of the cranium." In these cases Capuron avers, that unless the pelvis be unnaturally large, or the child unnaturally small—if the forceps are not used as a "precaution," failure of the powers of the mother, pressure, contusion, and inflammation of the intro-pelvic organs, will follow, with apoplexy and death of the infant. He attempts to prove all this on mathematical and theoretical grounds, rather than on the results of observation and practice—and correctly enough shows, by various admeasurements and arguments, that in the great majority of cases, the head cannot physically pass through the pelvic outlet if the forehead is placed anteriorly;—but he seems quite unaware of the other all-important fact, that in exactly this great majority of cases, the head is rotated round by the natural efforts, so that the forehead is ultimately placed posteriorly, and not anteriorly, and passes with perfect facility.

In relation to the supposed indispensable necessity of instruments in occipito-posterior positions, Puzos, Basignan, and others, have published opinions more or less similar to these opinions of Capuron. The general mass, however, of obstetric writers do not look upon artificial aid in such cases as entirely indispensable. But many seem to regard it as useful and proper in most cases of the kind, and have advised various modes of interference.

When you detect the occipito-posterior position upon the breaking of the membranes, you are, observes Pugh, "to lay the woman in a proper posture, turn, and extract the child by the feet, being the best and safest method that can be taken; but if sent for after a midwife, who perhaps has kept the patient too long, so that turning is impracticable, then you must have recourse to the curve forceps." Chapman details the case of a "lady of distinction" whom he delivered by turning, in consequence of the forehead or face coming towards the os pubis. Dr. Smellie, in the first case of the kind which he details, tells us he first tried to turn the forehead backwards, then attempted to bring the child footling, but, failing in this, he subsequently applied the forceps and blunt hook. In the latter part of his practice, he seems to have trusted to the forceps alone, when aid was required, and rotated the head with them. He details one case where he turned the forehead backwards with his fingers. Exton advises interference upon this last plan. In 1800, Dr. Clarke of London published a paper "on the management of cases in which the face of the child presents towards the os pubis." He speaks of the cases as "comparatively rare," "attended with considerable difficulty," and as having had no proper means suggested for their special treatment. The means, however, which he proposed and practised was not novel, and consisted of "introducing one or two fingers between the side of the head, near the coronal suture, and the symphysis pubis, and pressing steadily against the parietal or frontal bone during the labor pains, till the forehead is pressed backwards, and at length the occiput is brought to the groin." "I have now," he states, "met with 14 cases, in 13 of which the practice has succeeded, and as some years have elapsed since the first case, I think myself fully authorized in recommending this method to be always pursued, when the face is found in the situation above described."

From what I have stated of the natural mechanism of these cases, you know that in 13 out of the 14 cases of which Dr. Clarke speaks, nature would have herself performed the rotation in question, and that, too, with greatly less risk, and annoyance, and pain to the mother, than by the fingers of the accoucheur thrust up between the head and pelvis, and at the same time less danger to the child. "I have known," says Dr. Merriman, "one instance in which the space opened between the pelvis and the child's head, by passing the finger (as Dr. Clarke advised), allowed the funis to prolapse, and thus destroyed the infant."

In the "Practical Observations on Midwifery" which Dr. Hamilton published a few years ago, he adopts Dr. Clarke's practice in this set of cases, though criticising him, in so far that he properly points out the forehead to lie towards one or other groin, and not directly forwards to the pubis. "In such cases," Dr. Hamilton remarks, "there is a risk that the face be turned towards the pubis, if the practitioner have not sufficient intelligence to make strong pressure upon the brow, which is generally sufficient in the course of a few pains to turn the face into the hollow of the sacrum."

Dr. Dewees, in his valuable work on midwifery, declares, that he considers that "man incompetent to practise midwifery, who cannot detect and change this position of the head," in the mode recommended by Baudelocque and Clarke. His countryman, Professor Bedford of New York, in writing on the same subject in 1844, after commenting upon nature as "full of wisdom and benevolence, always vigilant and prompt," &c., proceeds to state, that when the practitioner "arrives at the bedside, and in making a vaginal examination, ascertains that the occiput is at one of the posterior points of the pelvis, he should, as soon as the mouth of the womb is sufficiently opened to admit the manipulation, introduce, with great gentleness and caution, his hand, and place his thumb on one of the lateral portions of the head, and the fingers on the other, and slowly elevate the head, at the same time bringing the occiput to one of the anterior points of the pelvis, either the left or the right acetabulum; for example, if the occiput be at the right sacro-iliac symphysis, it should be brought to the right acetabulum, if at the left sacro-iliac symphysis, to the left acetabulum. When this change has been effected, nature will usually be competent to accomplish the delivery, and the fœtus will not be exposed to the same hazard of a protracted birth. Indeed, if the change be not made, it will often be necessary to resort to the forceps to terminate the delivery; and if the labor be submitted to nature, the child will frequently be sacrificed."

In occipito-posterior positions, Dr. Burns recommends, like Dr. Bedford, early rectification of the head. "As," he observes, "this presentation, whichever way the head turn, is generally productive of a labor more tedious than the natural one, we should co-operate in the acceleration of the process of turning the head. If it be discovered early, it is certainly proper to rupture the membranes, and turn the vertex round, which is easily accomplished." Dr. Burns further advises us, that "if this opportunity be lost, we may still give efficient assistance," by Dr. Clarke's method of interference;—and in corroboration, he adds, that he has been himself successful in thus artificially pushing backwards the forehead, in cases "where the head seemed rather to be turning with the vertex towards the sacrum, and had descended so low as to have the nose on a line with the arch of the pubes." It is sometimes at this late point that nature turns the head by her own efforts, and occasionally during the course of a single pain. And I doubt whether Dr. Burns does not here ascribe to the efforts of art what belonged to the efforts of nature.

I shall cite only one other esteemed modern authority, viz., Dr. Blundell of London. Dr. Blundell's rules of practice in occipito-posterior positions are more heroic than that of any other of the writers I have mentioned—for his rules embrace one and all of the modes of active interference which I have hitherto named. His observations are too long for quotation; but he tells us that "when the case is indisputable, the dexterity great, and the circumstances are conducive, he will not venture to assert that turning the child is universally unjustifiable;" that if the softer parts are lax, the pelvis large, &c., rectification of the position of the head above the brim may be justifiable with the hand, or this may be subsequently done with the forceps, "remembering that you are operating upon the softer sex;" that in all cases, Dr. Clarke's method recommends itself to your adoption by its ease and safety, "unless turbulence and violence unfit you for the duties of an accoucheur;" that in the majority of cases, however, and especially if yet inexperienced in the practice of midwifery, you may trust with confidence to the natural efforts; that if all these methods, and nature also fail, the lever or forceps might be tried "with tenderness and prudence;" or at last, "compelled by an inexorable necessity, you must have recourse to the perforator." "Adhering to these directions, I am persuaded," says Dr. Blundell, "you cannot wander far from the correct line of practice."

Strangely enough, Dr. Blundell introduces his long remarks upon this subject with the following pertinent criticisms. "It seems," he observes, "that where the face throughout the labor is lying forward on the symphysis, many difficulties are occasioned. What is it then," he asks, "that the accoucheur can do in order to diminish, surmount, or remove them? What is there that he can do with prudence, without committing the *unpardonable sin* of midwifery—the sin I mean of those obstetric reprobates, the meddlesome, and the pragmatic?" And he closes his discussion of the treatment in the following words: "Under the best management, *unless* you can rectify, these are bad cases, for bruising, laceration, and sloughing of the parts, and the death of the child, are to be apprehended." I leave you to decide upon the evidence which I have already laid before you, whether or not Dr. Blundell's own criticisms do or do not aptly apply to Dr. Blundell's own practice, and whether the child's death, and that "bruising, laceration, and sloughing" of the maternal parts, which he apprehends in occipito-posterior positions, is more likely to be the result of the natural mechanism and uninterfered-with course of nature in such cases, or the result of that formidable list of operative procedures which he prescribes in this form of labor—and which list includes, in fact, according to his own enumeration, almost every obstetric operation except the Sigaultian and Cæsarean sections.

Let us now pause for an instant, and recapitulate one or two of the principal results at which we have arrived in the course of the preceding inquiry. We have found many estimable authors considering occipito-posterior positions to be exceedingly rare; I have shown you that, on the other hand, they are exceedingly common. They are generally represented as maintaining throughout the labor their original position with the forehead anteriorly; I have pointed out to you that, on the contrary, in twenty-nine out of thirty cases, the forehead rotates round, and ultimately emerges posteriorly. Formerly it was believed that this internal rotation of the head was never performed by the unaided natural mechanism; now we know it to be easily accomplished in every, or in almost every case, and without the slightest artificial assistance of any kind. Many excellent authors allege that, in occipito-posterior positions, the process of labor as a whole is attended with unusual difficulty and danger; but we have seen abundant evidence to prove that this is a most ungrounded fear, and that the labor—like that in occipito-anterior positions—belongs most strictly to the class of natural labors. And, lastly, we have seen no small variety of artificial and operative measures more or less strenuously recommended in the treatment of these cases in some of our best and ablest text-books; but on consulting the text-book of nature, we have further found that the peculiarities of cases of this position require, in the way of artificial or operative aid—nothing—absolutely nothing. In commenting upon these cases elsewhere, I some years ago observed, "If there is any truth whatever in statistics, we venture to say, from the data we have adduced, that such cases daily occur, and pass over unobserved in hundreds of instances in which the labor is supposed to be, and no doubt is, perfectly natural."[1] Occipito-posterior positions of the head, "require," says Naegele, "no peculiarly favorable circumstances, but these species of labors can be completed by the natural powers under the most usual proportions, in the same time, with the same expense of strength, and without greater difficulty, than when the head takes the more common position." Perhaps these remarks of Naegele are too absolute; for, in occipito-posterior positions, the occiput requires to pass in its transit, through a longer and more curved pelvic line, than in occipito-anterior positions; consequently in the Hospital statistics, drawn up for me by Dr. Barry, occipito-posterior positions seem on the whole to require a somewhat greater length of time than occipito-anterior positions. The difference, however, is so inconsiderable as not to invalidate in any, the slightest degree, what I have already stated regarding the perfect safety and facility with which unaided nature is capable of finishing the labor in this common class of cases.

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[1] See a review of the works of Rigby, Ramsbotham, and Davis, in the British and Foreign Medical Review, Oct. 1841, p. 473. The portion of the review here referred to, we have omitted, because the subject of occipito-posterior presentations is discussed at greater length, and with fuller details, in the present lecture.—(ED.)

But supposing that, in consequence of inertia of the uterus, or constitutional exhaustion, or the state of the maternal passages, or size of the head of the infant, or any other of those causes which sometimes force us to deliver by the forceps in occipito-anterior positions, we were necessitated to use the same instrumental aid in an occipito-posterior position, is there any notable difference in the mode in which we should conduct the operation in the latter case, from the mode in which we should conduct it in the former? I believe there is, in one respect, an important difference, and that there is

ONE SPECIAL RULE IN DELIVERY WITH THE FORCEPS IN OCCIPITO-POSTERIOR POSITIONS.

It is this. *In occipito-posterior positions the mechanism of the extraction of the head with the forceps should be an exact imitation of the mechanism of the expulsion of the head by nature.* In other words, I am strongly convinced that, in the artificial extraction of the head, in occipito-posterior positions, we should make the forehead rotate backwards, and the occiput forwards, according to those rules which we have seen nature following under the same conditions. For here, as elsewhere, the more perfectly we imitate her principles, the more perfect will our own practice be. If the infant's head is of such a size as to pass with comparative facility through the maternal pelvis, we may, after seizing the head, forcibly pull it down and extract it in the position in which it was originally placed, namely, with the forehead still looking anteriorly. But if the head and pelvis are more accurately fitted in size to each other, such efforts will be fruitless, unless at the expense of great and unnecessary bruising and compression of either the mother, or infant, or both. Almost all authorities, however, in midwifery, seem to recommend and practise this direct traction. But the principle of the practice amounts to this—it is as if (reverting to a previous simile) we attempted to push or pull forcibly the spindle of a screw through its corresponding nut, in a direct and straight line, instead of effecting the same object with far more ease and simplicity, by revolving the former upon and within the latter. I have now happened to be called to several cases of occipito-posterior positions, in which the forceps had been applied with the greatest adroitness and dexterity; where, subsequently, every allowable degree of force had been employed, but employed in vain to pull forth the head in its original position, with the forehead directed anteriorly; and where I have succeeded, with a tithe of the power previously used, by adding to the requisite act of extraction a simultaneous act of rotation of the head, so as to turn the occiput anteriorly, and to the right, and the forehead posteriorly, and to the left.

Indeed, I sincerely believe that such cases as those I have just spoken of, are not unfrequently regarded as unfit and improper cases for the forceps—and that, as a consequence, the child's head is opened and broken down by embryulcia—merely because the forceps have failed from the position of the head having been unattended to, or, if attended to, from the forceps not having been employed in a proper manner in the attempts at delivery made with that instrument. An obstetric friend told me that some years since he received from nature a most instructive lesson upon this point. He had fixed a pair of curved forceps upon a head lying in an occipito-posterior position, and ineffectually pulled at it in that position till he was afraid to pull more. He was resting for a pain or two, cogitating what step he should take next, and whether he should perforate the infant's head or not, when a very strong uterine contraction came on. During the contraction, the handles of the forceps were wheeled round in his hand, and the head was expelled, with the occiput under the arch of the pubes, and the concavity of his forceps turned towards the concavity of the sacrum. Nature thus strongly preached to him how he should have acted in order to assist her.

Dr. Smellie knew the propriety of rotating as well as extracting the head in these cases. He tells us, that in 1745 he applied the forceps in a case in which the large fontanelle was at "the left groin." Under the efforts which he made at direct traction, and so as to bring the forehead out anteriorly, the instrument slipped off three times, one of the blades giving way. He was still (he continues) "loth to destroy the child by opening the head," or to apply the blunt hook, and "luckily thought" of trying the forceps again, and turning the forehead backwards into the hollow of the sacrum, and by this last plan he safely delivered the infant. "This method," he ingenuously adds, "succeeding so well, gave me great joy, and was the first hint, in consequence of which I deviated from the common method of pulling forcibly along, and fixing the forceps at random upon the head. My eyes were now opened to a new field of improvement, on the method of using the forceps in this position [the occipito-posterior], as well as in all others that happen when the head presents."

Unfortunately, Smellie's observations on this point appear to have been, in general, completely overlooked by his various pupils and followers. Wallace Johnson, Denman, Hamilton, Ramsbotham, Davis, &c., tell us to extract the head with the forehead directed anteriorly, and passing under the arch of the pubes. They seem all afraid that if we acted otherwise, and rotated (as I believe we should do), the forehead backwards from the left foramen ovale to the left sacro-iliac sychondrosis, we should produce a violent and dangerous twisting of the neck of the infant. We have seen, however, that in twenty-nine out of every thirty such cases, nature produces exactly this same rotation of the head in this same class of cases, and without any risk or danger whatever to the neck of the child. But in order that our mechanism should be equally safe with hers, we must imitate nature in the process as nearly as possible, and only turn, or attempt to turn the head after it has *already* reached the pelvic floor—never when higher—and only *during* a pain, when the trunk and head are compressed together by the uterine contraction into, as it were, one mass, so that the body readily follows the movements of the head. The highest authorities, also, in the continental schools, seem to have similar fears regarding the rotation, and lay down the same plan of extracting the head in the position in which it is already placed, viz., with the occiput posteriorly. Even those obstetricians who know most perfectly the natural mechanism in such cases, totally disregard and deviate from that mechanism in their own instrumental procedures. Thus, Naegele himself advises us to bring out the forehead anteriorly, when we use the forceps in occipito-posterior positions of the head. But, then, in explanation of this apparent inconsistency, we must remember that the different varieties of Levret's forceps used by Naegele and most other continental practitioners, are so large and curved as not to enable us to rotate them and the head after they are applied, without the most imminent hazard to the vaginal structures of the mother. It is in occipito-posterior positions, above all others, that we see the superior advantage of employing a *straight* pair of short forceps, such as those of Dr. Denman or Dr. Ziegler. They enable us to rotate the head easily and safely, as I can testify to you from sufficient experience. If we employed a *curved* pair in this position, and tried to turn the head with them we should be obliged either to introduce them at first, or extract them at last, with their concavity, instead of their convexity, looking backwards, and consequently with great and unnecessary risk of contusion and laceration of the soft structures of the mother, from the projecting ends and sides of the blades.

MIGHT THE VECTIS BE SUBSTITUTED FOR THE FORCEPS IN OCCIPITO-POSTERIOR POSITIONS REQUIRING INSTRUMENTAL AID?

I put the question problematically, and must answer it problematically too, because I have no personal experience of the vectis. At the present day, when the vectis is scarcely used by a single practitioner, it does appear to us strange to be told, as we are by Dr. Denman, that by many of those men who, before him, had successively enjoyed "the chief practice" in London, the vectis was preferred to the forceps, and that several of his most experienced and eminent cotemporaries were of the same opinion. Of late I have sometimes thought that if useful at all, the vectis might be useful in occipito-posterior positions, when such cases happened, from any cause, to require instrumental assistance. In order that the rotation of the occiput forwards should be accomplished with adequate freedom, I have shown you that the head must be flexed upon the neck, in order that the impelling force sent from the uterus along the infant's spine may act in a proper line upon the occipital region of the cranium. Sometimes, however, when the head is not sufficiently flexed, and the occiput and forehead are thus placed too much upon the same plane (*when* they reach the floor of the pelvis), the momentum of the action of the uterus is received upon the middle or forepart of the head, and not upon the occiput. The mechanism of the labor thus comes to be perverted, and delay and danger may follow. In such a complication, three alternatives present themselves: 1. Rectification of the position, by depressing the occiput, or raising the forehead. 2. Rotation; and, 3. Forcible extraction of the head. With the straight forceps we may fulfil the *two* last objects—rotation and traction—but with that instrument we have no power of effecting the first, or rectification.

On the other hand, the lever or vectis, introduced over the left mastoid or occipital region of the infant's head, would enable us to fulfil *all* the three indications, that is, rectifying the position, rotating the head, and probably applying any sufficient degree of further extractive force that may be required. In most cases, the rectification and rotation—one or both—are the only conditions that are required under such circumstances, the unassisted uterus readily finishing the expulsion when the normal mechanism of the labor is so far restored. And Smellie, Lachapelle, and Montgomery, have all shown us that we may occasionally and completely effect these first two objects in such morbid cases, and when the head is at the same time transverse to the outlet, by raising and rotating the forehead with the fingers alone. If these fail, the vectis applied over and above the occiput would sometimes perhaps greatly and safely aid in the operation, the fingers being applied to the opposite temple or forehead; and we know that afterwards the fingers may be thus further made to serve as a kind of second blade to the vectis, if we wish subsequently to use that instrument with the extractive action of the forceps. If the delay, therefore, be owing to the perversion in the mechanism which I have alluded to, the vectis may be occasionally of as great or greater use than the forceps. But if the delay be the result of simple inertia of the uterus, or of any other cause in which the flexion of the head is not interfered with, or the forehead not made to descend too low—and when extraction and rotation are required *without* rectification, then I believe the forceps will certainly be the safer and preferable instrument.

The remarks which I have made apply to the action of the *straight* forceps, as compared with the action of the lever, in occipito-posterior positions. But if we compare the *curved* forceps with the vectis, or with one blade of the straight forceps used as a vectis, in this class of cases, then the result, I believe, must be still more strongly in favor of the lever. For while, with the curved forceps, more especially when the curve is great, or the instrument of a large form, it is impossible to do aught but simply extract, we may, on the contrary, with the vectis, successively or simultaneously, as circumstances require, alike rectify, rotate, and extract.

But, *above all*, it is necessary for you to remember, that it is rarely—very rarely, that you will be driven to apply any instrumental assistance in either occipito-posterior, or occipito-anterior positions of the head; for in the one class of positions, equally as in the other, nature seldom—very seldom indeed—so far fails as to require the direct aid and operative interference of art.

TREATMENT OF FACE PRESENTATIONS.[1]

(From British and Foreign Medical Review, October, 1841, p. 473.)

The history of the treatment of face presentations is an illustration of how very much may be gained by studying obstetric mechanism, and by observing how much nature can effect in particular instances.

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[1] Review of the Works of Rigby, Ramsbotham, and Davis.

Many authors of the seventeenth and first half of the eighteenth century, seem to have looked upon face cases as so preternatural and difficult, that it was generally necessary in the treatment of them, as in the treatment of a hand or shoulder presentation, to have recourse to the operation of turning. Lamotte, Cooper, Smellie, Burton, &c., all appear to have followed this practice, particularly when called in at an early stage of the labor. Giffard, in his posthumous work, published in 1734, seems to apologize for finishing two face cases with his extractor or forceps, by truly observing, that turning is attended with great difficulty where the "waters are gone off, and the uterus closely envelopes the child." Again, Perfect, Baudelocque, and others, while they deem it unnecessary to have recourse to the dangerous operation of turning, still considered active interference necessary, and have, as a common rule, recommended the face to be pushed up and the vertex brought down, contenting themselves thus with a rotation of the head of the infant, and not a rotation of its whole body. Peu speaks of the crotchet—if the face has already descended into the pelvis; and Stein recommends the conjoined use of the fillet and forceps in managing these presentations. Indeed Stein maintained that without some such interference face cases would always be cases of delay and danger. He avers that their spontaneous termination is impossible, except where the fœtus is preternaturally small, or the pelvis preternaturally large. Mesnard endeavored to prove this by geometrical measurements of the child's head and mother's pelvis, and we regret to add, that one of the most distinguished accoucheurs of the present day, M. Capuron, still upholds the same doctrine, and obstinately opposes the result of his observations with the rule and compass upon the dead and dry head and pelvis, to the result of the direct observations of all his contemporaries upon living nature; thus forgetting or despising one of the great rules of the father of the inductive philosophy—"Non arctandus est mundus ad angustias intellectûs, sed expandendus est intellectus ad mundi imaginem recipiendum qualis invenitur."

The sufficiency and success, however, of unassisted nature in face cases was long ago announced to the profession. In 1685, Paul Portal, who in this and several other matters far outstripped the age in which he lived, taught that in face presentations the child might suffer, and its face be black and swollen, but there is not, he adds, more mystery in this than in a natural or vertex presentation. "I have," he says, "delivered several women whose children came with the face foremost, and always without any great difficulty, it being only observed, that in such cases no violence must be used, but nature left to its own course, which done, there is no danger either of mother or child." In 1769, Wallace Johnson in this country, and in 1770 Deleurye in France, both publicly avowed the same opinion in regard to the safety and propriety of leaving face presentations to the unaided efforts of nature; and latterly such an accumulation of statistical proof has been collected upon this question, as must, we think, prove perfectly satisfactory to every unprejudiced mind, and carry conviction to all that prefer the force of simple facts to arguments and dogmatic theories. In 1789 Zeller published 43 cases of face presentations, all of which terminated without operative or artificial interference. In 1793 the celebrated Böer declared, that out of 80 face cases which he had himself observed and noted down, none of the mothers suffered in the slightest degree; three, or at most four, of the children were born dead, and in one case only was artificial assistance (by the forceps) deemed necessary. In 1826 Chevreuil, physician to the Maternité Hospital at Angers, stated that in 18 face cases which he had met with, all terminated naturally. The infants were of the ordinary size; 15 were born alive, and 3 were dead; but apparently, he adds, in these cases death had taken place before delivery. In 1836 Dr. Collins of Dublin, in his excellent account of the cases in the Lying-in Hospital of that city, reports 33 face cases as having occurred during his charge of the institution. All the 33 were delivered without assistance, and recovered well. Four of the children were still-born, and one of the four was acephalous (anencephalous). In one of the four the labor had been allowed to be prolonged thirty-six hours. With the two others it lasted seven or eight hours. In 5 the labor was over within an hour. "Some cases of face presentations," Dr. Collins ingeniously adds, "I am disposed to think were not noted, delivery having taken place so very speedily."

If it were necessary, we might add to the foregoing the statistics in face cases of the Prague Hospital, as given by Kilian; or those of Bourg, as reported by Pacoud; and the observations in Copenhagen, made by Bang; but it is unnecessary to accumulate further proof, when that proof all tends to the same conclusion. These last three authors conjointly report above 200 face presentations, all showing the great sufficiency of nature in such cases, and the impropriety of rash interference.

In the works of Drs. Rigby, Ramsbotham, and Davis, we find different doctrines of practice inculcated, in regard to face cases. Dr. Rigby gives no direct rules for their management, but his observations all show that he believes face cases to require no particular interference on the part of the practitioner. Dr. Ramsbotham expresses the same opinion, and shows the same repugnance to the employment of any artificial management in ordinary face cases. We have already seen some practitioners advising an attempt at rectifying the position of the head, by transforming the face into a cranial presentation. This is a line of practice too often attempted even at the present day. We believe its performance to be impossible after the face has been pushed down into the cavity of the pelvis, when the child and pelvis are both of the natural dimensions, for we believe it physically impossible to turn the fœtal head on its long or occipito-mental axis of five inches through the cavity of the pelvis, which is normally only four inches and a half. We quote one or two excellent remarks of Dr. Ramsbotham on this topic:

"It becomes a most important question whether, under a face presentation, any means should be adopted to place the child under a more favorable position. So difficult and almost impossible was the transmission of the head under this presentation at one time thought, that it was recommended that the hand should be introduced into the uterus, that the feet should be laid hold of, and that the child should be delivered by turning. This operation, performed under the most favorable circumstances, is always attended with great pain, and frequently with great danger, danger both to the mother and the child; to the mother, from the chance of injury to which her structures (particularly the uterus) are exposed; to the child, in consequence of the pressure which the funis umbilicalis must more or less experience, when the head is passing through the pelvic cavity. All these circumstances, then, being taken into consideration, the practice of changing the position of the child, under a face presentation, by turning, is now almost entirely exploded; and we rather leave the case to nature so long as we can safely trust her, than subject the woman and the infant to such dangers. But suppose, on watching the case, we find no advantage gained, no alteration in the position of the head, no advance from hour to hour, what then is to be done? We must here also act upon the same unerring principles elsewhere laid down, wait till symptoms require our interference, and then use that instrument which seems most applicable to the emergency; for it is impossible, by any counter pressure, to make a beneficial change in the situation of the head under a face presentation. We cannot cause the head to turn upon the neck so as to approximate the chin to the chest by pressure."[1]

We have said that Dr. Rigby has not thought it necessary to offer any rules for the treatment of face cases. Dr. Davis, on the other hand, has laid down rules enough for the management of these cases, but these rules are, in our opinion, in various respects highly objectionable. Indeed, if our continental neighbors judge by them of the state of British midwifery, we fear they will consider us as undoubtedly retrograding rather than advancing in this department of the healing art. In order to avoid all risk of misrepresentation we shall here quote his own words:

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[1] Principles and Practice, p. 209.

"When the face is discovered to present at the brim of the pelvis at an early period of a labor, whether before or very soon after the escape of the liquor amnii, there can, in the author's opinion, be no doubt as to the preferableness of turning to all other modes of treatment. That operation dexterously performed would, at all events, give the child a good chance of preservation of its life, whilst it would also be the means of rescuing the mother on the very brink of a great impending danger."[1]

We certainly regret to find such practical doctrines published by an author of Dr. Davis' name and standing, and, above all, by a lecturer on midwifery. The interference he thus teaches is, as we have already seen from the statistical data, useless and unnecessary. We wish this were all we could say against it. We must add our solemn conviction that the practice is not only superfluous, but that it is fraught with great danger to both the child and mother. In original footling cases, about one in every four is lost from compression upon the cord, and other causes which it is unnecessary at present to inquire into. Dr. Davis advises us to change artificially the face into a footling case, and if so, we could not certainly expect to save more than in the natural footling cases. We believe we should save fewer. How strongly, then, does this degree of danger to the child contrast with the degree of danger to which it was found subjected in Böer's cases; when leaving the case to nature, not more than 1 in 20 was lost. In the exercise of our profession we have too often to blame ourselves for errors of omission. Here we think human life is manifestly endangered by an error of commission, if meddlesome and unnecessary interference may bear that name. But the practice affects not the safety and life merely of the child: it affects also the safety and life of the mother. Turning the fœtus in utero is an operation which, as Dr. Churchill calculates from various data, proves fatal to the mother in about 1 in every 15 cases in which it is had recourse to. Are we justified in subjecting any woman to even a fraction of this danger in rectifying or attempting to rectify a presentation, any supposed difficulties con-

[1] Elements of Obstetric Medicine, p. 659.

nected with which nature every day shows us she can easily and safely overcome without any assistance on our part?

Let us not be misunderstood. We by no means inculcate that the practitioner is to remain perfectly apathetic because the presentation is discovered to be a face one. We would rather wish to guard him against the opposite and more dangerous extreme of direct and dangerous interference with the head or body of the child itself. In all such cases we firmly believe, as a general rule, that the *infant* or its position should not be interfered with. The only exception is the rare case where the head is too large for the face to enter the brim at all. We shall not stop to inquire whether the conical-shaped head of the child, when it passes through the pelvis with the face or base of the cone first, is much or in any degree larger than when the vertex or apex of the cone forms its presenting part. But even admitting, what we conceive to be true, that the head when it passes down with the face presenting is slightly larger and more unyielding, and consequently requires a somewhat larger space to pass through than in vertex or cranial cases,—still we would strongly repudiate any attempt to manage such cases by intermeddling with the child, while we deem it proper and justifiable to use every simple and safe expedient within our power, which may act upon the *mother* so as to render either the uterine action more decisive, or the parturient passages more dilated, or at least more easily dilatable. For this purpose we avert more carefully all causes that could interfere deleteriously with the functions of the uterus, and at the same time, we also have recourse to bloodletting and other measures calculated to fulfil this indication, earlier and more decidedly in face than in cranial presentations, when there is even any tendency to delay; the membranes should be allowed to remain as long entire as possible; the rectum and bladder kept empty; and if, as not unfrequently happens, a fold of the cervix uteri happens to be pushed down before the presenting part, it should be very gently relieved and pushed aside; and the perineum in this, as in all malpresentations of the head, must be guarded with especial care under the unusual degree of distension to which it is subjected.

Cases of delay and difficulty and danger will happen with presentations of the face, as with presentations of the cranium, and from the very same causes which in cranial cases may render the uterine powers insufficient to complete the expulsion of the child. Under such circumstances our interference or non-interference must be regulated by the same principles as in natural or cranial presentations. We will only add one remark: If in a laborious face case we use the forceps or the vectis as an extracting instrument, we must hold it ever in our recollection to direct at the outlet the chin and not the forehead under the arch of the pubis. If the chin were turned backwards instead of forwards, all our physical efforts at the extraction of the head would be utterly fruitless.

NEW FORM OF OBSTRUCTION IN HEAD PRESENTATIONS,[1]

FROM DISPLACEMENT OF THE ARM.

(From Edinburgh Monthly Journal of Medical Science, April, 1850, p. 389.)

Dr. Simpson stated that he considered the case to which he wished to direct the attention of the Society an important one, because the peculiar obstruction in head presentations which it illustrated was, so far as he knew, hitherto undescribed. The form of obstruction consisted in one of the arms of the infant being displaced backwards across the neck or occipital region; or more properly speaking, it was the forearm that was thus thrown across the back of the head and neck, the arm being thrown upwards in a line with the body, in order to admit of this malposition of the forearm. In this abnormal position the displaced elbow and forearm of the child first increased greatly the dimensions of the basis of the head; and secondly, these same parts formed a kind of projecting obstruction, which readily hitched and caught upon the brim of the pelvis, thus preventing the descent of the head. But the effects might be better judged of by detailing the case itself.

The patient had previously borne nine children. All the labors had been easy, and she had frequently been delivered so speedily, that the labor was over before the medical attendant could reach the house. In her last and tenth labor, pains came on about four in the afternoon, and the os uteri was not completely opened up till about ten o'clock. About an hour before, the membranes ruptured. At six next morning, Dr. S. received a note from her medical attendant, Mr. Carmichael, asking him to see her, as the head had remained in the same position at the brim for several hours, the uterine contractions were becoming weak, and the woman herself exhausted. On placing the patient deeply under the influence of chloroform, in order to make a complete examination, Dr. S. found the maternal passages perfectly relaxed and open, and the head of the child to be by no means large, and not even entirely filling up the brim. The vertex presented, and the face was directed towards the left sacro-iliac synchondrosis—a rare enough position, but one not in any degree calculated to account for the delay. On passing the examining fingers farther upwards, in order to trace any possible obstruction, he touched a projecting body (the elbow) beyond the left ear of the child; and on now making the examination more carefully, he traced this body backwards across the neck of the infant, and found it to consist of the left forearm of the child thrown back posteriorly behind the head. He then brought the hand downwards and forwards, believing that if it were converted into a head and arm presentation, the case might terminate without further interference. During the next half hour, however, the pains, which had for some time been weak, had little effect in forwarding the presenting parts, and as the pulsation of the child's heart had now sunk as low as 78 beats in the minute, Dr. S., in order to preserve the child, again chloroformed the patient deeply, and delivered the child by podalic turning. The mother made a speedy recovery. The child soon cried strongly, and went on quite well. Its left arm was for a day or two after delivery easily thrown into the position described. The occipito-frontal circumference of the head was afterwards measured by Mr. Carmichael, and found to be 14½ inches; when the arm was placed in its anomalous position, the same circumference measured 15½ inches. The circumference of the shoulders was 13½ inches. The child was of about the usual size, and weighed 7½ lbs.

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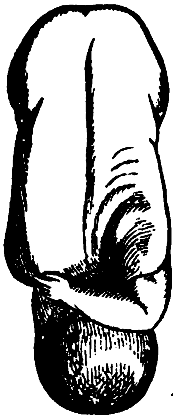
[1] Extracted from Proceedings of Edinburgh Obstetric Society, February 13, 1850.

Fig. 24.



The *treatment* of such a cause of obstruction, when it was once recognized, should probably consist in bringing the hand downward and forward over the side of the head, so as to convert the case into one of simple presentation of the head and arm. Perhaps it might occasionally be possible to push the elbow forwards in the direction of the lower end of the sternum, and thus draw back the displaced arm into its normal position in front of the chest. If either of these measures proved impossible, or failed, then the podalic version would be required.[1]

The *Diagnosis* of the case was the most difficult point in its management. And in this, as in other complications—as detention from intra-uterine hydrocephalus, &c.—the assistance of anæsthetics in midwifery was invaluable as a means of enabling the accoucheur to make a far more searching and successful manual examination and diagnosis, in cases of obstructed labor, than it was possible to do when the patient was awake and incapable of bearing with steadiness, and without unnecessary suffering, the introduction of the hand for the purpose.

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[1] Dr. Simpson has latterly seen this cause of difficulty overcome without any artificial aid. He has had various communications showing the comparative frequency of the complication.—(Ed.)

When a labor, as in the preceding case, notwithstanding steady and continued uterine contractions, became morbidly prolonged in a mother who had previously borne easily a large family, there was every probability of obstruction of some kind on the part of the infant. Dr. S. had seen two such cases, where the detention was the result of intra-uterine hydrocephalus. In the present instance it was the result of the malposition of the arm. Some time since he had mentioned to the Society two cases of tedious labor, which, several years ago, he had seen with Dr. Ziegler: in both, the head, despite of strong pains, remained in the pelvic brim without descending; in both, the head was evidently not disproportionately large to the maternal passages; in both, some point of the shoulder or arm could be touched by the finger on examination; and, perhaps, if the examination could have been made more complete by the use of ether or chloroform, which were then unknown, a malposition of the arm, similar to the one above described, might have been detected. Various cases are recorded of obstructed labor, with the head, as usual, presenting, in mothers who had previously had natural deliveries, and where the forceps failed to extract the child, and where even extraction after craniotomy was difficult. Some of these cases were in all probability instances of obstruction from dorsal malposition of the arm, or rather of the forearm. The late Dr. Campbell, shortly before his death, had told him of a case where there was no pelvic or other deformity on the part of the mother, no want of uterine contraction, and no disproportionate size of the head of the child, and yet he and others had entirely failed in extracting the detained infant by the forceps, and at last were obliged to open its head.[1]

When looked for, Dr. S. believed, therefore, that the dorsal malposition of the forearm would be found a more frequent cause of obstructed labor than the total silence of obstetric authors on the subject might, *à priori*, lead us to suppose. Further, he considered the present case as interesting, not only as an instance of an undescribed species of malposition and obstruction, but probably as one of a new *class* of malpositions as yet unrecognized in any of our accounts of the mechanism of labor, and the malpositions of the child. It would probably be found that other degrees and forms of malposition of the arm might occasionally lead to the same result.

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[1] For a full description of this case by Dr. Cumming, see Edinburgh Monthly Journal of Medical Science, April, 1850, p. 389.—(Ed.)

At the same meeting Dr. Simpson observed, that sometimes, in women who had previously borne a large family, a cause of obstruction might exist in a late labor, not on the part of the child, as was generally the fact, but on the part of the uterus. Last summer he saw, with Dr. Skae, a case of this kind, where the source of detention was a firm and contracted circular band of the uterus around the site of the neck of the child A patient who had had a large family and easy labors, complained of a feeling of tightness across the lower part of the abdomen, during the last three months of her last gestation. Dilatation of the os uteri did not seem actually to begin until the patient had been suffering at least 48 hours from labor pains. Nor could the head be detected as the presenting part for nearly 24 hours longer. For several hours more, the patient continued suffering apparently from severe labor pains, during which the head descended to the brim, but never entered it. Œdema of the os uteri now showed itself, and the strength of the patient began to flag.

Dr. Skae felt satisfied that the obstruction to labor must be of some unusual kind, for the pelvis was large and well formed, and the size, firmness, and configuration of the child's head opposed the idea of hydrocephalus. He therefore sent for Dr. Simpson, and after the patient was placed under a full dose of chloroform, the cause of obstruction was found to depend on the presence of a rigid stricture, situated in the lower third of the uterus, upon which rested the shoulders of the fœtus. After administering 120 drops of Sol. Mur. Morphiæ, and keeping her pretty deeply under the influence of chloroform for two hours, it was found that no material relaxation of the stricture had taken place, to admit of turning without endangering the integrity of the uterine walls. It was feared that ovariotomy might be ultimately necessary; but employment of the long forceps was first resolved upon, notwithstanding the unusual difficulty of applying them so high up, as necessarily to require their being locked within the vagina. Dr. Simpson, however, succeeded readily in applying them, and accomplished delivery in some fifteen or twenty minutes, by dragging the shoulders of the fœtus through the stricture. In this case both mother and child did well, the recovery being unattended by any unfavorable symptoms. See a fuller account by Dr. Skae, in Edinburgh Monthly Journal for April, 1850, p. 390.—(Ed.)

ON THE MODE OF APPLICATION OF THE LONG FORCEPS.[1]

(From Edinburgh Monthly Journal of Medical Science, September, 1848, p. 193.)

When the head of the child becomes fixed in the brim of the pelvis, and the powers of the uterus fail in propelling it through that aperture, one of two modes of instrumental delivery is usually resorted to—namely, either, 1. Diminution of the infant's head by perforation and craniotomy; or, 2. Extraction of it by the long forceps.

Craniotomy is the preferable operation where the child is dead, and where the pelvic brim is much contracted, or the child's head very much above the average size. But besides being always necessarily fatal to the child, craniotomy is an operation by no means without danger to the mother;—one in every five mothers, according to

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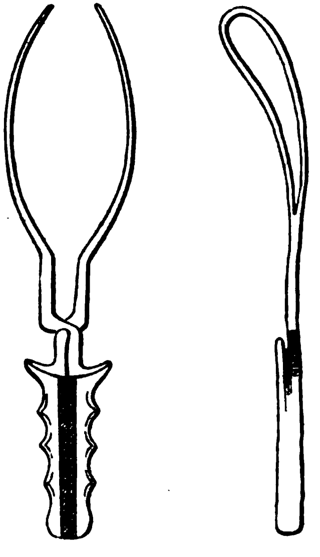
[1] Proceedings of Edinburgh Obstetric Society, May 10, 1848.

Dr. Churchill,[1] dying, upon whom it is performed. And in most schools of midwifery, it is now, consequently, much more rarely practised than heretofore. Still many authors and practitioners, as Drs. Collins, Johnson, Lee, &c., appear to resort to it in all cases in which the bulk of the head is unable to pass the brim, and reject entirely the other alternative of the long forceps.

The long forceps afford in these cases the best chance of life to the child, and when used with proper views of their mechanism and mode of application, their employment, as is often seen in Edinburgh practice, is by no means so difficult and dangerous as some authorities seem to believe. Misapprehension on these points is perhaps the cause why they are not more frequently used. They differ from the short forceps in some points of construction, but more particularly in regard to their mode of application and working. They differ for example in their length; in the shanks being parallel for some distance beyond the lock, an indispensable point in order to prevent their injuring the outlet; in their blades being curved; and in the part intended to embrace the head being sufficiently long and large. The instrument which Dr. Simpson has frequently and successfully used during the last five or six years, has some additional peculiarities, as is seen in the woodcut. The blades are the same as Dr. F. Ramsbotham's, but scarcely so much curved. The lock is Smellie's, but with knees or projections above it of such size as to prevent the blades readily unlocking in the intervals between the pains, thus giving it the fixed character of the locks of Levret and Brunninghausen's instruments, without their complexity. The joints are made so loose as to allow of their lateral motion and overlapping to a very considerable degree, thus facilitating their introduction and application. And, lastly, the handle is that used by Naegele and other German accoucheurs, viz., with transverse knees or rests below the lock for one or two of the first fingers of the right hand to drag by, the long forceps being only properly used as an instrument of traction, not of compression. In addition, the handles are grooved and marked on the anterior side, to distinguish that

Fig. 25.



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[1] Operative Midwifery, p. 173.

from the other side when the blades are within the pelvis. The following are some of the dimensions of Dr. Simpson's forceps: Length of the entire instrument, 13¾ inches; length of handle, including the lock, 5¼ inches; length of shank, from the lock to the commencement of the curve of the blade, 2¼ inches; length of the blade, 6¼ inches; extreme breadth of blade at 1½ inches distance from the point of the instrument, 1¾ inches; length of the fenestra, 4½ inches; extreme breadth of the fenestra, 13 lines; breadth of the groove of the lock, ⅜ inch; thickness of the shank to fit in the groove of the lock, 2/8 inch; extreme distance between the blades at three inches distance from the point of the forceps when locked, 3 inches; distance between the points of the two blades when locked, 1 inch.

In their modes of application, the long forceps differ from the short. The short forceps are applied always to the lateral surfaces of the child's head, in whatever position the head may be. Generally, the long diameter of the head lies in the right diagonal diameter of the pelvis; and, consequently, the short forceps are placed in the opposite or left diagonal diameter; or, in other words, at right angles to the long diameter of the head. The mode in which the long forceps ought to be applied, and are really applied in practice, has given rise to considerable difference of opinion.

If the long forceps are ever, for inertia, hemorrhage, or other such complications, in any case applied while the head is passing through the brim, and the brim and head are natural in size, the instrument may be perhaps applied, like the short forceps, directly to the lateral surfaces of the child's head. But the common reason for the employment of the long forceps is morbid contraction of the brim of the pelvis in its most general form, and from its most general cause, viz., in the conjugate or antero-posterior diameter, from projection forward of the promontory of the sacrum. How are the long forceps applied when used in this, the case in which they are most generally had recourse to in practice? It is first requisite to state, that under this complication the child's head is found situated in the brim, with its long or fronto-occipital diameter lying in the transverse diameter of the brim, or with the forehead looking to one ilium, and the occiput looking to the other. In other words, the long diameter of the head is not placed, as usual, in the right diagonal diameter of the brim, but more in its transverse; for where the promontory of the sacrum forms a morbid projection, the transverse forms the longest diameter of the brim, and, consequently, the one in which the child's head comes to be placed by the uterine efforts. The face or forehead looking to the ilium, and the occiput to the other ilium, the lateral surfaces of the child's head come to be compressed between the protruding sacral promontory and the interior of the symphysis pubis. Now, in seizing the head in this case some authors aver, that—

1st. *The blades of the long forceps are placed, as in applying the short forceps, on the lateral or aural surfaces of the child's head,* and consequently with one blade in front of the sacral promontory, and the other behind the symphysis pubis. Burns,[1] Dewees,[2] &c., speak of thus applying the long forceps in the conjugate diameter of the brim; and Dr. Churchill[3] has published a wood-cut representing this as the actual method of their application in practice. But its application in this position is impossible in the very cases in which the long forceps are generally required, viz., where the conjugate diameter is contracted, for there is not room for the additional thickness of the blades of the instrument; if applied, they add to the thickness of the head in that one diameter and place in which it is already too thick and large; their pressure would greatly endanger the urethra and bladder in front, and the soft structures placed over the promontory of the sacrum behind; and they could not thus be placed in the axis of the brim in consequence of the pressure of the perineum upon the instrument below. Other authors aver, that—

2dly. *The blades of the long forceps should be placed over the occiput and forehead or face of the child, and consequently in the transverse diameter of the brim.* This is the view of their mode of application taken by Deleurye,[4] Davis,[5] &c. &c., and approaches much nearer the reality than the former opinion; but that it is not strictly true, is shown by the marking of the place of application of the blades of the instrument after the child is born, and by a more attentive consideration of the mechanism of such labors. One blade is found to have been placed behind one ear, and the point of the other to have pressed upon the side of the forehead, temple, or region of the eye; but these would not be the places of the markings of the blades if they were applied in the transverse diameter, upon a head placed directly transverse. Dr. Ramsbotham[6] has published a beautiful plate of the mode of application of the long forceps, and has given an excellent chapter on the subject in his work on Midwifery. He correctly represents in the plate the anterior blade as placed upon the side of the forehead and eyebrow; but in order to give this view with the forceps placed in the transverse diameter of the brim, he has been obliged to represent the face as turned backwards, whilst in reality, in morbid contractions of the conjugate diameter of the brim, it is actually turned laterally; and he places the long diameter of the blades of the forceps so as to traverse the right oblique, instead of the left oblique pelvic diameter.

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[1] Burns' Midwifery, p. 488.

[2] Dewees' System of Midwifery, p. 315.

[3] Churchill's Midwifery, fig. 76, p. 298.

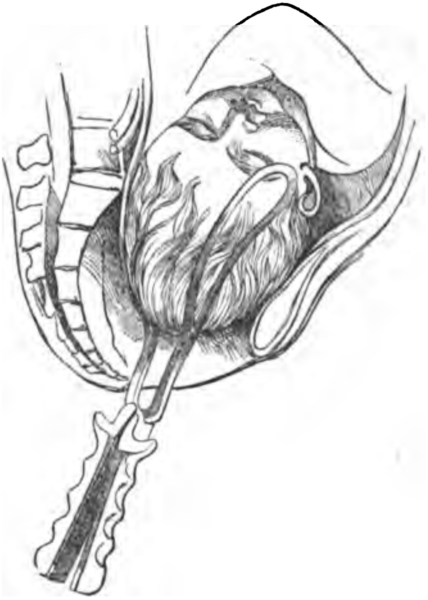
[4] Deleurye, Traité des Accouchemens, p. 340.

[5] Davis's Operative Midwifery, p. 242.

[6] Ramsbotham's Obstetric Medicine, plate lvi.

3dly. *The blades of the long forceps should, I believe, be placed obliquely upon the child's head—one, the posterior, over the side of the occiput; and the other, or anterior, over the side of the brow or temple; and consequently they should be generally situated somewhat in the oblique diameter of the brim*, as in the wood-cut. The markings on the child's head after birth always show this mode of application of the instrument; when properly applied upon the mother, and when their situation relative to the pelvis is examined, they are found to have assumed this position; and in experiments with the instrument, when the head of a dead child is fixed in a pelvis with a contracted brim, this is the position and relation which the instrument will be seen to assume with relation to the infantile head and maternal pelvis. Besides, in thus placing the instrument, while we incur less danger of injuring the urethra and other important parts, we place the blades of the instrument in exactly those parts of the pelvic circle where there is least pressure, and consequently most room for them. It is apparently in consequence of misconception on this point, that some authors have come to prefer the use of the perforator to that of the long forceps. Dr. Collins, for example, argues that when the head is detained in the pelvic brim, the brim "measuring little more than three inches from pubis to sacrum," there cannot possibly be space for the long forceps even were the bones denuded, seeing that the blades of the smallest-sized forceps used in Britain, even when completely closed, measure from 3⅛ to 3½. "How," he adds, "is it possible with the forceps to drag a child through a pelvis where there is not space, except by force, to introduce, as is commonly said, a straw, or where the smallest flexible catheter cannot be passed in some instances into the bladder?" These and such opinions proceed on the erroneous idea, that the long forceps are to be applied, within the pelvis, at the parts, or in the diameter in which the pelvis is *most* contracted, and they

Fig. 26.



suppose that the head, when fixed in the pelvic brim, fills completely the *whole* circle of the brim. The usual shape of the morbidly contracted pelvic brim is cordate, or rather elliptico-cordate; but the child's head is not of this shape, it is ovoid, and consequently, when applied to the cordate brim, leaves unoccupied spaces. The most unoccupied spaces, before and behind, are at the extremities of the oblique diameters of the brim, where sufficient room is left for the passage of the blades of the forceps, and in these points they are passed when properly applied.

We are led to believe that a practitioner will have little need of possessing a pair of short forceps, if furnished with such a pair of long ones as those described in the preceding paper. We have repeatedly heard Dr. Simpson assert, that the more frequently he applied the forceps, the more firmly he became convinced that one pair of proper form and workmanship would answer for all forceps cases; and that he believed an obstetrician much more likely to become dexterous in manipulating, who confined himself to one pair of forceps, and studied that form carefully, than if repeatedly changing the form of instrument, according as the fœtal head was situated high or low in the pelvis. The curve of Dr. Simpson's forceps is so slight as to keep them available for altering an occipito-posterior position of the head into occipito-anterior, as described at p. 428 of this volume. The curve of the German and French instruments, and of many English forms, is generally so great as to prevent them entirely from being used for this rotation of the head.—(Ed.)

THE AIR-TRACTOR[1]

AS A SUBSTITUTE FOR THE FORCEPS IN TEDIOUS LABORS.

(From Edinburgh Monthly Journal of Medical Science, Feb. 1849, p. 556.)

Cases ever and anon occur of morbid protraction and delay in the second stage of labor, with the head of the child sunk down into the pelvic cavity, or resting upon the perineum; with the caput succedaneum enlarging more and more under the pressure and prolonged uterine contractions, and yet these contractions producing little or no effect upon the actual advancement of the infant; and with local and general symptoms threatening to supervene, and showing the propriety, if not the necessity, of the patient being relieved, and delivery being completed.

In such tedious and trying cases, if there were space for us to pass our hands between the soft parts and head, so as to seize with them, and drag upon the head, we could curtail the patient's sufferings. There is not room for this. Our hands are too thick for the purpose. As a substitute, accoucheurs under such circumstances introduce a thin metallic hand, so to speak, on each side of the child's head—viz., the forceps—which exactly, like a couple of slender hands, take hold of the head, and enable us to apply, when necessary, extractive force to it. One objection which has been often urged against the use of the forceps (though it is an objection against their abuse rather than their use) is this, that the instrument, in being introduced between the maternal passages and head, is apt to injure these passages, and to contuse and even lacerate them during their working; that they are thus liable to inflict local damage on the mother at the very same time that they shorten the term of her sufferings and save her infant.

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[1] Extracted from Proceedings of Edinburgh Obstetric Society, Dec. 20, 1848.

Is there any other mechanical power which it is possible for us to apply to the infant's head in order to seize and move it forward, which would not be liable to the same objection of danger to the mother?

In such tedious cases as seem to require the use of the short forceps, the idea has perhaps crossed the minds of most accoucheurs, that if they could get hold of the *exposed* portion of the scalp of the child, or of the skin forming the caput succedaneum, and could possibly pull by *this* hold, they might thus expedite the process of delivery, and abridge the sufferings of their patients. The spherical form of the infant's head, and the intimate mode in which the scalp is spread over the arch of the cranium, prevent the possibility of taking any such hold by the fingers alone. By means of a small suctorial disk, the shell of the patella or limpet, which is so common upon our shores, fixes itself with great force to the stones or rocks on which it is placed. Dibranchiate cephalopods—as the common cuttle-fish—fix their arms, by similar suctorial disks, so firmly to different surfaces, that their arms themselves will often tear before the suckers with which they are attached will give way. If we could fix upon the *exposed* portion of the fœtal scalp the suctorial disk of a limpet or cuttle-fish with the usual force with which they adhere to the sea-rocks, &c., to which they are attached, we should have, in many cases, a power sufficient to enable us to apply by them the necessary amount of extractive force. The disks of the limpet and of the cuttle-fish attach themselves firmly to the surfaces to which they adhere, by being formed so as to act upon the principle of the common sucker used by the schoolboy to lift stones, &c.—viz., by removing, or rarifying as far as possible, the air placed between the attaching and attached bodies, and thus taking advantage of the great power exercised by pressure of the atmosphere upon the surfaces of solids. This pressure is, as is well known to all, equal to nearly fifteen pounds upon the square inch when the subjacent vacuum is perfect; or, in other words, it would require a force equal to fifteen pounds for every square inch attached, to effect the separation of surfaces thus united. The limpet and cuttle-fish have the surface of the acetabula or disks with which they fix themselves so strongly upon the rocks, bedewed with a thick mucous secretion; after placing the surface of the disk upon the part to which they are to attach themselves, they, by a muscular movement, raise the centre of the disk so as to produce a more or less perfect vacuum; and the cuttle-fish has a central body in the middle of each disk, which it draws up and uses for this purpose, exactly on the principle of the piston of a syringe.

Such an arrangement and apparatus may be imitated by art; and when rendered more perfect and complete, may perhaps give us a simpler and safer obstetric power for some cases than even the forceps. In one protracted case which Dr. Simpson described, he had lately made use of this power to extract the child. When applied, the head was still high up in the pelvic cavity, and the instrument easily afforded such a hold of the head as to allow it to be slowly dragged forwards and extracted. During this extraction the instrument required to be reapplied once or twice. Dr. Duncan and Mr. Dickson were present at the delivery.

The instrument used in this case was very rude and imperfect. It consisted of a common metallic vaginal speculum, fitted with a piston, and with the edge of the trumpet-shaped concave disk at its outer or broader end covered with leather. This broader and leathered end was coated with lard, and applied to the head of the child; and then an exhausting effect produced by moving the piston forwards. The apparatus would admit of much improvement and simplification, as by its mouth being made expansible, and capable of altering in shape, instead of metallic and fixed; by its inner edge being coated, as in atmospheric railways, by a thin layer or cushion of air enclosed in caoutchouc; by the exhausting apparatus being valved and more perfect, &c., &c. But if the air-tractor could not be made both simple and satisfactory in its application, it would not replace the forceps; and more experience would be required to decide whether it had any title to do so.

If the instrument, when properly constructed, should be found to succeed, it would be still more advantageous in replacing the long, than in replacing the short forceps. In the case in which it was used, the head was at the height for which long forceps are usually required. If a suctorial tractor should answer in some long forceps cases, and enable us to drag with sufficient force upon the exposed portion of the scalp, it should save the danger dreaded by many, of wounding the uterus by introducing and working the blades of so long an instrument as the long forceps, high up in the neck and cavity of the uterus itself.

Presentations of the breech sometimes require instrumental assistance. The hook passed over the flexure of the thigh is dangerous, and very apt to injure. The forceps, as recommended in these presentations by some authorities, are often inapplicable and inefficient. Perhaps the air-tractor may afford us a new and sufficient instrumental force for the management of some of these cases. Its use would be simpler and safer than any of the other methods proposed.

Dr. Simpson further observed, that he was not aware that any one had applied practically this obstetric means before it was employed in the case detailed to the Society. But the idea of using such a power had been long ago proposed by a gentleman, for whose works and talents they all entertained the utmost respect, Dr. Arnott of London. In his admirable work on Physics, Dr. Arnott alludes to the subject in the following words: "The forceps," says he, "to be well and safely used, require address, which even the naturally dexterous man cannot possess without a certain degree of continued practical familiarity with it: and, except in large towns, a man must be unfortunate in his practice who often requires it; hence the really small number of persons who use it well. A tractor of three inches in diameter would act upon any body, to lift or draw it, with a force of about a hundred pounds—with more, therefore, than is ever required or allowable in obstetric practice. In lifting a stone, the tractor does not act as if it were glued or nailed to the stone, but merely bears or takes off the atmospheric pressure from one part, and allows the pressure on the opposite side, not then counterbalanced, to push the stone in the direction of the tractor; so when placed upon the child's head, it would not pull by the skin, in the manner of a very strong adhesive plaster applied there, as uninformed persons would be apt to suppose; but by taking off a certain atmospheric pressure from the part of the head on which it rested, it would allow the pressure on the other side or behind to urge the head forward on its way. Of course the pressure in such a case would not operate on the head directly, but through the intervening parietes and contents of the abdomen. It would be preferable to have a gentle and diffused action of the tractor over a large space, rather than an intense action on a small space, and therefore a tractor for the purpose now contemplated should not be very small, and should have a little air underneath it in a slight depression or cavity at its centre. The forceps must be more effective than the tractor for rectifying malpositions of the head, and diminishing its transverse diameter; but the tractor will answer both these purposes in a greater degree than might at first be expected."[1]

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[1] Elements of Physics, p. 636.

The inner cup might be round, oval, &c., and varying in form and size. The outer caoutchouc cup would admit of much further improvement. A double cup of caoutchouc seemed to render the instrument stronger. Of course, with all this, the vacuum, however great, was still always more or less imperfect.

The air-tractor seemed to possess various advantages over the forceps. It was far less dangerous to the mother, as it was attached to the exposed part only of the scalp of the infant; the forceps were required to be passed high up between the head and maternal passages, and in incautious and inexperienced hands were apt to injure one or both. The materials of the air-tractor, caoutchouc and sponge, were safer to the mother and child than the material of the forceps (steel). The forceps always took up a certain amount of space between the head and passages; the air-tractor did not. The air-tractor was greatly less in size, and consequently far more portable. It could be applied with sufficient firmness and power to enable us to rotate the head, or change its *position;* as from, for example, an occipito-posterior to an occipito-anterior position, the form of rotation most frequently required in practice. It probably could be applied also to change the *presentation*—which the forceps could not effect—as, for instance, to bring down the occiput, when fixed there, in frontal presentations. Perhaps it may be found as useful, or more so, when the head is at the brim as when it is at the outlet. It may be made so as to fix upon the breech—in cases in which the forceps cannot be very readily or safely used. If found perfectly easy of application, it may enable the accoucheur, by adding a few pounds to the strength of each pain, to bring to a safer and speedier termination cases that would otherwise go on tediously, hour after hour, and in which we should still not choose to use so formidable an instrument as the forceps; for it is to be recollected that the danger of parturition to both mother and child increases in a ratio progressive with its duration. In cases of inertia of the uterus—the most common cases for the short forceps—it will probably be found specially applicable. And in such instances it is surely better to extract the child by a safe force, thus applied *ab anteriori*, than to effect its expulsion by the ergot of rye—which produces its result by forcing the uterus to push and press its parietes with renewed power and violence against the opposing body of the fœtus.

In conclusion, Dr. Simpson stated that he had now used the air-tractor which he had constructed, in several cases of labor, and with results answering his best expectations. But it doubtless admitted of much further improvement in construction, in mode of application, in working, and other details.

The two preceding abstracts of communications on the air-tractor, read before the Medico-Chirurgical and Obstetric Societies, express the same views, but somewhat differently illustrated in the detail. We have chosen to reprint them complete, rather than omit any portions which might be regarded as repetitions. We observe that Scanzoni, in his Lehrbuch der Geburtshilfe, alludes to some experiments before the Medico-Physical Society of Würzburg, in which M. Schierlinger demonstrated that the air-tractor would not lift more than 15 or 20 lbs. without separating from the surface to which it had been applied, and that this force must be totally ineffectual for the purpose of extracting a fœtal head detained at the outlet of the pelvis.

We can only understand how this should be the case with an imperfect instrument, as we have on many occasions seen a tractor of the size described by Dr. Simpson, bear 70 or 80 lbs. without pulling off.

The chief objection in the practical use of the tractor is doubtless in its application, and not in its power of traction; the large size of the caoutchouc cup rendering difficult its introduction within the maternal passages. To this may be added the difficulty of keeping the valves in working order.

Dr. Simpson however holds, and we believe correctly, that if ingenuity could suggest any form of tractor which, umbrella-like, could be folded into little space for introduction, and afterwards expanded over the scalp, and then exhausted by the attached piston, it might supersede the forceps in many cases.

Mr. Young, the instrument-maker, has shown to us a recent letter from an English practitioner, who speaks in the highest terms of the value of the tractor in his obstetric practice. Indeed, although in its present form the air-tractor may not be so generally applicable in obstetric practice, as to lead to its common adoption amongst practitioners; yet when we revert to the history of some of our most useful obstetric instruments (contrast, for example, the rude form of the early forceps with their present improved construction), we have reason to hope, that the tractor may, at some future time, be so far improved, as to be easily applied and used.—(Ed.)

ON TURNING,

AS AN ALTERNATIVE FOR CRANIOTOMY AND THE LONG FORCEPS, IN DEFORMITY OF THE BRIM OF THE PELVIS, ETC.

Dr. Simpson, presenting the following papers on this subject in 1850, in a collected form to his pupils, prefaced them with the following remarks:

"During a residence of a few weeks on the west coast of Scotland in the autumn of 1847, I wrote the following imperfect Memoir; and it was subsequently published in different numbers of the 'Provincial Medical Journal.' The date of absence from my duties in Edinburgh expired before I had time to finish the Essay; and hitherto, other avocations and inquiries have prevented me from redirecting my attention to the subject. Four additional sections or chapters were intended to be added—namely, one on the mechanism of the proposed mode of delivery as influencing the steps of the practice itself; a second, on the cases of obstructed labor chiefly adapted for delivery by turning; a third, on the best mode of conducting the operation in these special cases; and a fourth, or last, upon the history of the practice in ancient and in modern times.

"The Essay is placed in its present unfinished form in the hands of my pupils, principally with the view of saving time in the discussion of the subject in the class-room; and the preceding notice is offered to them as some apology for its many deficiencies."

I.

SECT. I.—ILLUSTRATIVE CASE, AND INTRODUCTORY REMARKS.[1]

At a meeting of the Obstetric Society of Edinburgh, held on the 20th of January, 1847, I had an opportunity of showing to the members a large infant, extracted on the preceding evening by the operation of turning, through a pelvis, the brim of which was greatly contracted. The following particulars were at the same time stated regarding the history of the case.

CASE I.—The mother was very lame, with the lumbar vertebræ much distorted, and had been twice pregnant. Her first labor had been extremely protracted, in consequence of the promontory of the sacrum projecting forwards and downwards, so as to diminish much the conjugate diameter of the brim. After being several days in labor, symptoms requiring interference supervened, and Mr. Figg, her medical attendant, availed himself of the able advice of my friends Drs. Malcolm and Marr. These gentlemen considered it proper to ascertain whether the child might not be capable of being delivered, without the dire necessity of embryulcio. After a long and cautious trial, however, of the long forceps, they found it impossible to advance the head with them, and were at last driven to have recourse to craniotomy. Even after the cranium was perforated, and freely broken down, it was found a matter of much time and difficulty to drag with the crotchet the collapsed head of the fœtus through the distorted brim, and the patient made a very long and protracted convalescence.

She was earnestly advised to have premature labor induced, provided she again fell in the family way; but Mr. Figg was not made aware of her state till she was near the end of the ninth month of her second pregnancy, and when it was too late to justify interference. Parturition supervened in a few days. I saw her with Mr. Figg on the afternoon of the 19th of January, a few hours after the first labor-pains had commenced. The os uteri was tolerably well dilated, the membranes still entire; and the head, which was difficult to reach, was found high and mobile above the brim of the pelvis; a pulsating loop of the umbilical cord was prolapsed before it. During the course of the few following hours, no advance being made, I proceeded shortly after nine in the evening, for the labor had commenced in the forenoon, to make the mother inhale the vapor of sulphuric ether, and to extract the child, as I had previously determined to do, by the operation of turning. The os uteri was so dilated, as not to offer any impediment to the introduction of the hand; the head was pushed aside, and a knee seized with great ease. With this hold the infant was readily turned, and its extremities and trunk drawn down, but the extraction of the head through the distorted brim was a more difficult task. After the arms were brought down, very great exertion in the direction of the axis of the brim was required before the head was extracted; still not above two or three minutes elapsed from the first introduction of the hand till the complete extraction of the infant. It gasped several times after it was born, but full respiration could not be established. Its head was compressed laterally, the left parietal region flattened, and the anterior part of the right parietal bone deeply indented by the pressure to which it had been subjected against the projecting promontory of the sacrum. The transverse or bi-temporal diameter of the head, at the seat of the indentation, was found, on careful admeasurement, and when held compressed by the fingers, not to be above two and a half inches. Hence the conjugate diameter of the brim did not, in any probability, exceed this. The infant, a female, was large, and above the usual size; it weighed exactly eight pounds, the average weight of the female infant at birth being about six pounds and three quarters. In consequence of being placed under the complete anæsthetic influence of the inhalation of sulphuric ether before the operation was begun, the mother was quite unconscious of pain or suffering during the whole process of the turning and extraction of the infant. She made a recovery that was uninterruptedly good and rapid, and left her bed, dressed, and walked into the next room, on the fourth day after delivery.

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[1] From Provincial Medical and Surgical Journal, December, 1847, p. 673.

The preceding case was, at the time of its occurrence, one of intense interest to me in two points of view. For, *first*, it was the first case in which I or any other accoucheur had ever tried the effects of ether-inhalation during labor, and so far it is, I believe, destined to form the commencement of a new and important epoch in obstetric practice. But *secondly*, the case appeared to me to be one of great moment as an apposite illustration of views which I had been previously led to entertain, as to the possibility and propriety of substituting, in some instances, extraction by the feet for extraction by the crotchet—the delivery of the infant by the hand of the accoucheur instead of its delivery by instruments—the lateral compression of the child's head by the contracted sides of the pelvis, instead of its more dangerous oblique or longitudinal compression by the long forceps—and, *above all, the transient and not necessarily fatal depression of the flexible skull of the fœtus, for the destructive and necessarily deadly perforation of it.*

It is in this second or latter point of view that I propose to consider the preceding and other analogous cases in the present memoir.

In directing the attention of the Obstetric Society in January last to the case which I have stated above, and its relations to the question of turning, I took occasion to state that I had practised the same operation as an alternative for craniotomy and the long forceps, in other instances in which the head had been morbidly detained at the brim, from the existence of disproportion between the two; and that I believed it to present the following advantages over embryulcio: It gave the child a chance of life; it was more safe to the mother, because it could be performed earlier in labor and more speedily; it enabled us to adjust and extract the head of the child through the contracted pelvic brim in the most advantageous form and direction, the head flattening laterally under the traction; the neck, if the child were living or only lately dead, was so strong as to allow us to exert such a degree of traction upon the obstructed head, that the sides of the cranium might become very greatly compressed, or even indented under it, and that without necessarily destroying the child; and lastly, it was a practice which could be followed when proper instruments were not at hand, and the avoidance of instruments was generally desirable, when it was possible.

These observations were published in the March number of the "Monthly Journal of Medical Science," for 1847, p. 718.[1] In a later number of the same Journal, in describing a case of delivery through a pelvis extremely deformed by malacosteon, I took occasion to discuss, in brief terms, the different means of treatment required in contracted pelves of different dimensions; and I repeated in the same words the above advantages of turning in comparison with embryulcio. At the same time I stated in addition, in relation to the theory of the practice, my belief "that when the child presents by the feet, and thus the apex, instead of the vertex or base of the cone, formed by the head and body of the fœtus, comes first, it may make its transit without embryulcio, through a pelvis, the smallness of which would otherwise have necessitated mutilation or the operation of craniotomy."

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[1] I had previously stated the details of the case just related at page 639, and I again had occasion to do so at page 721 of that Journal.

The essay from which I make this extract appeared in the number of the Journal for the 1st of July, 1847.

A few weeks subsequently—viz., in the "Provincial Medical and Surgical Journal" for July 28th, Dr. Radford of Manchester engaged in the investigation of the question, by publishing a communication "On Turning in Labors rendered Difficult by Distortion of the Pelvis." In this communication Dr. Radford avoids all allusion, by name or reference at least, to the previous observations which I had ventured to offer to the profession on the subject, and he propounds the practice to his readers as if it were a new and original suggestion on his own part, as far as British midwifery is concerned. At the same time, in a spirit of true humanity, which does him high honor, he strongly declares that "whatever practice can safely supersede the murderous operation—craniotomy—should be adopted." "The records of operative midwifery ought not," he observes, "to be stained with so barbarous a procedure, which, according to the present recognized principles of practice, is so unconditionally and so unhesitatingly performed." "And," he adds, "it cannot be a matter of surprise that I should, entertaining opinions that craniotomy ought to be considered an operation of necessity, and not of election, hail with delight any measure which only promises to lessen the number of these destructive operations."

Afterwards, Dr. Radford proceeds to offer some remarks which evidently show that he has mistaken, alike in its principles and in most of its details, the alternative operation which I had proposed to substitute for instrumental delivery; the fault, however, is, I believe, chiefly or entirely my own, and is a natural result of the brevity and imperfections of my previous notices on the subject. Indeed, I have reason to know, that Dr. Radford's misconceptions of the proposed practice are, on this very account, shared in by many others. It is with the anxious view of remedying this defect that I draw up the present communication, and I trust that the great practical importance of the questions discussed in it will stand as some apology for the otherwise unwarrantable prolixity and repetions that may possibly be observed in the discussion.

In following out the subject I have indicated, I shall treat severally and successively of the kind of cases which gave rise to the suggestions of the proposed practice; of the theory, principles, or mechanism of the practice; and of its advantages in relation to the curtailment of the duration of labor. I shall then answer the objections which I have heard raised against it, in relation to the safety of the infant, and the safety of the mother; and lastly, I shall consider the classes of cases best adapted for the practice, and the conditions requisite for its success.

SECT. II.—EVIDENCE SUGGESTIVE OF THE PRACTICE.

In studying in their more minute details, the obstetric histories of recorded cases of morbid contractions of the pelvis, I have often been forcibly struck by the circumstance, that apparently when the child presented preternaturally, and in consequence, ultimately passed with the feet or pelvic extremity first, the labor seemed frequently both easier and safer to the mother and infant, than when, in other labors in the same patients, the head of the fœtus happened to form the presenting part. In this way, mothers who had always suffered under very severe and instrumental labors, when their children presented with the head foremost, were, I found, not unfrequently fortunate enough to escape far more easily, when, at other times, the infant chanced either to present originally with the feet, or had the feet artificially brought down during the labor in consequence of the infant lying transversely with the arm or shoulder constituting the presenting part; nay, the history of some few cases incidentally but strongly showed, that when the distortion and obstruction in the maternal passages were so great as to have invariably necessitated in all the previous labors the death of the infant by the operation of craniotomy, a living child was occasionally at last happily born alive, when it happened to pass, or to be brought through the contracted pelvis as an original or artificial footling presentation. In such instances the presentation of a hand or foot, when first discovered at the commencement of labor, has been regarded as an undoubted source of increased danger and difficulty, but it has at last more frequently proved a source of increased safety to the mother, and the indirect means of the preservation of the infant.

As an illustration of these remarks, I might appeal to the evidence of many different cases observed and recorded by different authors, without any view to such a question as that which forms the subject of the present memoir. But to avoid superfluity of proof, I will in the meantime restrict the evidence to a very abridged statement of similar cases recorded by one or two authors; and for this purpose, I first select the work of Dr. Smellie, believing, as I sincerely do, that his writings are not less remarkable for the strong thought and singular practical sagacity which they display, than for the spirit of candor and truthfulness with which all his facts are reported. In his volumes, Dr. Smellie has recorded an unusual number of contracted and distorted pelves; among them I find the following cases illustrative of the remarks which I have ventured to offer in the preceding paragraphs. The first three cases relate to natural pelvic presentations; the last two are instances in which the child was extracted footling, in consequence of its presenting transversely, or with the upper extremity.

CASE II.—To a woman "who had suffered *very much* in her former labors from the pelvis being distorted," Dr. Smellie was called, and found the breech of the child presenting. The extraction of the head required considerable management, but the child was saved.[1]

CASE III.—Dr. Smellie's assistance was requested for a patient with "the breech presenting, and the pelvis distorted." The midwife told him "that the woman's former labors had been *very* difficult and tedious, but now as the breech presented she was afraid the difficulty would be greater." He "saved this child also, although a good deal of force was used to deliver the head."[2]

CASE IV.—A woman, with the "pelvis narrow," and "who formerly was used to have tedious labors," had a breech presentation. Dr. Smellie brought down the legs, "as the breech did not advance with the assistance of the strong pains." The child was delivered with difficulty, but alive.[3]

CASE V.—A woman, with the "pelvis distorted and awry, from the right ilium being much higher than the other," was three times pregnant. The first labor was terminated by craniotomy. The child in the second labor was premature, and presented by the arm. It was brought away alive by turning. In her third labor the child reached the full time, presented by the head, was turned, and lost.[4]

CASE VI.—A woman, with "a narrow and distorted pelvis, from the three lowest vertebræ of the loins bending forward," four times reached the full period of pregnancy. In her first and third labors the child required to be mutilated by craniotomy before delivery could be accomplished. The infant in the second labor was small, and "with the greatest difficulty saved, by the assistance of the forceps." At the fourth confinement the right shoulder of the child presented. Dr. Smellie first tried cephalic version, but failed. He then seized one leg, turned the child, brought out the body and extremities, and ultimately, after much exertion, extracted the head.

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[1] Collect. 32, Case 10. [2] Ibid. Case 2. [3] Ibid. Case 13. [4] Ibid. 34, No. 2, Case 1.

The child was alive. The mother recovered better than in any of her preceding labors.[1]

In his "Clinical Midwifery," Dr. Lee reports the following case as one probably similar to those which first suggested the propriety of the artificial induction of premature labor:

CASE.—The patient had been five times pregnant. She was delivered of her first child by craniotomy. Dr. Lee employed the same operation to deliver her at her fourth confinement. "At the end of the seventh month of her second pregnancy labor came on spontaneously, and the child was born alive without artificial assistance, and has been reared." Her third pregnancy was terminated by labor coming on at the commencement of the eighth month. "The nates presented, and the child was also extracted alive." "Dr. H. Davies induced premature labor at the seventh and a half month of her fifth pregnancy, and the child was born alive, but died soon after in convulsions."

Occurrences similar to what happened in the preceding case must, observes Dr. Lee, "originally have suggested the idea of bringing on premature labor artificially in cases of distorted pelvis, and probably led, in 1756, to that consultation of the most eminent practitioners in London, at which the practice was approved of, and soon after successfully carried into effect by Dr. Macauley;"[2] and by a similar study of the modes of delivery occasionally adopted by nature in the same cases of distorted pelvis, we have, I think, suggested to us likewise the propriety of artificially turning and extracting the infant as a footling presentation. I shall cite only one other case from the same writer, showing the degree of difficulty in contracted pelvis to be less when the child passes by a pelvic or footling presentation, than when it presents and passes by the cephalic extremity.

CASE VII.—A patient, with the outlet and brim of the pelvis both considerably contracted, was delivered of her first child by craniotomy, after being more than forty-eight hours in labor, and extremely exhausted. "The difficulty," Dr. Lee remarks, "experienced in extracting the head with the crotchet, after it was opened, *proved* that delivery could not have been completed by any other method." At her third labor she was again delivered by craniotomy, after the labor had endured for some time, and four fits of convulsion had supervened. "I found her," says Dr. Lee, "completely insensible, with dilated pupils and constant convulsive movements of the muscles of the face. The pains continued with such violence, and recurred at such short intervals, that I dreaded rupture of the uterus. * * * The small size of the pelvis, the impossibility of applying the forceps to the head, the imminent risk of rupture of the uterus, with the result of the former labor, were the circumstances which made me determine to open the head." At her fourth confinement, the child's head was again perforated. Labor had commenced on the 2d of December; the liquor amnii escaped soon after; the pains continued strong and regular during the whole night. On the morning of the 8th, six days after labor "had commenced," the head being still wedged in the brim, the abdomen tense, the patient occasionally delirious, &c., craniotomy was performed. The patient was thus delivered by embryulcio in her first, third and fourth labors. The history of her second confinement is this,—"I proposed to induce premature labor on the 21st of July, 1835, when she was seven months and a half pregnant; but she would not consent to this. Labor came on spontaneously at the commencement of the ninth month of pregnancy; a foot presented, and the child was extracted dead, without craniotomy."[1]

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[1] Collect, No. 5, Case 2.

[2] Clinical Midwifery, Case 81, p. 39.

These, I repeat, and similar instances (and I shall have occasion to quote cases still more striking in the sequel), appear to me to point evidently to this probable conclusion—that in particular forms and instances at least, of distorted pelvis, the passage of the child by the feet or pelvic extremity affords to it and the mother some special facility of transit which is wanting when the head or cephalic extremity forms the presenting part. And we have thus placed before us this problem: Upon what does this greater facility and safety of footling, as compared with head presentations, in such cases, depend? Let us proceed to the consideration of this point, and attempt to ascertain how this result is obtained.

II.

SECT. III.—THEORY OR PRINCIPLES OF THE PROPOSED PRACTICE.[2]

The form of the infant at birth has often and justly been compared to that of a cone; the feet serving as the apex, and the arch or bi-parietal diameter of the head forming the basis of the cone, and there being a gradual tapering and increase of size from the former to the latter point. Consequently, when in cephalic presentations, the head or broad end of the cone once dilates and passes a given point, the narrow remainder of the cone, viz., the trunk and extremities, afterwards pass it without impediment. In fact, both in cranial and in footling presentations—even in instances of contracted pelvis—the transit of the body is usually attended with no special difficulty or delay. The obstruction is referable to the child's head alone; and hence the necessity of accurately studying the obstetric configuration and relations of the fœtal head, in order to be able to overcome the obstruction offered by this part.

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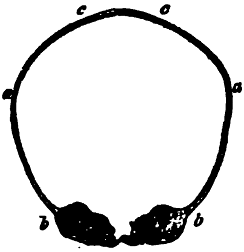
[1] Clinical Midwifery, p. 62.

[2] From Provincial Medical and Surgical Journal, January, 1848, p. 1.

Fig. 27.



Now, besides that the whole infant is, as I have just stated, of a conical figure, the head taken alone, presents more imperfectly the same configuration; for the basis of the skull is considerably narrower than the arch; or, in other words, its bi-mastoid diameter (*b b*, Fig. 27) is less than its bi-parietal diameter (*a a*), so that the cranium increases gradually in breadth and size, like the whole body, from below upwards.

The bi-parietal diameter of the head or vertex is, in this way, the *basis* both of the cone of the whole body, and of the cone of the head taken singly.

To understand thoroughly, however, the subject of our present inquiries, it is necessary to have some more precise and definite ideas of the degree of tapering of the cranium from its broad arch to its narrow basis; or, in other words, it is requisite to fix more accurately the degrees of difference between the bi-parietal and bi-mastoid diameters of the head. With this view, I made, some months ago, upon the heads of several new-born infants, a variety of measurements relative to these two points. I arrived at nearly the same conclusions as those which I afterwards found Mr. Tomlinson had previously obtained, when admeasuring the fœtal head for the purpose of assisting Dr. Hull in his inquiries as to the smallest dimensions the skull could be possibly reduced to, by the operation of craniotomy. And to prevent all cavil, I will here take Mr. Tomlinson's measurements as standard and correct, and the more valuable on this account, that they were made and published without any view to such a question as the present.

Mr. Tomlinson gives, among other measurements of the head, the breadth of the bi-parietal and bi-mastoid diameters in six infants at birth. I shall throw them into a tabular form, and add a column, showing the difference between these two diameters in each case.

Fig. 27. Vertical section of a fœtal skull, showing its conical form.

TABLE I.

Measurements of the Bi-parietal and Bi-mastoid Diameters of the Heads of Six Infants at Birth.

Number of Case.	Bi-parietal Diameter.		Bi-mastoid Diameter.		Difference between the two.	
	In.	8ths.	In.	8ths.	In.	8ths.
I.	3	4	2	7	—	5
II.	3	6	3	2	—	4
III. (a twin)	3	1	2	6	—	3
IV.	3	2	2	6	—	4
V.	3	3	2	6	—	5
VI.	3	4	2	6	—	6

We thus find a difference between the bi-mastoid and bi-parietal diameters of the head, varying from three-eighths to six-eighths of an inch, or from four and a half to nine lines. Or, if we exclude, as in fact should properly be done, the third case, as being a twin infant, and consequently having the head and body smaller and less remarkable than in a single and full-sized child—then we have, in the remainder, a difference between the bi-parietal and bi-mastoid diameters, varying from six to nine lines—that is, from four to six-eighths of an inch ; or, in other words, *from half an inch to three-quarters of an inch.*[1] And in artificially extracting an infant through a narrow pelvis, as a footling instead of a head presentation, we may gain, I believe, when necessary, this great difference between the size of the body to be extracted, by varying the method of the extraction itself. For it must be further held in view that at its base (or bi-mastoid diameter), the cranium of the infant is so strong, and its bones so firmly and densely united, as to render it quite incompressible. On the other hand, at its arch (or bi-parietal diameter), the cranium at birth is, in general, so thin, and its bones so loosely and imperfectly united, as to permit of the head being much flattened and compressed, or even depressed and indented at some point, without necessarily destroying life.

Now, when the brim of the pelvis is morbidly contracted at one part—let us imagine, for instance, its conjugate diameter to measure only three inches instead of four—then the child, upon being forced down upon it as a head-presentation, meets with difficulties which, probably, no uterine effort could possibly surmount. A round body, the diameter of which is some lines *above* three inches, is attempted to be pushed through an opening measuring *only* three inches.

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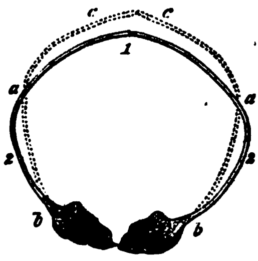
[1] "From the one parietal protuberance to the other, a transverse line measures from three inches and a quarter (3¼) to three inches and a half (3½). From one mastoid process to the other along the base is about two inches (2). From the one temple to the other is two inches and a half (2½)."—Dr. Burns' Principles of Midwifery, 10th Edition, p. 25.

But, turn the child, extract it footling, and let the head pass through the contracted brim by engaging in it, first the base or bi-mastoid diameter of the cranium, and the difficulty may possibly be overcome; for then we have the head entering the contracted brim of three inches as a body *less* in its diameter than three inches, and capable of having its broader upper portion flattened and reduced to the size necessary for its complete transit, by the force which we can apply to the already protruded body of the infant, producing the requisite degree of lateral compression of the cranium against the opposed sides of the contracted pelvic brim.

Or, let us take another and perhaps simpler view of the subject. Suppose, in want of a better wood-cut, we take the letter A as a round cone simulating the figure of the infant, the apex of the letter corresponding to the feet of the infant, the base of it corresponding to the bi-parietal diameter of the head, and the cross-bar of the letter representing the incompressible floor or basis of the skull, and the two divergent feet of it representing the elastic and compressible arch of the cranium. Now, if we desired to pass this round cone A through an oblong aperture O, the diameter of which was somewhat *less* than the diameter of the basis of the cone, should we succeed best by pushing it through the oval opening with its basis foremost, or by dragging it through it with its apex foremost? If we extract the cone through the aperture by bringing its narrow end foremost—or, in other words, if we bring the child by the feet instead of the head—then two objects are gained; for, first, we have the power of using any justifiable degree of force that may be required, by the command we thus obtain of the protruded and narrow end of the cone; and, secondly, the elastic sides of the base of the cone situated above the cross-bar—or, in other words, the sides of the cranium itself above its basis—will yield and become compressed together to such an extent as to enable the collapsed body to pass through the supposed opening. If, on the other hand, we attempt to make the broad basis of the cone, or the vertex of the head, to pass first through an aperture less than its own diameters, then we are apt, the more we press, to increase rather than diminish the difficulty attendant upon its transit; for the more force we apply we are liable to render that part which is already too broad, still broader, by flattening it against the sides of the aperture, and making its limbs or parietes at one point or another diverge and widen, instead of converge and contract. It is true that if we can insinuate *obliquely* or otherwise, the basis of the compressible cone (or the arch of the head), into the contracted aperture, so as to get *its* sides compressed between the sides of the opening, and consequently its bi-parietal diameter lessened by the force with which the body of the cone is pushed downwards, a similar object would be gained, but in a more uncertain and imperfect degree. Besides, there is always in this way the chance of making the two sides of the cranial arch (2 2, Fig. 28) spur out, and diverge lower down at *their* basis, in proportion to the amount of force applied to the upper surface of the arch itself (*c c*), so that while we contract the sides of the arch above by the kind of pressure applied to it, the direction of that pressure may possibly widen and open the limbs of the arch below, and so far increase, for the time being, the difficulty accompanying the transit of that part.

Fig. 28.



But this transverse or lateral form of flattening and compression of the cranium is very far indeed from being always obtained when the vertex is allowed to present at the brim of a distorted pelvis; for frequently the whole, or the greater mass at least, of the head remains obstinately above the brim, despite the action of the propelling efforts upon it of labor-pains, both dangerous in the amount of their duration, and dangerous in the amount of their force. By bringing, however, the *apex* of our cone, or the narrow diameters of the cranium foremost, we not only improve and simplify the mechanism of the labor by so far converting the entrance and passage of the child's head into the contracted pelvic brim, from a matter of comparative chance into a matter of comparative certainty; but further, we effect, thus, in the course of minutes, by turning, what it might require the course of hours or days to accomplish, provided the transit of the fœtal head were left as a head-presentation to nature alone, or to nature assisted at last artificially by the long forceps or crotchet.

But other advantages are obtained by turning, as compared with embryulcio and the long forceps, in the class of cases which we are considering. We not only bring the tapering or cone-shaped fœtal head through the distorted brim, by making its narrow extremity enter first into the contracted aperture, and *afterwards* using the sides of that aperture to compress the elastic parietes of the broader and higher part of the cranium, but in some cases we in this way, I believe, eschew altogether engaging the broadest part of the head (*a a*, Fig. 28), in the narrowest part of the contracted pelvic opening —a circumstance and adjustment that, in a head-presentation, would

Fig. 28. Illustrating the effect of pressure upon the top of the arch of the fœtal cranium, as happens when the head presents. The dotted line, *c b b c*, is the vertical section of a normal fœtal skull, as seen in Fig. 27. The outline, 1 2 2, shows the disadvantageous alteration in form produced by the presentation of the head to a contracted pelvis.

be otherwise quite unavoidable. For besides being found of a conical form when looked at vertically, the infantile head shows the same configuration also when viewed antero-posteriorly. The accompanying outline, for instance, of the fœtal head, as seen from above, is an exact copy of that given in Chailly's late work on Midwifery, and shows that while the head is very broad behind in its bi-parietal diameter (*a a*, Fig. 29), it gradually tapers and diminishes in breadth as we proceed forwards to its bi-temporal and bi-frontal (*b b*) diameters; and its bi-temporal diameter is in general fully half an inch less than its bi-parietal. But, in the mechanism of head cases, the neck, as is well known, becomes early flexed in the labor, so that the chin is brought towards the top of the sternum, and the vertex or upper and back portion of the head first becomes pushed downwards into the pelvic aperture, and thus constitutes the presenting part—in other words, the broadest part of the cone of the whole child, because the broadest part of the head, or its bi-parietal diameter, is thus naturally first driven downwards into the pelvic cavity, and is first directed against the contracted brim. The same head, when extracted as a footling presentation through the same distorted pelvis, will in some instances entirely escape having this, its broadest part, placed and engaged in the narrowest part of the pelvic aperture; for, as the extremities and the body of the child are gradually extracted in the operation of turning, the head, in ultimately adapting itself to the shape of the pelvis, comes to have its bi-temporal, and not its bi-parietal, diameter implicated in the narrowest or conjugate diameter, the parietal protuberances passing into the wider lateral space opposite the sacro-iliac synchondrosis. That such is the fact, the position of the mark or indentation upon the skull, produced by compression against the protruding promontory of the sacrum in Case I., and in other instances which I shall afterwards quote, amply proves.[1] The injury and depression of the cranium, if any, seems always situated before the ear, and consequently much before the parietal protuberance; or, in other words, it is, as I have said, in the temporal region. The anterior edge of the parietal bone, or the anterior edge of it and the corresponding posterior edge of the frontal, are generally the osseous points that

Fig. 29.

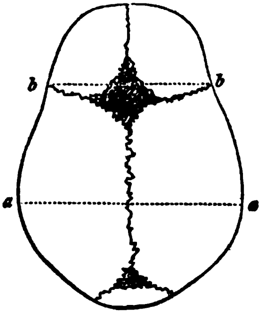


Fig. 29. Outline of the fœtal head as seen from above.—(From Chailly).

[1] See, for example, the figures of indented fœtal skulls given by Professor Sandifort, in pl. xxxiv. of vol. ii. of his Museum Anatomicum.

are depressed and indented, as represented in the accompanying diagram.

Now, the child's head narrows greatly from behind forwards, from the occiput towards the forehead, as seen in Figure 29; and in the temporal region its diameter is generally from three to five-eighths of an inch, or from four and a half to seven and a half lines *less* than between the two parietal protuberances.[1] Hence it is evident that a great gain is effected by the head passing through the narrowest part of the brim, with a diameter at least several lines less than it otherwise would offer, provided it happened that the vertex of the head had been kept and retained as the presenting part.

Fig. 30.

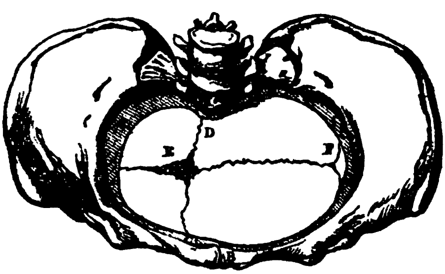
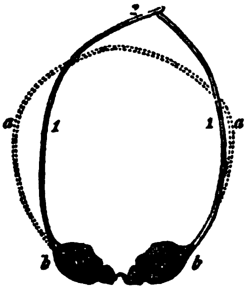


Fig. 31.



In order that the full-sized fœtal head may pass through a contracted pelvic brim we require to have the head flattened laterally, and its sides at its anterior parietal or temporal region collapsed and compressed. The outline, *a b a b*, Figure 31, is the outline of the vertical section of the infant's skull that I have already given in Figure 27. The line, 1 2 1, marks what we may presume, for the sake of illustration, to be the shape and degree of compression to which the head would require to be reduced in order to pass the contracted pelvic aperture.

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[1] See preceding note from Dr. Burn's Midwifery. Chailly gives the bi-parietal diameter as three inches and a quarter (3¼) to three inches and a half (3½); and the bi-temporal diameter as two inches and a half (2½) to three inches (3).—See his Treatise on Midwifery, 1844, translated by Dr. Bedford, p. 62.

Fig. 30. The form in which the bones of the fœtal cranium are compressed in passing through a contracted brim. A. Promontory of sacrum. B. Symphysis pubis. C. Unoccupied space between the ilium and forehead of the child. D. Depression in child's head corresponding to the promontory of the sacrum. E. Anterior fontanelle. F. Posterior fontanelle.

Fig. 31. Illustrating the effect of lateral compression upon the sides of the arch of the fœtal cranium, as happens when the child is extracted footling. The dotted line, *a b*, *a b*, is the vertical section of a normal fœtal skull, as seen in Figure 27. The outline, 1 2 1, shows the advantageous alteration in form produced upon the fœtal cranium by extracting the child footling through a contracted pelvis.

Would this necessary degree of compression be effected more easily and by less amount of force if the head were drawn through the narrow pelvis as a footling presentation, than if it were driven into it as a cephalic presentation? I certainly believe that less power would be required to produce the same degree of lateral compression and collapse in the cranial arch, provided the compressing force were applied, as in cases in which the child is extracted footling, directly to the two sides or lateral surfaces (1 1, Fig. 31) of the arch itself, than under the alternative condition supposed. For if the child descended into the pelvic brim as a head presentation, the uterine contractions would drive it against the narrow points of the pelvic brim, so that the reacting compressing power of these points would be applied either to the two upper surfaces of the arch (*c c*, Fig. 28), or partly to one of them (*c*), and partly to one of its opposite lateral surfaces (*a a*, Fig. 31). In such circumstances the compressing powers would be acting much more indirectly and far more at a disadvantage in effecting the required compression, than if they were applied, as in extracting the child footling, directly and immediately to the sides themselves (*a a*, Fig. 31) that are to be compressed.

"Does the head," asks Dr. Radford, "elongate more readily upwards than downwards? If only," he answers, "the same degree of extractile force is used, it does not." But what I have stated shows that the same degree of extractile force will produce the desired effect more easily, and as we shall afterwards see, more safely also, when the child passes with the pelvis, than when it passes with the head first. And this, I believe, affords us another advantage in the mechanism of footling as compared with cephalic presentations in instances of diminished pelvic brim. We are enabled to produce by it the same amount of compression of the fœtal head by a less expenditure of compressing force, for we apply it to the lateral surfaces or piers of the cranial arch (*a a*, Fig. 31), and not to its pedentures or upper surfaces (*c c*, Fig. 28). We effect a saving of force by the *direction* in which we apply the force.

I am well aware both that it is exceedingly difficult to state accurately and intelligibly the opinions one may happen to entertain on such a subject as we have been discussing in the present section, and that I am far from having succeeded in making the demonstration so simple as I could wish. It may assist, however, our comprehension of the matter if, before concluding it, we try to recapitulate and sum up the advantages which, in regard to the mechanism of labor, are, in the contracted states of the pelvic brim, obtained by the child passing as a footling instead of as a cephalic presentation. These advantages, as far as I have hitherto attempted to trace them in the preceding remarks, for we shall trace out more of them afterwards, amount to the following:

1. The fœtal cranium is of a conical form, enlarging from below upwards, and when the child passes as a footling presentation, the lower and narrower part of the cone-shaped head is generally quite small enough to enter and engage in the contracted pelvic brim.

2. The hold which we have of the protruded body of the child, after its extremities and trunk are born, gives us the power of employing so much extractive force and traction at the engaged fœtal head, as to make the elastic sides of the upper and broader portion of the cone (viz., the bi-parietal diameter of the cranium) become compressed, and, if necessary, indented, between the opposite parts of the contracted pelvic brim, to such a degree as to allow the transit of the entire volume of the head.

3. The head in being dragged downwards into the distorted pelvis, generally arranges itself, or may be artificially adjusted, so that its narrow bi-temporal, instead of its bi-parietal, diameter, becomes engaged in the most contracted diameter of the pelvic brim.

4. The arch of the cranium or head is more readily compressed to the flattened form and size required for its passage through a contracted brim, by having the compressing power applied, as in footling presentations and extraction, directly to its sides or lateral surfaces, than by having it applied, as in cephalic presentations, partly to the lateral and partly to the upper surfaces of the arch.

Lastly, I may add, as a result of the whole mechanism, that the *duration* of the efforts and sufferings of the mother is greatly abridged by turning, when used as an alternative for craniotomy and the long forceps, and that thereby her chances of recovery and safety are increased. But as this is in itself a matter of the highest moment, in reference to the whole question of the proposed practice, we shall devote a special section to the consideration of it.

SEC. IV.—THE DURATION, AND CONSEQUENTLY THE DANGER, OF THE LABOR IS DECREASED BY THE PROPOSED PRACTICE.[1]

In the short summary which I gave in January last of the advantages of turning, over embryulcio, I stated one strong reason in favor of delivery by the alternate operation to be this—"*that it was more safe to the mother, because it could be performed earlier in the labor, and more speedily,*"[2] than craniotomy, or the application of the long forceps. And I have the most sincere belief that the practice I propose may be the means of saving much, both of maternal and infantile life, by enabling us in some cases to abridge the mere duration of a difficult labor, so that we may terminate in a few minutes a delivery, which under other plans of treatment, might still be protracted onwards for hours or even for days. At the same time I am fully aware that when I state my conviction that the mere degree of *duration* and continuance of a labor is, *per se*, dangerous both to the mother and child, and very often fatal even in its influence, I venture to broach a doctrine which stands up alike against the opinion and the practice of some of the highest authorities in the obstetric profession.

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[1] From Provincial Medical and Surgical Journal, February, 1848, p. 57.

[2] Monthly Journal of Medical Science, March, 1847, p. 718.

About half a century ago, when treating of the influence of the duration of labor in difficult and instrumental deliveries, Dr. Osborne observed—"I believe it is confirmed by general observation, that women recover at least as well, after long, lingering, and laborious labors, the duration of which may have been extended to several days, as after the easiest, quickest, and most natural delivery."[1] In making this remark, Dr. Osborne stated not his own opinion only, but, I believe, the general opinion of the accoucheurs of his time; and the same doctrine, little or not at all modified, still continues to be taught and acted upon, down to the present day, in the great English and Irish schools of midwifery, as the able and excellent writings of, for example, Professors Davis and Murphy in London, and Drs. Collins and Beatty in Dublin, &c., fully testify.

That, *per se*, the duration of labor has no very direct nor decisive influence upon the degree of danger and fatality accompanying parturition, is a doctrine which probably originated in, and has been perpetuated by, an erroneous system of observation. In founding and supporting it, obstetricians have, I believe, drawn their deductions, not from the *whole* of their practice, but from *parts* only of it; they have not reckoned upon the certain results of their *general* collection of facts, but depended upon the fallacious results of isolated and *individual* instances: they have not, in short, counted up the terminations and consequences of *all* their protracted cases, but have relied upon the evidence of some *single* striking case or cases of perfect, and it may be, rapid recovery after labor of a very prolonged duration. In proceeding thus, obstetric authors have not followed a course altogether different from that pursued in the study and investigation of many other similar questions in medical science and practice. For confessedly, the medical mind has ever been too apt to recollect and reason upon those facts only which are in favor of any preconceived opinion or opinions it may chance to have adopted; and the cases of failure are too often forgotten amid the more agreeable remembrance of the cases of success. It is on this account that the numerical method of reasoning and investigation, by obliging us to count up *all* our cases, and *all* our results, whether good or bad—whether instances of recovery or instances of death—is no doubt destined to revolutionize, in a great degree, our modes of inquiry, particularly in surgery and midwifery, by imparting infinitely more precision and certainty to our present deductions and precepts where they are true, and showing us in language that cannot be misunderstood, the erroneousness of our doctrines where they are not true.

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[1] Essays on the Practice of Midwifery in Natural and Difficult Labors, p. 66.

The question of the duration of labor in relation to its effects upon the danger and mortality accompanying parturition will be found, in my humble opinion, to afford an apposite example of the truth of these remarks—for when this question is examined, as it can alone properly be examined—by the numerical or statistical method, the results that are thus yielded, assuredly directly gainsay and contradict the accredited impressions and common belief of the profession upon the subject. With the exception of Dr. Collins of Dublin, few, or indeed no writers have, in their obstetric reports of hospital or private practice, furnished us with data showing the duration of labor either in their natural or in their operative cases of midwifery. In his "Practical Treatise on Midwifery" (a work, the great value and candor of the facts contained in which it would be difficult to overpraise) Dr. Collins has given the result of the cases which occurred in the Dublin Lying-in Hospital during a period of seven years, commencing November, 1826, and he has published throughout the work, and especially at the end of its individual chapters, tables, and data, from which the duration of the labor in most cases and complications can easily be calculated and compared. I shall state briefly some of the results regarding the influence of the duration of labor which I have obtained by summing and counting up the individual facts and data which Dr. Collins has published. And the results which I have in this way obtained will, no doubt, be regarded by some as the more valuable and trustworthy, seeing that the data from which they are calculated have been collated by Dr. Collins, and given by him to the world under the generally prevailing idea that the mere length of the labor was no guide or test of its danger or mortality.

During the seven years that Dr. Collins had charge of the Dublin Lying-in Hospital, 16,414 women were delivered in it; and of these 164 died, or one in every hundred of the mothers were lost. In 15,850 of these cases the duration of the labor has been marked and published; in 1 in 29 only it was not noted. Out of the 164 fatal cases, in 138 the duration of the labor has been marked and published; in 1 in 6 it was not noted. Now, if in relation to the duration of labor as bearing upon the maternal mortality, we calculate the proportion of the 138 maternal to the whole 15,850 labors, I find that the interesting result is that which is shown in the following table. Let me merely premise that the table reads as follows: Out of all the women delivered within one hour, 1 in 322 died; out of all those in whom the labor continued from two to three hours, 1 in 231 died; and so on.

TABLE II.

Showing the Proportion of Maternal Deaths in relation to the Duration of Labor in 15,850 cases of Delivery.

Duration of Labor.	Proportion of Mothers Lost.
Within 1 hour,	One in 322 died.
From 2 to 3 hours,	" in 231 "
" 4 to 6 "	" in 134 "
" 7 to 12 "	" in 80 "
" 13 to 24 "	" in 26 "
" 25 to 36 "	" in 17 "
Above 36 hours,	" in 6 "

The preceding table affords ample evidence that, contrary to the general opinion of the obstetric profession, the mere length of the labor is a most serious and important element in reference to the degree of danger and fatality accompanying the process. For, in fact, as shown by these calculations, "*the mortality attendant upon parturition increases in a ratio progressive with the increased duration of the labor.*"[1] And this does not hold true with regard to the

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[1] The law itself—and I believe it to be a most important one—was originally educed by a series of calculations from Dr. Collins' cases. I formerly published merely the *results* of the calculation, without publishing the *data* on which these results were founded; for my object was not to overload my paper, or the pages of the Journal unnecessarily. As Dr. Collins believes, however, that these results are obscure, I shall simply insert, in a tabular form, the data themselves upon which this law and its results were founded.

Table showing the Proportion of 138 Maternal Deaths in Relation to the Duration of Labor in 15,850 Cases of Delivery recorded by Dr. Collins.

Duration of Labor.	Number of Deliveries.	Number of Deaths.	Proportion of Deaths.
Within 1 hour,	3,537	11	1 in 322
From 2 to 3 hours,	6,000	26	1 in 231
From 4 to 6 hours,	3,875	29	1 in 134
From 7 to 12 hours,	1,672	21	1 in 80
From 13 to 24 hours,	502	19	1 in 26
From 25 to 36 hours,	134	8	1 in 17
Above 36 hours,	130	24	1 in 6
Total,	15,850	138	1 in 115

Out of Dr. Collins' 16,414 cases, he noted the duration of the labor in 15,850; and among these 15,850 cases, 138 maternal deaths occurred. The preceding table contains, in its first column of figures, the total number of deliveries that took place under each specified period of duration; in the second column is stated the corresponding number of deaths occurring life of the mother alone. It equally holds true with regard to the life of the infant. For, exactly in the same way, as proved from the same collection of data, I find also that, as a general fact, "*the infantile mortality attendant upon parturition increases in a ratio progressive with the increased duration of the labor.*" Dr. Collins reports 1121 children as having been still-born. In 1045 of these cases, the duration of the labor was noted. The following table shows the proportion of the children lost in labors of different degrees of duration. Like the preceding table, it reads thus: Out of all the women delivered within two hours, one in twenty of the children was lost; out of all the women whose labor endured from three to six hours, one in eighteen of the children was lost; and so on.

TABLE III.

Showing the Proportion of Infantile Deaths in relation to the Duration of Labor in 15,850 cases of Delivery.[1]

Duration of Labor.	Number of Infants Still-born.
From 1 to 2 hours.	One in 20 was lost.
" 3 to 6 "	" in 18 "
" 7 to 12 "	" in 11 "
" 13 to 24 "	" in 6 "
" 25 to 36 "	" in 3 "
Above 36 hours,	" in 2 "

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among each set of these deliveries; and in the third column is given the proportion which the deaths bear to the whole deliveries in labors of each specified degree of duration. In other words, the table reads thus;*—3,537 mothers had their labors terminated within one hour from their commencement; and of these 3,537 mothers 11 died, or 1 in every 322. The labor continued from 2 to 3 hours in 6000 cases, and out of these 6000 cases 26 mothers died, or 1 in every 231; and so on.—Letter to Dr. Collins.—Prov. Med. and Surg. Journal, Nov. 1848.

[1] The above rule is merely a simple generalization of Dr. Collins' own experience and cases, as will be seen in the following table, also deduced from Dr. Collins' own data:

Table showing the proportion of Still-births in Reference to the Duration of Labor in 15,850 Cases of Delivery.

Duration of Labor.	Number of Deliveries.	Number of Still-born Children.	Proportion of Still-born Children.
Within 2 hours,	7,050	347	1 in 20
From 3 to 6 hours,	6,362	346	1 in 18
From 7 to 12 hours,	1,672	151	1 in 11
From 13 to 24 hours,	502	88	1 in 6
From 25 to 36 hours,	184	42	1 in 3
Above 36 hours,	130	71	1 in 2
Total,	15,850	1045	1 in 15

Dr. Collins also reported 284 cases of death of the child subsequent to birth.

If we throw out of these 284 cases, the premature or twin children, as too small in size to be influenced much in their passage during labor, and those born imperfect or diseased

* The other tables that follow are constructed on the same plan, and read in the same way.

The two preceding tables refer to the proportion of maternal and infantile deaths as regulated by the duration of the labor. But many morbid complications and accidents may arise during parturition without necessarily proving fatal either to the mother or infant. The occurrence, however, of these complications and accidents is regulated by the same law of duration as I have above shown to regulate the actual deaths of the mother and children. For, exactly in the same way, it may be laid down as a general proposition, that "*the liability to most of the morbid complications connected with labor increases in proportion as the labor is increased in its duration.*"[1] It would be easy to demonstrate, for instance, that this holds good with regard to various obstetric complications taken

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we have 155 children left, born at the full time, and apparently perfect in formation at birth. The following table of these cases will prove that tedious and protracted labor is also a *cause of death to the child after birth.*

Table of Duration of Labor in 155 Deliveries where the Children Died within a few Days after Birth. (From Dr. Collins' data.)

Duration of Labor.	Number of Deliveries.	Number of Deaths.	Proportion of Deaths.
Within 6 hours,	13,412	110	1 in 122
From 7 to 12 hours,	1,672	24	1 in 70
Above 12 hours,	766	21	1 in 36
Total,	15,850	155	1 in 102

—Letter to Dr. Collins.

[1] Dr. Collins remarks that this does not accord with his experience. But his own facts and data show the opinion to be true. The following table is a brief way of expressing these data in reference to the eighty-four cases of puerperal fever, in which the previous duration of the labor was noted by him, and it sufficiently shows that the liability to this, like other complications, increases as the labor increases in duration:

Table of Duration of Labor in 84 Cases of Puerperal Fever, in the Dublin Hospital, during Dr. Collins' Mastership.

Duration of Labor.	Number of Deliveries.	Number of Cases.	Proportion of Cases.
Within 6 hours,	13,412	61	1 in 219
From 7 to 12 hours,	1,672	10	1 in 167
Above 12 hours,	766	13	1 in 59
Total,	15,850	84	1 in 186

One fundamental mistake in his statistics and calculations, led Dr. Collins into numerous errors and inaccuracies relative to the effects arising from the morbid protraction of labor. This error has made him repeatedly express and maintain, as the supposed result of his own facts and experience, opinions which his own facts and experience, when properly interpreted, totally and altogether contradict.

The error I allude to is this: in calculating from his experience of 16,414 deliveries, the effects upon the maternal mortality of morbid prolongation of the labor, as a special or individual complication, he calculates the resulting number of deaths in relation to the *total sum of all the cases delivered* (16,414), instead of calculating them in relation to the *total sum of all the cases merely that were protracted* (452); he calculates the effects of the complication individually—such as convulsions, rupture of the uterus, retention of the placenta, or hemorrhage before and after its removal, puerperal fevers, puerperal inflammations, &c. The liability to each one of these complications and results certainly increases with the length of the process of parturition. But it may be enough for my present purpose to show that, taking these complications collectively, such is the result: the tendency to their general occurrence becomes greater and greater in proportion as the length of the labor is allowed to become greater and greater.

In his Treatise, Dr. Collins has recorded the duration of the labor in 24 cases of puerperal convulsions; in 24 cases of rupture of the uterus and vagina; in 62 cases of retention of the placenta; in 102 cases during and after the third stage of labor; in 84 cases of puerperal fever; in 5 cases of fatal pelvic abscess; and in 5 fatal cases of inflammation and sloughing of the vagina; in all, 306 cases of complication occurring in 296 patients. The following table shows the proportion in which these 306 complications occurred in labors of different durations. It reads thus: Of all the women who were delivered within one hour after the commencement of labor, one in 114 suffered from one or other of these obstetric complications; of all those whose labors were prolonged from one to two hours, one in 90 suffered from one or other of these complications; and so on.

TABLE IV.

Showing the Proportion in which 306 *Morbid Complications occurred in* 15,850 *Labors of Different Durations.*

Duration of Labor.	Proportion of Complications.
Within 1 hour,	One in 114 was complicated.
From 1 to 2 hours,	" in 90 "
" 2 to 3 "	" in 69 "
" 4 to 6 "	" in 58 "
" 7 to 12 "	" in 32 "
" 13 to 24 "	" in 12 "
" 25 to 36 "	" in 9 "
Above 36 hours,	" in 5 "

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relatively to the whole number of cases of delivery, instead of relatively to the whole number of cases of this special complication (protraction). An example may illustrate my meaning. I shall take it from the subject of rupture of the uterus and vagina.

During Dr. Collins' seven years Mastership of the Dublin Hospital, 34 cases of rupture of the uterus and vagina occurred, and 32 of the mothers died; 2 only of the 34 survived. If he argued upon this special complication as he does with regard to the special complication of protraction, he would maintain that the mortality from rupture was strikingly small; for only 32 mothers out of 16,414, or about 1 in every 513, died of it. This would show, certainly, the proportion of deaths from rupture in relation to the total sum of all the cases delivered; but it would not show what was wanted, viz., the proportion of deaths from rupture in relation to the total sum of all the cases in which rupture happened. If he wished, in short, to state the real risk and danger attendant upon this special complication, The evidence which I have adduced in the preceding tables affords ample proof that the proportion of maternal and infantile deaths, and of obstetric complications, becomes, as a *general law*, gradually more and more numerous as the process of labor becomes more and more prolonged in its duration. But I have brought forward the facts principally for the purpose of showing that out of them, and in connection with them, there is deducible another all-important principle, in reference to a practical and operative obstetric inquiry of the present nature, viz., that when operative interference, such as the forceps, crotchet, turning, &c., is required, in order to terminate a difficult and morbid labor, the *time* of the labor at which that interference is had recourse to, regulates the amount of attendant danger much more than the details and effects of the operation itself. In other words, in accordance with the general law already illustrated, the extent of danger or fatality attendant upon operative and instrumental delivery is regulated in a direct and demonstrable degree by the previous length of time allowed to elapse before the artificial interference is had recourse to; the resulting mortality increasing in amount according as the period at which the operation is performed is proportionally more distant from the date of the first commencement of the labor.

It is not in my power to prove from Dr. Collins' Treatise that this law holds true in regard to the alternative operation of delivery by the long forceps, for his work affords no data on this point. In fact, in instances in which the head of the child is arrested at the brim, he and the profession generally in Dublin prefer the use of the perforator to the use of the long forceps; and during the course of the seven years throughout which his report extends, this latter instrument does not seem to have been employed once in the Dublin Hospital, while craniotomy was, during the same period, employed above eighty times. During these seven years, however, the short forceps were used to terminate the delivery in twenty-four patients in whom the head was sufficiently low down in the maternal passages; and perhaps the results in these twenty-four cases will be regarded as a sufficient illustration that in forceps operations the general law holds good, that "*the operation is dangerous and fatal in proportion to the length of labor allowed to elapse before the artificial delivery is practised.*"

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rupture, he should state that 32 mothers died of it out of 34 cases in which it occurred, instead of stating that only 1 in 513 died of it. And, exactly in the same way, if asked what was the degree of maternal risk and danger attendant upon another special complication, viz., protraction of labor beyond twenty hours, his answer upon the same principle should not have been 42 deaths in 16,414 labors, or in the proportion of 1 in 391; but 42 deaths in 452 labors, the whole number protracted beyond twenty hours, or 1 death in every 11 cases of labor prolonged to this amount of protraction.—See Letters to Dr. Collins.—Prov. Med. and Surg. Journal, Nov. and Dec. 1848, pp. 601 and 683.

TABLE V.

Showing the Maternal Mortality attendant upon 24 Forceps Operations to be regulated by the previous Degree of Duration of the Labor.

Date of Operation.	Results to the Mothers.
In labors terminated within 24 hours,	One out of 13 died.[1]
In labors terminated in from 25 to 36 hours,	" " 6 "
In labors terminated in from 37 to 48 hours,	" " 4 "
In labors terminated in above 48 hours,	" " 2 "

Dr. Collins' data regarding the operation of craniotomy are more extensive, and hence more valuable. During the seven years included in his report, perforation of the head of the infant was practised in eighty-five cases in which there was extreme difficulty in the labor, or where the child was dead, and interference necessary for the patient's safety, or for the full completion of the delivery. In seventy-six out of these eighty-five cases, data are afforded for ascertaining the duration of the labor.

Out of the seventy-six women, fourteen, or one in every five and a half, died. The following table of the results of these seventy-six cases of craniotomy, as influenced by the previous duration of the labor, affords direct evidence of the proposition which I have already stated, that the fatality attendant upon this, as upon the other allied forms of forced or artificial delivery, is regulated by the date or period of the labor at which the operative interference is had recourse to.

TABLE VI.

Showing the Maternal Mortality dependent upon 76 Craniotomy Operations to be regulated by the previous Degree of Duration of the Labor.

Date of Operation.	Results to the Mothers.
In labors under 24 hours,	One out of 19 died.
" " from 25 to 48 hours,	" " 8 "
" " above 48 hours,	" " 3 "

The preceding body of evidence might easily be rendered more minute and detailed if it were necessary. But I hold that it affords proofs sufficiently strong and direct for our present inquiry. And the important relations which it has to that inquiry amount to this, that the facts adduced demonstrate—

1st. That, as a general law, the amount and proportion of maternal and infantile deaths accompanying parturition are regulated and modified by the length and duration of the labor.

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[1] In the single fatal case of forceps operation out of thirteen in which the women were delivered before the labor had continued twenty-four hours, the mother became insensible after the pains had set in for a few hours, and the instruments were apparently used in consequence. The whole labor was only of eight hours' duration. She died in three hours after delivery.

2*d.* That the liability to various morbid obstetric complications during and after delivery is modified and regulated by the same law; and—

3*d.* That the mortality attendant upon obstetric operations, such as the use of the forceps and crotchet, is also strictly modified and regulated by it—operative delivery being more and more fatal in proportion to the lateness in the course of the labor at which it is adopted; and, on the other hand, less and less fatal in proportion to the earliness in the course of the labor at which it is practised.

Now, the object of the present memoir is to propose that in cases of arrestment of the head at the brim of the pelvis, artificial delivery should be accomplished by turning, instead of by the long forceps or craniotomy. And the operation which I thus wish to substitute has, as I have stated at the commencement of this section, this great and high advantage over both the two others, for which I propose it as an alternative, that *cæteris paribus*, delivery by turning can, and must, as a general rule, be practised far earlier in the labor, than delivery either by the long forceps or the crotchet; and in proportion as it is practised earlier, so far also will it be attended by greater safety and greater success. A few remarks upon the time at which the employment of the long forceps or perforator is usually adopted will render this deduction more evident. In our next communication we shall consider this point, and the value of auscultation in relation to operative delivery.

SECT. V.—RELATIVE PERIODS OF THE LABOR AT WHICH THE LONG FORCEPS, PERFORATION, AND TURNING, ARE RESPECTIVELY EMPLOYED: VALUE OF AUSCULTATION IN OPERATIVE DELIVERIES.[1]

In the preceding section I have attempted to show the comparative advantage of turning over the employment of the long forceps or crotchet, on the ground that we could thus accomplish the delivery much earlier in the course of the labor, and hence much more safely for the mother. In fact, when the long forceps are used in instances of arrestment of the head at the brim, they are in general never applied till the cranium is thrust and wedged down into the upper pelvic aperture to as great an extent as can possibly be effected by the unassisted uterine efforts, and till symptoms of local irritation, or constitutional reaction or exhaustion offer to appear. But before all this state of matters is brought about, both much expenditure of muscular and vital exertion on the part of the mother, and also much expenditure of time is usually required; and the walls of the maternal passages are liable to have become injured by irritation and compression. Before the assigned conditions are produced, many long hours commonly elapse, each one of them directly and rapidly adding to the chances of danger to the mother. On the other hand, in the same set of cases, if artificial delivery by turning were adopted, it might and would be practised as early as the passages were adequately dilated, before the head were pushed down into the pelvic brim, and long before any symptoms of local lesion or general irritation and exhaustion presented themselves.

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[1] From Provincial Medical and Surgical Journal, February, 1848, p. 85.

But it is principally as a substitute for craniotomy that I venture to suggest the operation of turning in instances of arrestment of the head at or above the brim of the pelvis. And when we contrast the different times, as far as regards the duration of labor, at which these two modes of delivery (craniotomy and turning) would be respectively followed in the same class of cases, we shall perceive still more strongly the advantages of the alternative practice. For turning, as I have just now stated, would necessarily be performed early in the course of the labor, and so far at a period when the danger would be comparatively slight, according to the law of duration which I have attempted to establish. Commit, however, the same morbid cases to the usual rules of management, by which they would be treated under the prospective idea that they would in all probability ultimately become cases of craniotomy, and mark the difference. The labor is allowed to go on from hour to hour, and possibly for one or two long and weary days, in order to give the child every legitimate chance of being at last expelled alive, or, at least, expelled without operative mutilation. In the meantime, however, as the preceding tables abundantly prove, the chance of immediate or ultimate danger to both mother and infant increases fearfully in proportion as the labor drags on its duration. How long, then, shall we allow it to continue before we perforate and break up the child's head? Most modern authorities would answer that question by earnestly inculcating the propriety of not applying the perforator till the child were distinctly and certainly dead, unless indeed, the appearances of danger to the mother were extremely marked and imminent, for almost all eagerly and properly declare their abhorrence at the idea of "plunging an iron instrument into the centre of the skull of a living human being."

But how long must we probably wait before the child really dies? We have seen (Table III.) that the infantile, like the maternal mortality, increases in a ratio progressive with the increased length of the labor; and that out of all cases in which, in the Dublin Hospital, the process of parturition was prolonged beyond thirty-six hours, one in every two of the children was lost. But still the labor may go on to the fortieth, fiftieth, or sixtieth hour, and yet the child live. Out of twenty-seven cases in all, reported by Dr. Collins, in which the labor was prolonged to sixty hours and upwards, in sixteen the child was ultimately born dead; and in the remaining eleven it seems to have still survived, and ultimately to have been expelled alive. Whilst, however, we are thus waiting and watching for the certainty of the child's death *before* we operate, the chance of the mother's death increases with the lapse of every hour. Out of these same twenty-seven cases of labor prolonged sixty hours or upwards, seven of the mothers, or one in every four, perished. We may thus wait with the expectation of the infant's death, till at last we run the imminent hazard of also losing the mother; and even then, when at last we find ourselves driven to have recourse to craniotomy in order not to place the parent in greater jeopardy, we may be unwillingly compelled to practise the operation while the child still really lives, goaded on by a dire, but now irresistible necessity, to attempt to save the life of the mother by taking away the life of her child.

Auscultation is the criterion upon which all modern accoucheurs properly and mainly depend for ascertaining the death of the infant and fixing the date at which it threatens to perish, or actually does perish, during a protracted labor. And certainly, in the progress of a long case, auscultation is, as Dr. Collins states, of "immense value in ascertaining the life or death of the child." "I know," he remarks, "of no case where the advantage derived from the use of the stethoscope is more fully demonstrated than in the information it enables us to arrive at with regard to the life or death of the fœtus, in the progress of tedious and difficult labors. It is, in my opinion, one of the greatest improvements made in the practice of midwifery."[1] The whole evidence of Dr. Collins' general remarks and individual cases shows, that in protracted parturition the special advantage which he has found and expects others to find, from the employment of auscultation, is a far more precise and certain knowledge than we could otherwise possess, of the life or death of the infant, and of the exact time at which it does die, with a view of fixing more exactly the time at which we should deliver it by craniotomy. Actuated by the best and most humane feelings, Dr. Collins believes that in tedious and difficult labors, obstetricians should rarely or never perform craniotomy upon the head of a *living* child, and that auscultation will enable us to eschew this otherwise unavoidable error. For, in one set of cases, it will show us the propriety of postponing the operation, by showing us that the child's heart is still beating. In another set of cases it will show us the propriety of practising it earlier than we should otherwise do, by showing us that the child is already dead. At the same time we must still not use our perforator immediately after the fœtal heart ceases to be heard by the stethoscope, or indeed for some hours afterwards, lest life be not perfectly and entirely extinct in the child. "It is totally contrary," says Dr. Collins,[1] "to every rule of practice to deliver the patient with the crotchet when the fœtal heart's action *first* becomes inaudible. It is only after frequent examinations, with some interval between each, that this is ever resorted to, except where the mother's life is in *very imminent danger* indeed."

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[1] Practical Treatise, p. 18.

Such is the use and value of auscultation in protracted labors, according to the opinion of Dr. Collins and of the Dublin school generally; and certainly, while for one, I most gladly and gratefully acknowledge that it is to that school that British midwifery stands indebted for the first introduction of auscultation into English obstetric practice, still I cannot help declaring my conscientious conviction here, as I have often done in lecturing upon the subject, that the preceding application of it in tedious labors is so far undoubtedly erroneous in two most important and fundamental points.

Errors in Practical Application of Auscultation.

For, *first*, according to the principles upon which the Dublin practitioners use auscultation in tedious labor, viz., in order to fix the fact of the death or life of the child, with a view to the immediate performance or further postponement of craniotomy, they altogether leave out of consideration the great influence which the mere duration of the labor exerts upon the maternal constitution and life. They forget that whilst they are delaying for the death of the child, the very delay is rapidly destroying the mother; whilst they are waiting and watching for the fœtal life to become extinct, the maternal life is every hour becoming placed in greater and greater danger. "Under proper management," argues Dr. Collins, "the death of the child takes place in laborious and difficult labors before the symptoms become so alarming as to cause any experienced physician to open its head for the sake of the safety of the mother." But I believe his own facts totally and sternly contradict this argument. When for this or other causes he waited till the labor had continued from twenty-five to thirty-six hours from its first commencement, one in every seventeen of the mothers died (see Table II.); when he waited for twelve hours longer, allowing the labor to be prolonged from thirty-seven to forty-eight hours, then one in every six of the mothers was lost; when he waited for twelve more hours (namely, forty-nine to sixty hours), then one in every five of the mothers was lost; and when interference was not adopted, or delivery completed, till the labor had gone on for sixty hours or upwards, then one in every four of the mothers perished: and that too, although, as we have already seen, in eleven out of twenty-seven cases, or in about one in two and a half of these very same protracted labors, the child was still living, and was indeed ultimately expelled alive, thus *still* contra-indicating craniotomy, as far as its life was concerned, notwithstanding that the life of the mother was reduced to an extreme degree of danger and hazard.

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[1] Dublin Medical Journal, vol. xiii. 1838, p. 423.

But, *secondly*, the opinions which Dr. Collins and others of the Dublin school profess with regard to the application of auscultation, as a valuable aid to the management of tedious and instrumental labors, are, I conceive, still more objectionable in another point of view. In cases of protracted parturition they use the stethoscope, as we have shown, in order to ascertain the life or death of the child, and in order to decide and determine the proper time at which embryotomy should be performed. For this purpose they employ it with the special object of fixing the fact that the child is dead; and fixing, when requisite, the true time when it does die. But I am strongly of opinion that the mighty boon which auscultation offers us in protracted parturition is quite different and far more important. For it is not by any means so valuable in thus often affording us evidence that the child is dead; it is not so much of real and practical use in showing us that we may now perform embryotomy upon a dead infant, as in showing us *when*, in protracted cases, we ought to extract the child by the forceps, turning, or other safe means, if we wish to *preserve* its life, as well as the life of the mother. In this way I have repeatedly found auscultation of incalculable benefit in protracted labors, and been, I believe, enabled by its evidence alone to save several times the life of the child by the timely application of the long and short forceps. And though the principle upon which I have thus acted in the management of tedious labors is not acknowledged by Dr. Collins under that subject, yet certainly, the very same principle of using instrumental delivery for the sake of the safety of the child is acknowledged by him under another head, and in dependence upon another form of evidence of the danger of the infant. In speaking of the treatment of cases of prolapsus of the umbilical cord, he observes:[1] "The forceps I consider highly desirable when the child is so situated that the head can be reached with safety; but as the funis generally descends at the commencement of labor, it is very seldom that this instrument is applicable till fœtal life is extinct. We should not fail, however, to apply them when practicable, should any delay occur in the delivery likely to

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[1] Practical Treatise, p. 344.

endanger the life of the child, or if we find *the pulsation in the cord becoming gradually slower and more feeble.*" Now, exactly on the same principle, when in tedious labors, in which there is no contra-indication, we have evidence that the life of the child is becoming endangered, when by the stethoscope we learn that the pulsations in the fœtal heart are becoming gradually slower and more feeble, it is in my opinion equally our bounden and solemn duty to interfere by the same means in order to attain the same end, viz., the preservation of the infant's life. For I hold that neither in principle nor in practice does it make any difference whether with this view we ascertain the imminent danger of the child, and the fact that the pulsations of its heart are becoming slower and more feeble, by *feeling* the pulsations of the cord, or by *listening* to the pulsations of the heart itself. The sense of hearing affords us precisely the same evidence in the one case as the sense of touch affords us in the other, and it indicates also precisely the same line of treatment.

The bearing of this long discussion and digression upon the practice of turning in contracted pelves, &c., is sufficiently evident. Over both of the operations for which I propose it as a substitute, viz., delivery by the long forceps and crotchet, it possesses this great and weighty advantage, that it can be practised at a far earlier, and consequently at a far safer, period of the labor. But it is principally as an alternative for craniotomy that I feel anxious to introduce the operation of turning to the consideration of my professional brethren; and over craniotomy it possesses, in addition to the above, this other all-important advantage, that it offers the child a chance of life, while craniotomy implies the very certainty of death: and that while the existence of the sounds of the fœtal heart, and the continuance of the infant's life, would determine us to postpone, and postpone almost indefinitely, the delivery by embryulcio, the existence of these sounds, and the evidence they give of the continuance of the infant's life, would determine us to adopt, and form indeed, *cæteris paribus*, the very strongest reason for adopting, delivery by turning. In the one operation we would delay, and perhaps delay long, and hence dangerously, the delivery of the mother, because the child was living; in the other operation we would promote and hasten the delivery of the mother on exactly the same grounds on which we postponed it in the first, viz., because the infant was still living, and might still be extracted alive.

Perhaps the detail of one or two cases may impress this view more strongly than any more lengthened remarks. I shall select the first two from the Clinical Report of Dr. Lee of London, who, like Dr. Collins, rejects the employment of the long forceps when the head is delayed at the brim, and prefers the extraction of it by the perforator and crotchet.

CASE IX.—In a woman whose pelvis and extremities were distorted by rickets, the labor began on the 9th of February, 1842.[1] "The membranes," to quote Dr. Lee's own words, "ruptured on the 10th. On the 11th, the os uteri was not dilated to the size of half a crown, the head was wholly above the brim, and the base of the sacrum easily felt. Active pains came on and lasted during the night, and till two P. M. of the 12th. The os uteri was still very imperfectly dilated. The head, *entirely* above the brim, now much swollen. It was clearly right to interfere, which I did (by craniotomy), and she recovered without a bad symptom, though I now feel persuaded that she ought *not* to have been left so long in labor. The danger," he adds, "of rupture and fatal contusion of the uterus is great in all such cases, and delivery should be effected as soon as it is evident that the head cannot pass. I knew from the first, in this case, that it would not pass through the brim, but was *prevented from interfering in consequence of hearing the pulsations of the fœtal heart.*"

In the preceding case Dr. Lee states his conviction that the patient ought not, in reference to her own safety, to have been left so long in labor. The continuance, however, of the pulsations of the fœtal heart prevented him for some time from interfering and delivering by craniotomy. It is not stated whether the child continued alive or not till that operation was at last adopted. But the circumstance which thus directly deterred him from earlier delivery, viz., the life of the fœtus, would have directly urged him to the new practice which I am here venturing to propose in such cases. The reason he had for longer postponing the operation of craniotomy would have been the strongest of all reasons for at once preferring the operation of turning, and giving so far, a chance of life to the child, and at the same time preventing the continued duration of the labor from placing the mother in greater danger and jeopardy.

I quote the following case from the same author for the purpose of showing what I have omitted to insist upon in the course of the preceding remarks, that in instances in which the brim of the pelvis is contracted, the protracted duration of the labor may occasionally lead to the most deplorable morbid consequences on the part of the mother, even though it do not actually destroy her life. An earlier delivery by turning would, I believe, as has been already often stated, not only enable us to avoid these results to the mother, but offer a chance of escape and life to the child.

CASE X.—The patient[2] had borne nine children; all her labors had been difficult, and the two last so much so that artificial assistance was required, and the children were still-born. Her tenth labor commenced on the 3d of December, and the membranes were soon after ruptured. During the 4th the pains were feeble and irregular. Thirty-five drops of laudanum were given. On the morning of the 5th they returned, but again feebly and irregularly.

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[1] Clinical Midwifery, Case 113, p. 51.

[2] Ibid. Case 84, p. 40.

At ten o'clock on the evening of the 5th, two doses of the ergot were given, and soon several strong forcing pains were experienced. "The movements of the infant were not felt after this," and the uterine contractions appear to have ceased. When Dr. Lee visited her next forenoon—viz., at eleven o'clock on the morning of the 6th (three days after the labor had commenced and the membranes had been ruptured), the state of the patient was as follows: "The discharge from the parts has become offensive; bladder filled with water; the vagina is swollen and tender; the head greatly swelled and compressed, is firmly fixed in the brim of the pelvis, and the finger cannot be passed around it without giving great pain; the ear cannot be felt, the greater portion of the head being still *above* the brim." Dr. Lee opened the head, and two hours elapsed before he could complete its extraction by the crotchet. He thus continues the report: "December 7th. A bad night; retention of urine; great swelling and tenderness of the parts; pulse rapid; tongue loaded; headache; rigors. Sloughing of the vagina took place, and on the 14th a *fistulous* communication had been formed between it and the bladder. This unfortunate woman," adds Dr. Lee, "was soon after deserted by her husband, and has led a life of great indigence and misery ever since."

In offering some general concluding remarks upon this and sixty-five other cases[1] of "difficult labor from distortion of the pelvis, &c. &c., in which delivery was effected by the operation of craniotomy," contained in his second report, Dr. Lee observes—"In thirty-eight of the cases in this report, the labor continued from forty to seventy hours."—"In a very large proportion of the cases the difficulty arose from distortion, or a contracted state of the pelvis. Rupture of the uterus took place in three before perforation, and the inflammation and sloughing of the uterus, vagina, and rectum, which proved fatal to eight others, were chiefly or solely produced by the long-continued violent pressure on the soft parts by the head of the child *before* it was opened and extracted. In those who recovered with vesico-vaginal fistulæ or contractions of the vagina from cicatrices, the unfortunate occurrences arose from craniotomy being too long delayed."—"In several, had the delivery been *sooner* effected, the fatal consequences which ensued would have been wholly prevented."

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[1] Clinical Midwifery, 1842, p. 59.

In some of the following cases from Dr. Collins' report, the delivery of the patients by craniotomy was evidently postponed till such time as the child was ascertained by the stethoscope to have died. The state of the fœtal heart is not mentioned by Dr. Collins in all, but the omission was probably made in order to avoid unnecessary repetition; and except for this counter-indicating circumstance (the continuance of fœtal life), the mothers would probably not have been allowed to have had their sufferings protracted, or so much constitutional excitement created, before resorting to artificial delivery. In the last, or last two cases, the state of the mother probably ultimately necessitated delivery, independently of any considerations relative to the life of the infant. I shall quote the cases in Dr. Collins' own words.

CASE XI.—"Was admitted in labor of her eleventh child; uterine action very frequent and strong; the os uteri dilated to the size of a crown, and the fundus very much inclined to the right side. In twenty-four hours after admission (the head not having made any progress for the last eight), *the fœtal heart having ceased to act for some time*, it was thought advisable to lessen the head, and deliver with the crotchet."[1]

CASE XII.—"Was admitted in labor of her seventh child; she had been delivered artificially in her previous labors, and had but one child born alive. She had been ill a considerable time before she was brought to the hospital, and in nine hours afterwards, there being no progress made, the pulse 132, her strength much exhausted, and the child *dead*, she was delivered by the crotchet."[2]

CASE XIII.—"This patient was admitted in labor of her first child; uterine action was feeble, and continued so for seventy-two hours after she came in. As the *fœtal heart had ceased to act for some time*, and the pulse became hurried, it was considered advisable to deliver her. The *os uteri was not fully dilated;* the head was *high*, and resting on the pubis; it was lessened, and cautiously brought down with the crotchet."[3]

CASE XIV.—"Was reported to have been in labor two days before admission; the head was high in the pelvis, and pressing strongly on the pubis. It remained in this situation for twenty-four hours after, during which time she suffered great distress, uterine action being almost constant. This was her tenth child; she had been twice force-delivered in this hospital; dreading every moment rupture of the uterus, the head was lessened and brought down by the crotchet, as it had still remained so high as to be scarcely within reach of the finger."[1]

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[1] Collins' Practical Treatise on Midwifery, p. 480, No. 1032.

[2] Ibid. p. 480, No. 1005.

[3] Ibid. p. 481, No. 1038.

CASE XV.—"Was reported to have been twenty-four hours in labor before admission. About twelve hours after she came in, it was discovered that the face was turned towards the pubis, and pressing so strongly on the urethra that the catheter could with difficulty be passed. The pains continued strong for fifteen hours from this time, yet the head did not advance. It was deemed advisable to lessen it."[2]

CASE XVI.—"Was thirty-three hours in labor of her third child without having made the least progress for the last twelve. The bladder was forced down before the head. Her pulse became much hurried, and strength greatly exhausted, rendering immediate delivery necessary. The head was lessened, and the child brought away by the crotchet. It was very large; all her former children were still-born."[3]

CASE XVII.—"S. B. was forty hours in labor; it was her fourth child; all still-born. The head not having made any progress for the last twelve hours, and the fœtal heart having ceased to act, the delivery was effected by the crotchet; the face was turned towards the pubis. She had been force-delivered in this hospital two years ago, when the face presented—(see 'Observation on Still-born Children,' No. 441); pelvis very defective. The placenta in the present instance had to be removed by the hand, in consequence of hemorrhage; it was firmly adherent to the fundus of the uterus. She died on the ninth day, and on examination, extensive ulceration of the vagina was found, forming a communication with the rectum. At one point the vagina presented the appearance as if a laceration had taken place."[4]

In these cases delivery by turning, instead of by craniotomy, by being performed earlier, would, I believe, have preserved the patients alike from the sufferings and the dangers which the continued duration of the labor entailed upon them; and the children, instead of being necessarily sacrificed by the operation adopted and the delay attending upon it, might possibly have been saved by the adoption of turning, and by the earlier period at which delivery could thus have been practised. But it has been doubted whether the object I have just alluded to, namely, the preservation of the infant's life, could ever be the result of forcible extraction of the child through a contracted pelvis by turning. I shall next therefore notice this and other objections to the proposed practice on the allegation of danger to the mother, and disarrangement of the mechanism of parturition. The investigation of the objections urged on these several grounds will, I believe, leave us only with higher and more certain ideas of the real advantage of the proposed practice itself.

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[1] Collins' Practical Treatise on Midwifery, p. 470, No. 808.

[2] Ibid. p. 470, No. 526.

[3] Ibid. p. 470, No. 509.

[4] Ibid. p. 136, No. 79.

SECT. VI.—THE DEPRESSION AND INDENTATION DURING LABOR OF THE SIDES OF THE INFANTILE CRANIUM, IS COMPATIBLE WITH THE LIFE OF THE CHILD.[1]

Among the advantages of turning as compared with embryulcia, I have stated that "it gives the child a chance of life," and that the "neck of the child, if it be living or only recently dead, is so strong as to allow us to exert such a degree of traction upon the obstructed head, that the sides of the cranium may become greatly compressed or even indented under it; and that without necessarily destroying the child."[2]

As a comment upon these statements, Dr. Radford observes, "It has been said that the neck of the child, alive or recently dead, is so strong as to allow such a degree of force to be used as to greatly compress the sides of the cranium; *but*," he adds, "*such a procedure is at variance with all scientific views, and incompatible with the safety of both mother and child.*"[3]

The accuracy or inaccuracy of the two different, and indeed, opposite opinions, thus expressed upon the same point by Dr. Radford and myself, is one of those questions in pathology which no mere opinion or mere reasoning could ever settle. In order to decide it, we must have recourse to facts and experience alone. And it is of the highest moment in relation to the whole object of the present inquiry, that the evidence on the matter be as perfect and unequivocal as possible; for, in fact, the practice proposed is based in the main, on the fundamental idea that the required compression and indentation of the infantile skull is *not* incompatible—as craniotomy is—with the continuance of infantile life.

Now, the records of midwifery contain numerous accurately observed cases in which the cranium of the child has become greatly compressed or flattened, or actually depressed and indented in one of its sides, while passing through a very contracted pelvis, and that too, without necessarily destroying the life of the infant. Velpeau, for instance, the only author whom Dr. Radford quotes, tells us in a part of his work, which has evidently escaped Dr. Radford's observation, that the depression or "indentation (*enfoncement*) of the parietal or frontal bone, with or without fracture, has been noticed several times at the Maternity Hospital of Paris." * * * And "if," he observes, "the fracture or depression of the bones is not accompanied with effusion, nature ordinarily suffices to restore things to their primary state, and charges herself with the burden of the cure. Otherwise death is the usual consequence of them, or at least they produce drowsiness (*assoupissement*) and a great tendency to convulsions."[1]

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[1] From Provincial Medical and Surgical Journal, March, 1848, p. 141.

[2] Monthly Journal of Medical Science, March, 1847, p. 718.

[3] Provincial Medical and Surgical Journal, 1847, p. 405.

Chaussier, who tells us that he had reported some examples of these depressions and indentations of the infantile skull in 1807 and 1810, makes the following judicious observations upon their general pathology and effects. "Sometimes," he observes, "when the sacro-vertebral angle projects too far forward, we find on the portion of bone which rested on this prominence an indentation or depression (*enfoncement ou depression*), at which we often remark several small linear fissures (*fêlures*) in the form of a star, proceeding from the centre of the depression, and limited to the internal surface of the bone; at other times the bone is fractured in all its thickness, and its fragments are more or less separated, or forced in upon the brain; most commonly the infant dies in the act of delivery, or a short time after its birth." * * * * "These lesions, however, which would," he adds, "be always fatal for an adult, do not always terminate so badly in the new-born infant, for at this age the brain has neither the consistence, nor the action or use which it ought to have subsequently; they even heal easily and spontaneously if the infant be vigorous, and the labor has not been too prolonged."[2]

Various practical authors have left incidental notices of such cases of depression observed in the skull of the infant at birth, without the life of the child being compromised by the injury. Sometimes the injury has been the result of an excessive degree of compression by the forceps; but more usually it has been produced, as pointed out by Chaussier, by the head of the infant being strongly crushed against the projecting promontory of the sacrum, and that whether the infant passed as a cranial or footling presentation. Dr. Smellie, in his "Collection of lingering cases from a small, narrow, or distorted pelvis," prefaces his observations with this general remark, "that although these labors may seem to be of the same class, and require the same management with those that proceed from a large head, there is an essential difference; for though they are much the same with regard to the efforts of the woman, the operator in these has much less room when he is obliged to assist with his hand, and the child's head is disfigured and compressed into *large indentations*, occasioned by the jetting in of the upper part of the sacrum, and of the vertebræ of the loins."[1]

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[1] Traité des Accouchemens, tom ii p. 588.

[2] Recueil des Memoires, &c., sur divers Objets de Médecine Legale, Paris, 1824, p. 436.

In the course of his valuable volumes Dr. Smellie in different parts casually details several instances bearing out this remark, and at the same time showing that the injury is not necessarily fatal to the infant. I shall abridge some of these cases.

CASE XVIII.—In a woman,[2] "sickly from her infancy and very much distorted," Dr. Smellie effected the delivery of a living child by a double application of the forceps. "The forceps when first fixed had impressed the forehead, though the mark disappeared in five or six days after birth; but they made a very considerable impression when they were fixed a second time along the ears." The pelvis was "distorted from the jetting forwards of the upper part of the sacrum," and this projection had produced an *indentation*, distinct from those produced by the forceps; "the head was of a lengthened form, and contorted to one side, and there was a *deep impression* above the ear."—"Had the child been large its life could not possibly have been saved."

CASE XIX.[3]—The descent of the infant's "head was retarded by a jetting in at the middle of the sacrum," and the forceps were used. "The child's head was squeezed into a longitudinal form, flattened on the sides, with a *deep impression* on the cranium above the ears; and from an indentation on the os frontis by a blade of the forceps, which had been fixed on that and the occiput, I discovered the ears were not to the sides, as I had imagined. These impressions," the author continues, "had very much galled and inflamed the parts, but in consequence of proper care they digested, and the child recovered, and as he grew up the marks diminished and disappeared."

Dr. Smellie describes a case of depression and injury of the infant's cranium at birth that is still more important than the preceding two in reference to the subject of the present inquiry, for in it the cranial indentation was produced in following the very practice which I am discussing, viz., in turning the infant when the brim of the pelvis was narrow and contracted. The following are the principal particulars:

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[1] Collection of Cases and Observations in Midwifery, vol. ii. p. 358. 2d Edit. London, 1758.

[2] Ibid., vol. ii. p. 458.

[3] Ibid. vol. ii. p. 461.

CASE XX.[1]—A patient had a "narrow and distorted pelvis," in consequence of "the three lowest vertebræ of the loins bending forwards." The patient was delivered four times. At her first and third labors the infants were obliged to be extracted by the crotchet. Dr. Smellie tells us that he "managed her second labor from the beginning in a slow and cautious manner; but," adds he, "although the child was small, I with the greatest difficulty saved it by the assistance of the forceps." At her fourth accouchement the shoulder presented. Dr. Smellie tried ineffectually to perform cephalic version, and then with some effort seized one of the legs of the infant and turned it, and extracted it as far as the neck, the head still remaining above the contracted part of the pelvis. After using considerable exertion in order to drag it through the deformed brim, Dr. Smellie states that he began to despair of the child's life, but adds, "as there was still a weak pulsation in the funis, I resolved to make another effort with all my strength, by which the head was moved a little lower; then forcing up my fingers to the forehead, I got a firm hold of it, and finished the delivery." The infant showed some weak signs of life, and in about ten or fifteen minutes began to cry. "The infant," adds Dr. Smellie, "continuing to cry incessantly while the head was washing, I examined and perceived a large tumor above the right ear; I likewise found a *depression of the temporal bone* before the ear, and the frontal and parietal bones pushed outwards; these formed the swelling, and were the parts that stopped at the distorted bones of the vertebræ." The child was quieted by applying pressure on the tumor with a compress and bandage. Next day the swelling had disappeared. The mother recovered better than in any of her preceding labors.

Dr. Smellie relates another case,[2] in which the head of the child was apparently injured in a similar manner in dragging it through "a distorted pelvis."

CASE XXI.—The infant presented by the breech. After the body and extremities were born, "a good deal of force was used to deliver the head." Five minutes elapsed before the child breathed; at last it began to cry incessantly, "till one of the women observed a large swelling betwixt the left ear and temple." Pressure with the fingers first, and ultimately with a compress, quieted the child in the same manner as in the preceding case. "When I examined it," says Dr.

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[1] Collection of Cases and Observations in Midwifery, vol. iii. p. 230.

[2] Ibid. vol. iii. p. 95. Collect. 32, Case 11.

Smellie, "next day, the swelling was gone, and it appeared to have been that part which stopped so long at the projection of the upper part of the sacrum before the head was delivered."

Dr. Denman has recorded one exceedingly interesting case, in which the head of the infant was much injured during the version and extraction of it through a deformed pelvis, and that, as in the preceding cases of Dr. Smellie, without compromising the infant's life. The case is altogether so pertinent to the subject of the present remarks, and affords so much important corroborative evidence, that I feel assured I shall be excused in giving the details in full, and in Dr. Denman's own words:

Case XXII.—"Many years ago I attended a patient in two labors, in both of which there was a necessity of delivering with instruments, on account of the smallness and distortion of the pelvis, and neither of the children could be preserved. In her next pregnancy I made a proposal to bring on premature labor, to which she and her friends would not consent, and I was dismissed from my attendance. In the course of twelve or fourteen years she had five more children, not one of which was born living. In the forty-sixth year of her age she proved with child, and again applied to me. When her labor came on, the first stage was suffered to proceed without interruption, but when the membranes broke, I without delay passed my hand into the uterus, and easily brought down the feet and body of the child; but the head being stopped by the narrowness of the superior aperture of the pelvis, I was obliged to exert and continue much force, before it could be extracted. The child was born with very little or no appearance of life, but by the strenuous use of the common means recommended for this purpose, it was recovered. On the left parietal bone there was a depression of considerable extent, and to my apprehension, of full one inch in depth, occasioned by the projection of the sacrum, but the depressed part gradually rose. In the course of a few months the bone regained its natural form, and the child was for several years in good health, with its faculties perfect. The woman recovered without any untoward circumstance.

"But," adds Dr. Denman, in a cautious and contradictory note, "the success of such attempts to preserve the life of a child is very precarious; and the operation of turning a child, under the circumstances before stated, is rather to be considered among those things of which an experienced man may sometimes avail himself in critical situations, than as submitting to the ordinary rules of practice."[1]

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[1] Introduction to the Practice of Midwifery, p. 382.

In the second volume of her "Pratique des Accouchemens," Madame Lachapelle relates the following instance of footling presentation in which the child was born with a depression of the parietal bone:

CASE XXIII.—A woman, pregnant for the second time, had a large accumulation of liquor amnii, and the child floated so easily in it, that when labor pains first supervened, the presentation altered from time to time. The membranes at last were broken, and the left foot descended. By dragging at this foot, the lower extremities and then the trunk passed with facility. The left arm, however, got fixed between the head and pubis, and was disengaged with some trouble. "The head, however," to quote the author's own account, "although directed transversely, offered still greater difficulties in its extraction, either because the pelvis was a little distorted, or because our efforts were not absolutely directed according to the axis of the brim. At all events it is certain, that after the birth of the fœtus we found a *considerable depression* on the right parietal bone, which had rested upon the sacro-vertebral angle. In despite of this lesion, the infant, which was at first apparently stillborn, not only lived, but the depression was dissipated, and indeed was scarcely visible fifteen days afterwards. The mother did not experience the slightest bad symptom."[1]

Dugés has recorded two cases which came under his observation, in which the cranium was very deeply depressed and indented in the way described in the preceding instances, and yet both children lived and did well.

CASES XXIV. and XXV.—In one of these cases of Dugés the frontal bone was so much *depressed*, that the left eye appeared as it were protruded from the orbit; still the child neither suffered from convulsions, nor any other special symptoms. In the second case the child had come footling through a pelvis, the great [conjugate?] diameter of which was estimated at three and a quarter inches. (*Dont le grand diamètre etait evalué a trois pouces un quart.*) The head could not at first be extracted by the aid even of very strong tractions on the inferior jaw and shoulders, but scarcely were these artificial efforts momentarily suspended, when the head passed rapidly without assistance, and was expelled by the action of the abdominal muscles. The left parietal bone, which had pressed upon the promontory of the sacrum, was *depressed and indented* to the depth of half an inch, and throughout an extent of two inches. Two or three spoonfuls of blood were allowed to escape from the vessels of the cord, which yet beat with sufficient force, although the infant was apparently dead; and betimes the child became gradually reanimated, and began to cry, and passed meconium. It was troubled with constant hiccup, puckering of the lips, and torsion of the mouth to the left side; spasmodic closure of the right eye, immobility of the left; rigidity of the hands; fingers extended and separate, with the exception of the thumbs and the last phalanx of one of the index fingers, which were flexed inwards; rigidity of the arms and limbs. These symptoms were remittent, and returned in fits at intervals of ten minutes. Two leeches were applied to the left temple. The fits continued during the evening and night, but gradually became less and less severe. On the morrow the infant drank; it took the breast the following day; and in fifteen days after birth the cranial depression had almost entirely disappeared.

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[1] Pratique des Accouchemens, vol. ii. p. 166.

In his "Manual of Midwifery," the same writer (Dugés) observes, "The bones of the cranium are often fractured with indentation, by the sacro-vertebral angle of the mother. It is one of the frontal, or one of the temporal regions, which is the most common seat of these lesions. * * * These fractures often heal spontaneously; the indentations even rise in a few days."[1]

Jacquemier, one of the latest French writers on midwifery, and from whose work I have quoted Dugés' cases, states that these fractures of the fœtal cranium, with depression, are not by themselves a cause of certain death; "the depression," he remarks, "relieves itself by degrees, and after ten or fifteen days, there scarcely remain any vestiges of it."[2]

If it were required, I might cite other cases and remarks of the same description from other authors in evidence of the fact, that the state of compression and indentation of the bones of the infant's cranium at birth is not necessarily fatal to the infant's life. Some additional proofs will be quoted in the sequel; in the meantime I have, I believe, stated enough to show—*first*, that the neck of the child, if it be living, or only lately dead, is so strong, as to allow us to exert such a degree of traction upon the obstructed head, that the sides of the cranium may become greatly compressed, or even indented, under it; and, *secondly*, that Dr. Radford is truly wrong in stating that such a lesion and practice is incompatible with the safety of the child. On the contrary, the recorded experience of Velpeau, Chaussier, Smellie, Denman, Lachapelle, Dugés, Jacquemier, &c., &c., proves (as we have seen), that the depression and indentation of the child's head do not necessarily destroy the child's life. Nay, Dr. Radford's own experience on this point is, I know, not in favor of his own views, but of mine. In evidence of this I shall quote a case in point, recorded by Dr. Radford himself, from his own practice.

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[1] Manuel d'Obstetrique, pp. 358, 359.

[2] Manuel des Accouchemens, tom. ii. p. 773.

CASE XXVI.—"I was requested," says Dr. Radford, "by Mr. Dick, to visit a poor woman in New Blakely Street, who was suffering from protracted labor, caused by contraction of the brim of the pelvis. The long forceps were applied, and the child delivered alive. *A depression of considerable size was produced on the left parietal bone*, from the pressure it had sustained from the promontory of the sacrum. The signs which the child manifested were those of apoplexy. The funis was divided, but no blood followed, as the pulsation had previously ceased. The child continued in the same state during the day, and in the evening was attacked with convulsions, which were frequent. Two leeches were applied to the temples, the bowels were opened, and the child recovered."[1]

But farther, argues Dr. Radford, such a procedure as I propose is "at variance with all *scientific* views." I shall not stop at present to debate here whether the practice I suggest be "scientific" or not; but in a medical point of view I hold that appellation to be truly merited by any propositions and measures that tend, like the present, to promote the great essence and object of all medical science—namely, the preservation of human life; and in the various cases which I have quoted in the preceding sections, while none of the mothers were lost, the lives of the infants were at the same time all saved. But assuredly the heads of most of these children would have been opened by the perforator, and their lives thus destroyed, provided, in each instance, the structure of the cranium had not yielded, and its bony parietes become fortunately depressed and indented, to the degree required to admit of its safe transit through the contracted aperture of the pelvis.

SECT. VII.—PHYSIOLOGICAL AND PATHOLOGICAL REASONS FOR THE LIFE OF THE CHILD NOT BEING NECESSARILY COMPROMISED UNDER THE PROPOSED PRACTICE.[2]

It has been alleged that the preservation of the life of the infant is not compatible with its forcible extraction by turning through a contracted pelvic brim. It must perish, it has been urged, in consequence of the lesions it necessarily sustains, and especially—1st, in consequence of the injurious compression to which its head is subjected during the forced delivery; and, 2d, in consequence of the required injurious traction upon its neck. Let us examine, in detail, each of these alleged necessary causes of death to the child under the practice in question.

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[1] Essays on Various Subjects connected with Midwifery, p. 11.

[2] From Provincial Medical and Surgical Journal, April, 1848, p. 172.

1.—IS THE COMPRESSION TO WHICH THE INFANT'S HEAD IS SUBJECTED INCOMPATIBLE WITH ITS SAFETY AND LIFE?

In the remarks which I offered under the last section, with regard to the question whether, in turning in contracted pelves, the degree of injury to which the child's head is subjected, is or is not compatible with its future safety and life, I confined the observations which I made and the illustrative cases I adduced, to that form of cranial lesion in which the sides of the fœtal skull are both compressed and indented. In some instances, however, even when the pelvis is much distorted, one of these states only, as the lateral compression alone, *without* the indentation of the cranium, is all that is required in order to reduce the head to the size necessary for its transit. On one occasion, through a pelvis, the conjugate diameter of the brim of which was less than three inches, from projection of the promontory of the sacrum and disease of the symphysis pubis, I extracted a full-grown infant by turning; the head passing through in a flattened and compressed form, but without any depression or indentation upon it. It is, I believe, unnecessary for me to prove, that when the extraction can be effected with this single lesion, the child is not necessarily sacrificed; because I have already proved, that even when the two lesions of compression and indentation coexist, the presence of both forms of injury is not necessarily incompatible with the continuance of life. And sometimes by compression alone, more especially if the skull is not very fully ossified, the fœtal head may certainly be very greatly reduced in size without compromising life. Dr. Denman conjectures that, to quote his own expression, "it may be reduced one-third below its original size, without the destruction or even injury of the child from the compression."[1]

It would, however, be improper to leave this subject without attending to some anatomical and physiological circumstances, in the structure of the fœtal head, that enable it to bear this great degree of lateral compression which we have been considering, without the necessary destruction of life. Indeed, perhaps the very knowledge of these circumstances may impart to the mind greater faith in the correctness of the principles of the proposed practice.

We shall afterwards have occasion to see that by far the most frequent form of morbid contraction of the brim of the pelvis consists in the projection forwards of the promontory of the sacrum, and this is a kind of distortion especially adapted for the artificial delivery of the mother by turning, instead of craniotomy. Under this kidney-shaped pelvic brim, and other varieties of pelvic contraction, when the child is delivered by turning, the direction of the compression of the head is, as we have already found, lateral and transverse, or, in other words, in the line of its bi-temporal or bi-parietal diameters, these diameters being reduced and shortened by it.

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[1] Introduction to the Practice of Midwifery, 1816, p. 352.

Now, of all directions, the *lateral* is the one in which the child's head can endure the greatest degree of compression, with the least degree of danger. The power of diminution of the child's head under labor, "is greatest," observes Dr. Ramsbotham, "in the lateral diameter, and," he continues, "a full-grown fœtal head may be lessened from side to side, without endangering the child's life, one-seventh of its own extent, or from three inches and a half to three inches."[1] Dr. Ramsbotham states that this degree of compression of the head, namely, to one-seventh, may be accomplished without the life of the child being endangered. We have just now seen that Dr. Denman imagines that the amount of compression may be carried much farther, namely, to one-third, without the life of the child being necessarily injured and destroyed. No author has pointed out more correctly than Dr. Radford, the fact that the child's head will bear with comparative safety, a far higher amount of compression in its transverse or bi-parietal diameter, than in its longitudinal or occipito-frontal diameter. When the head is compressed in the first of these modes, or transversely, "the brain," he observes,[2] "bears this alteration in the figure of the cranium with comparatively little inconvenience, because the pressure it sustains is parallel with the fibres of some of those parts which lie between the two hemispheres, and with the falx, which in its natural state supports this organ. The pressure is also less injurious, because it is applied upon the sides of the vessels, but not in such a degree as considerably to influence their calibres." When, however, the direction of the pressure happens to be in the opposite or longitudinal axis of the head, the results are very different, and far more dangerous. Then "the alteration," Dr. Radford remarks, "which takes place in the head is as follows: A considerable diminution in the length of the occipito-frontal diameter is produced, in consequence of approximation between the frontal and occipital bones; the fontanelles become nearly obliterated, and the sagittal suture is wider and more prominent. The brain is pushed into this space, which is insufficient for its accommodation; its organization is injured, and the child, when born, is either dead or dies soon after birth. The pressure applied to the fore and hind part of the head has a tendency to change the relative situation of many parts of the brain. It forces one hemisphere from the other, which, if carried beyond a certain degree, will inevitably produce laceration of the coats of the veins which pass to the longitudinal sinus; and this danger is increased by the great congestion which exists. These serious effects," he again states, viz., apoplectic congestion, sanguineous effusion, and softening of the brain, "are produced by the brain being compressed in a direction contrary to the course of the fibres of some of those parts which lie between the hemispheres, and also to the current of blood along the longitudinal sinus." Dr. Radford adds, that injuries of this character are sometimes met with when the head has been compressed and dragged through the pelvis by the long forceps; and he gives two dissections of the fœtal encephalon to show these results, congestion, effusions of blood, &c., in two cases in which the delivery was accomplished by this instrument. The cause for instrumental delivery and the application of the long forceps is not specified in one of these cases; in the other they were used on account of distortion of the brim of the pelvis. And certainly, if we employ the long forceps to overcome this difficulty, and place them as Deleurye, Haighton, Davis, Ramsbotham, and other excellent operators have recommended, with one blade over the face or forehead and another over the occiput of the child, we in our extractive efforts, must compress the fœtal head with the instrument so applied, in its longitudinal or antero-posterior axis; and hence the actual direction of our pressure would endanger the child more than the actual degree of it. If, in the same case, and under the same obstruction, however, the child were extracted by turning, the required reduction of its head, by being effected entirely by lateral compression, would be effected, as we have already seen, not only with much greater facility but with much greater safety to the infant; and the chance of injury to the brain and of congestion and sanguineous effusion upon it would be greatly less.

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[1] Obstetric Medicine and Surgery, p. 25.

[2] Essays on various Subjects connected with Midwifery, p. 7, &c.

But, though the danger of cerebral congestion and its effects is diminished in lateral as compared with longitudinal compression of the child's head, yet it is by no means abrogated and removed; and in those cases of lateral compression and indentation of the cranium, which, under either footling or head presentations, prove fatal, it is this *effusion* and its dangers more than the mere *compression* of the proper structure of the brain, that seems to lead to this result. At all events, the cases I have already cited, in the last section, show that the cerebral tissue may be compressed, or, at least, displaced, in a lateral direction by the grooved and depressed cranium, not only without destroying or endangering life, but without producing coma or convulsions, or any greater lesion of function than a mere temporary asphyxia, provided always there be no serous or sanguineous congestion and effusion accompanying and resulting from it. The escape of the infant under this form of injury in which we are thus interested, is favored by other circumstances. When the fœtal skull and encephalon are decreased by pressure in one diameter, they become lengthened and increased in others. In the form, for instance, of lateral compression resulting from extraction through a distorted pelvis by turning, while the head is flattened and diminished in its transverse diameter, it is temporarily lengthened to a corresponding degree in its vertical and longitudinal diameters. The anatomical peculiarities of the fœtal skull, the existence of fontanelles and sutures between its component bones, and the consequent isolation and mobility of the individual bones themselves, freely admit of this; and so far under cranial depressions, no doubt, save the proper tissue of the brain from a deleterious and destructive amount of pressure in a way that, under the same form of injury, would be quite unavoidable with the cerebral contents of the solid adult cranium. Besides, the immunity of the infant at birth under such apparently severe injuries may partly be owing to another and a physiological cause. The embryo and fœtus possess a much lower physiological type of life than that which pertains to the adult; nor is the transition from the one to the other abruptly and entirely completed when the infant passes from its intra-uterine to its extra-uterine existence. It is stated by Billard,[1] and I could quote no more accurate observer, that symptomatic fever seems incapable of being excited in the constitution of the infant, by injury or otherwise, till the time of teething; and in all probability lesions such as these to which the crania and encephala are subjected at birth in the class of cases we have been considering, are less dangerous and fatal in their effects, in consequence of the general law, that the power of resisting and repairing injuries is greater in proportion as the type of animal life is lower. It is true, besides, that individual infants, like individual adults, have a power of withstanding injuries much more than others, and in a way that it is impossible to account for on any known principles.

Speaking of the differences among children in relation to the danger they incur, from compression of the head during labor, Jacquemier remarks: "One infant, of a feeble constitution, will resist a long compression by the parts of the mother, or by the energetic and reiterated action of the forceps; another, of a strong constitution, whose head has not been a very long time, or very strongly, compressed by the pelvis, or between the blades of the forceps, readily dies, or only has a few hours' existence."[1]

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[1] "L'absence de toute réaction fébrile, lors même qu'il existe des lésions graves chez les nouveau-nés; la promptitude, au contraire, avec laquelle la fièvre s'allume par la moindre cause chez les enfants qui ont dépassé l'âge de la dentition, imprime aux maladies de ces deux époques un caractére différentiel de la plus grande importance."—Maladies des Enfans, Preface, p. 2.

Lastly, let me observe that the kind and degree of ossification of the bones of the fœtal cranium at birth, admit of their yielding and becoming depressed in a manner that, no doubt, contributes to the safety of the infant. The ossification is as yet, in general, so imperfect that the flat individual bones, such as the parietal and frontal, are still sufficiently thin and flexible to allow of their being partially indented and altered in shape by strong pressure applied to one point of their surface. They are, at the same time, usually so elastic as easily to be made to regain their former shape. And this indentation and restoration may occur without any appreciable fracture. In the first case which I have related, the right parietal and frontal bones, when their external surface was carefully examined after the removal of the scalp, were found deeply depressed, but apparently not fractured. They afterwards fissured, however, at the site of the indentation when being dried and prepared for preservation. However, if the indentation is extremely deep, or the bones unusually ossified, no doubt fracture will take place, but still probably without danger, if without internal or encephalic effusion.

We have found Velpeau, and the other authors we have quoted, generally stating that the compressed cranium, and especially depressed portions of it, usually become spontaneously replaced and restored in the course of a short time after birth. This is, however, not always the case; nor is its occurrence absolutely necessary for the viability of the child. In evidence of this I shall content myself by citing a single illustrative case.

CASE XXVII.—In the course of the last winter, after lecturing on the subject of these indentations in the fœtal cranium, one of the students in my class afterwards showed me, on his own person, two deep impressions, one on each side of the cranium, which had been produced by the use of the forceps, and which had afterwards continued permanently depressed. He was 20 years old. On the right side of the head, a little above the ear, there were two pretty deep, curved fissures or indentations, from two to three inches long. On the left side there was a single shorter indentation above the commencement of the temporal ridge.

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[1] Manuel des Accouchemens, p. 770.

2. Is the Traction to which the Infant's Neck is subjected incompatible with its Safety and Life?

The answer to this inquiry need not detain us long; in fact, the proper reply to it has already been virtually given, when considering whether the existing degree of cranial compression and indentation was necessarily fatal or not. For if, as we have found, children can survive after being turned and extracted through so narrow a pelvic brim, that the sides of their crania are sufficiently collapsed and depressed against the sides of the contracted maternal pelvis, to admit of the complete transit of the head, then it is evident that the neck is not fatally injured under the proposed traction; for in all these cases the degree of traction upon it was adequate to produce all the results wanted, without destroying the child. In these instances the extractive force necessary to drag the head through the contracted pelvis was applied to the neck, and the neck was found sufficient to bear it without compromising the life of the child; and I believe, as I have stated previously, that this will generally, if not always, be found to be the case, "provided the child be living, or only lately dead." It will be very different, however, if the child has been for some time dead, and decomposing; the structures of the neck, certainly, will not then bear such traction as is required to extract a full-sized head through a distorted brim; nor is it of any practical importance that it should, for such cases are cases for craniotomy, the child being dead, and forming no element for consideration in the mode or kind of delivery that is followed.

But, independently of the above decisive and conclusive testimony, and even if it were wanting, it would be easy, I believe, to adduce such cases and observations from obstetric clinical reports and other works, as would, *à priori*, have rendered probable what these cases render certain, viz., that the degree of traction to which the child's neck is subjected under the proposed practice, is compatible with its safety and life. I shall, for the sake of brevity, confine my illustrations of this remark to a note or two of some cases recorded by Dr. Smellie, and some observations offered by the ever-cautious Dr. Denman. Dr. Smellie gives various cases in which he was evidently himself astonished, as other practitioners have often been, at the excessive degree of traction which the neck of the infant stood during delivery in natural or artificial pelvic presentations, without the life of the child being compromised by it. For example:

Case XXVIII.—In a breech case, where the woman was long in labor with a large child, and was suffering from some hemorrhage, Dr. Smellie brought down one foot, extracted the body and extremities, but the progress of the delivery was "stopped at the head," and required much traction for its transit, Dr. Smellie being "obliged to increase the force at every attempt." "I pulled," he observes, "so long, and with so great force, before the head was delivered, that I was surprised to find the child alive."[1]

CASE XXIX.—In the next instance which he reports, one of the breech presenting in a narrow and distorted pelvis, after the body of the child was born, Dr. Smellie brought down the right arm, and "helped down the forehead; but," he adds, "before I could deliver the head, I was obliged to bring down the other arm, and saved the child also, though a *good deal* of force was used to deliver the head."[2]

CASE XXX.—A young woman, of "a small size," was in her first labor. The thighs of the child, which was large, presented. The labor was very slow and lingering. Dr. Smellie brought down the feet, and effected the extraction of the infant. "I delivered," he remarks, "the head, but not without a good deal of force. The child was alive, which I scarcely expected."[3]

CASE XXXI.—The breech presented, and the pelvis was narrow, and the child was born living. In attempting to deliver the head, Dr. Smellie tells us—"I imagined it was impossible the child would be alive, as I found the neck was so overstretched," having met "with greater difficulty than any that I ever delivered in that manner."[4]

In his classical and highly practical introduction to midwifery, Dr. Denman has devoted a special section to the consideration of the practice to be followed, in order to extract the head through the brim, in cases in which preternatural presentations happen to be complicated with distortions of the pelvis. His observations are most valuable in regard to the necessity of combined perseverance and caution, but they are too long to quote in detail. He tells us that "it has been said that children have been sometimes born alive when the strongest efforts, and those continued for many hours, have been made, to extract the head in this position, but I have not been so fortunate as to meet with any such instances, a short space of time having generally been sufficient to frustrate my hopes, and convince me that the child was dead, though when the head has been detained a considerable time, a few cases have terminated more favorably than I could have expected, and I have been agreeably surprised with the discovery of some faint signs of life, which, by the assiduous and careful use of the common means, have been improved, and the life of the child at length perfectly recovered."

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[1] Collection 32, Case 10. [2] Ibid. Case 11. [3] Ibid. Case 18. [4] Ibid. Case 13.

"When we have," he continues, "in vain exerted all the force which we think reasonable and proper, and which, in some cases, must be more than any circumstance would be thought to require, it will be expedient to rest, for the purpose of gaining all the advantage to be attained by the compression of the head. On this account the mother will actually suffer no more inconvenience than would have been produced if the head had originally presented and been locked in the pelvis. After waiting some time, we must renew our attempts to extract, and thus proceed, alternately resting and acting with efficacy and resolution, and if the hold we may have of the body or extremities of the child does not suit, a silk handkerchief or other band may be passed round its neck, and this will be found a very handy and convenient instrument."

"It must be a *very great* disproportion," adds Dr. Denman, "between the head of the child and the pelvis, which is able to withstand this method of proceeding, if we persevere in it with prudence and steadiness; because the integuments of the head will burst, or the bones be bent inwards in an extraordinary degree, or even broken." If it fail, "it then only remains that we should lessen the head of the child, and the operation may be as easily performed in this as in the natural presentation of the head." "But it will," he continues, "be scarcely believed, how seldom this operation is necessary under these circumstances, if we have not been in a hurry, but have acted with prudence; nor have I ever known any ill consequences follow the compression which the soft parts undergo between the head of the child and the sides of the pelvis, if proper attention were afterwards paid to the state of the bladder and rectum."[1]

We have already found, that when the child is extracted by turning through a contracted pelvic brim, its life is not necessarily sacrificed, as has been alleged by some,—either, 1st, by the injurious compression to which its head is consequently subjected; or, 2d, by the degree of traction required to be made upon its neck.

One more special objection to the proposed operation of turning remains for our consideration, viz.:

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[1] Introduction to the Practice of Midwifery, pp. 497, 498, 499.

8. IS THE PRACTICE INCOMPATIBLE OR NOT WITH THE SAFETY AND LIFE OF THE CHILD IN CONSEQUENCE OF THE COMPRESSION OF THE UMBILICAL CORD?[1]

In speaking of the propriety of delivery by turning in the class of cases for which I have proposed it, Dr. Radford states as an objection to it, the danger of death to the infant from pressure upon the funis, and consequent obstruction of the umbilical circulation. In footling presentations, "the funis," he observes, "is subject to fatal compression;" and he imagines this danger must be greatly increased in cases in which the child has been turned on account of distortion of the maternal pelvis.

Perhaps the proper and the most direct answer to this, as to the other objections to turning, drawn from supposed dangers to the child, is simply a reference to the actual results of the cases which I have already quoted, and may yet have occasion to quote in the sequel. In many of these the child will be found to have passed or been drawn footling through a contracted pelvis, and yet, in despite of the alleged fatal danger of compression of the funis between the body or head of the child, and the interior of the maternal passages, the infant *has* been born alive. The infant has been subjected to the alleged cause of danger and death without the alleged effects following; and hence we are surely entitled to conclude that the influence of the cause is not so likely to interfere with the operation of delivery as has been theoretically presupposed.

Let me not be misunderstood. I by no means wish to argue that in turning in distorted pelvis there is no danger to the child from umbilical compression, for I believe quite the reverse; but I imagine, at the same time, that on first thinking over this undoubted source of danger, we are apt in our own minds to magnify its effects. Experience, as I have just now stated, teaches us that they are not so great as theory might lead us *à priori* to believe; and when we consider the subject for a moment we shall find, I think, sufficient reason for seeing—1st, that the risk and occurrence of umbilical compression in footling presentations and turning is not so very great as to invalidate the practice, and that it is in part averted by the very form of distortion to which the pelvic brim is most usually subject; besides, supposing, 2d, the compression to take place, there may still, I believe, be measures in the resources of science, by which, if we cannot now avert the occurrence of compression of the cord, we may prevent at least the more dangerous consequences of the complication for the child. I shall speak briefly of each of these points.

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[1] From Provincial Medical and Surgical Journal, July, 1848, p. 393.

The Danger and Mortality from Compression of the Cord is not so great as to invalidate the Practice of Turning.—Dr. Churchill has published a table of 542 cases of turning in which the result to the child is mentioned. In these 542 cases 182 children were lost, or rather less than 1 in 3. And when we recollect that no doubt in many of these 182 cases the child was dead *before* turning was adopted, and that in others the cause demanding the delivery, such as presentation of the placenta, was the cause also of the infant's death, &c., we shall see reason to believe that the chance of death to the child from the compression of the cord, would be found to be less than the chance of death from the application, and compression, and traction of the long forceps; for, even in the more common form of delivery by the short forceps—an operation which is generally looked upon as less dangerous to the child than delivery by the long forceps—the mortality among the infants is as great or greater than by turning.

Out of 871 cases of delivery by the forceps, and almost always the short forceps, collected by Dr. Churchill, 175 children were lost, or 1 in every 5.

Out of twenty-four short forceps cases which occurred in the Dublin Hospital during Dr. Collins' mastership, eight of the children were still-born, or 1 in 3. Out of thirty-three cases of presentation of the shoulder or arm which he treated by turning, in two the child was putrid, and in two others there was hemorrhage, in one, of the unavoidable, and in the other, of the accidental form, and death of the children. Among the twenty-eight remaining cases, nine were born dead, or nearly 1 in 3. Here, suppose we admit that the cause of death in these nine children was traceable to compression of the umbilical cord; still the infantile mortality was probably as little or indeed less than would be found to result from delivery with the long forceps.

Out of forty-two forceps cases related by Dr. Lee in his "Clinical Midwifery," in thirty-one cases the child was still-born, or 3 out of every 4 of the infants were lost. In thirty-nine instances in which he had recourse to turning in transverse presentations, twenty-nine of the infants were born dead, or about 3 out of every 4. His deliveries by the forceps were fatal to the children in 3 out of every 4 cases; and his deliveries by turning were fatal to the children in nearly the same proportion of 3 out of every 4 cases.

Out of ninety-three forceps cases related by Madame Lachapelle in her "Pratique des Accouchemens," in fifteen the children were still-born, or 1 in every 6 of the infants was lost. In sixty instances in which she had recourse to turning in transverse presentation, eleven of the infants were born dead, or 1 in every 5½. Her deliveries by the forceps were fatal to the children in the proportion of 1 in 6; her deliveries by turning were fatal also in a similar proportion, namely, about 1 in 5½. Of ninety-six forceps cases occurring in the practice of Madame Boivin,[1] in twenty the children were born dead, or 1 in every 5 of them was lost. In sixty-two cases in which she resorted to turning in transverse presentations, seven of the infants were still-born, or about 1 in 9 was lost.

To obtain with more certainty the general comparative average mortality of forceps cases and cases of turning in transverse presentation, let us combine the data above given from the practice of Collins, Lee, Lachapelle, and Boivin.

The following table exhibits in an abridged and simplified form, the results of this collection of cases:

TABLE VII.

Comparative Mortality of Children in Cases of Delivery by the Forceps, and by Turning in Transverse Presentation, in the Practice of Collins, Lee, Lachapelle, and Boivin.

Mode of Delivery.	No. of Cases.	No. of Children Still-born.	Proportion of Still-born Children.
Forceps cases,	255	74	1 in every 3 died.
Turning in transverse presentation,	194	58	1 in every 3½ died.

Arrangements and Means by which Compression of the Cord, and its Effects, may be averted.—Dr. Radford believes that when the child is extracted by turning on account of distortion of the maternal pelvis, the danger of fatal compression of the umbilical cord is then greatly increased by the fact of the distortion. But the common variety of contracted pelvis requiring turning is (as I have already stated, and shall insist more upon afterwards), the peculiar and common kidney-shaped distortion of the pelvic brim, which is produced by the anterior prominence and projection of the sacral promontory. When the fœtal head enters it, as it does under extraction by turning, with its long, or occipito-frontal diameter lying in the transverse diameter of the brim, then we have (see Fig. 30, page 463), a free space left, of greater or less size, between the face or forehead of the child, and the ilium to which it looks. In this space sufficient room may certainly, in general, be found for the free lodgment of the cord without injurious compression. The very narrowness of the conjugate diameter of the brim prevents this space from being entirely filled up and encroached upon, by preventing the great mass and breadth of the head, from the coronal suture backwards, from further passing over to this partially occupied side of the pelvis. Besides, this uncompressed lateral space is the very site along or near which the cord necessarily passes in descending from the uterus, and stretching between the placenta and umbilicus. For the anterior surface of the abdomen of the child, to which the lower end of the cord is attached, always, of course, looks to that side of the pelvis to which the face is directed; and it is on that side that we have the free and uncompressed space I speak of, as fitted for the safe transit and lodgment of the cord. In thus stating the space as free and uncompressed, I mean in so far as the adjoining opposite surfaces of the child's head and interior of the pelvis are concerned. The space itself may be filled up, or the cord in it pressed upon by the circle of the cervix uteri or other soft parts, but, certainly, a little adjustment and management may often prevent any fatal compression from these sources; for the cord may be carried round by the accoucheur to that point in which there appears the greatest space, and consequently the greatest freedom from compression; and he may sometimes defend it from the pressure of the cervix by allowing it to pass between two fingers, introduced either for the sole purpose of protecting it, or with the view of simultaneously assisting the extraction of the head by applying some force to the cheeks or lower jaw. In both natural and artificial footling presentations it has even been proposed to defend the cord more methodically, by shielding and enveloping it at the part liable to compression in a kind of special protecting instrument. Dr. Joos of Schaffhausen has ingeniously proposed an instrument for this purpose.[1] It consists of a tube, from four to eight inches in length, and of calibre sufficient to contain the umbilical cord, leaving a longitudinal slit or fissure equal to $\frac{1}{12}$th of its circumference. It is composed of caoutchouc or leather, in which are embedded a series of steel rings three lines broad, and at the distance of one line from each other. The slit is opened up at one end by the finger, so as to admit the umbilical cord, and in the same manner it is made to receive it along its whole length, the fissure closing up as the finger is removed. A handle may be required subsequently, to keep the instrument in its proper place. "The application of the instrument," says Dr. Joos, "takes place during the act of birth, by presentation of the breech or feet, in cases where delivery is effected by version and bringing down the feet, and in cases where the cord is prolapsed and cannot be returned." But I greatly doubt whether, even if we were provided with such an instrument as Dr. Joos describes, we should not find the difficulties of applying it, and the time required for that purpose, more dangerous to the infant than the extraction of the child's head by more simple and direct means. It is always well to avoid instruments when we can avoid them; and such a one as the present might not be used without some danger to the structures of the os uteri.

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[1] Memorial de l'Art des Accouchemens, Table, p. 464.

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[1] Medical Gazette, Oct. 22, 1847.

Suppose, however, that the means we used to avoid compression of the cord, by placing it in the least occupied point at the brim, or by preventing the soft parts from pressing upon it, by the fingers, or otherwise, were in vain, and that at last it really became irretrievably compressed, and its pulsations arrested before the head could possibly be extracted, is the safety of the child utterly hopeless? I believe not. Long ago, Pugh and Morlanne recommended a practice that has been latterly followed by Bigelow and others, and which I believe, is calculated in some instances to enable us to continue the life of the child, by continuing its respiratory function, whilst we go on exerting from time to time, those cautious yet decided means, which we have seen Dr. Denman recommending, for effecting the complete extraction of the obstructed head. The means to which I allude consist in exciting pulmonary respiration in the half-born child, now that its placental respiration is stopped, and in making it continue that vital process by the action of the lungs, now that it is prevented from being exercised by the medium of the placenta and umbilical cord. "When the parts are well made," says Pugh,[1] "and the child in proportion, happy the case! It will come then any way; the arms being brought down, the head only remains to be extracted, which must be done with as much expedition as possible, as indeed the arms ought to be; for, consider, when the child has passed the navel, the circulation between it and the mother is stopped from the pressure of the umbilical rope; you must then introduce the fingers of your left hand into the vagina, under the child's breast, and put the first and second fingers into the child's mouth pretty far; so far, however, that you are able to press down the child's tongue in such a manner, that by keeping your hand hollow, and pressing it upon the mother's rectum, the air may have access to the larynx; you will soon perceive the thorax expand, as the air gets into the lungs. *By this method,*" he adds, "*of giving the child air, I have saved great numbers of children's lives which otherwise must have died.*"

Acting on the plan thus suggested by Pugh, we may in some cases be able, with a little management, to change thus the respiration of the infant from its intra-uterine to its extra-uterine type though the head be still held and arrested at the brim; for the mouth of the infant is always then within reach, and could I believe be used for the purpose of admitting a proper quantity of air into the pulmonary passages, even when the great bulk of the head is still held above the brim. It is a practice, at all events, which sometimes succeeds sufficiently well when the head is down in the pelvis, and we have all the difficulties of a rigid outlet and perineum to contend against, in our attempts either to extract the head, or to admit a sufficiently free access of air to the mouth of the fœtus. The two following cases which I extract from Dr. Lee's "Clinical Midwifery," will illustrate and impress the practice and the remarks upon it, which I have just made:

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[1] Treatise on Midwifery, 1754, p. 49.

CASE XXXII.—The nates presenting, the trunk and extremities were extracted, "but the head could not be drawn through the external parts, from the rigid state of the perineum, and the pulsations of the cord were becoming more and more feeble. So great was the resistance of the perineum, that it was impossible to overcome it without destroying the child. I pressed back its edge, however, so far, that the external air could enter the mouth of the child and it respired in this way fully twenty minutes after the pulsations of the cord had ceased. In spite of all our care the edge of the perineum gave way as the bulky part of the head passed through the external parts, but the child sustained no injury, and continued to live."[1]

CASE XXXIII.—"I attended a lady in her first labor, on the 22d of October, 1837. The nates presented, and the cord ceased to pulsate after the trunk and extremities of the child had been extracted. The perineum was so rigid, that the head could not have been delivered without using so much force, that the parts must have been torn, and the neck of the child injured. I held the body of the child as far forward as possible, while Dr. H. Davies assisted me in holding back the perineum, that the air might enter the mouth of the child. The respiration went on for nearly half an hour before the head could be safely drawn into the world, and during the whole of the time there was no pulsation in the cord. The child is alive, and the perineum was not injured."[2]

The child may not only be able to breathe, but even heard to cry, with the head still unborn, when this form of treatment is adopted. In his paper on the subject, published in the American Journal of Medical Sciences for 1829, Professor Bigelow (of Boston) states, amongst others, the following case:

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[1] Clinical Midwifery, Case 239, p. 129.

[2] Ibid. Case 241, p. 131.

CASE XXXIV.—In a case of arm presentation, in which the feet were brought down, and the body delivered, "the face turned towards the perineum, the mouth was easily reached, and the fingers were opened to give passage to the air as before described. No struggle nor attempt at respiration however occurred. A handful of cold water was then dashed upon the body, upon which the child immediately gave a spring, and began to cry. The head was not delivered till some minutes afterwards."[1]

In his observations and directions upon this modification of practice, Dr. Bigelow observes: "After the body is expelled, if the head can be seasonably delivered, either by the recurrence of pains, or by the successful efforts of the practitioner, no difficulty ordinarily occurs. But this desirable state of things cannot always be realized; too frequently the size of the head, and the resistance of the pelvis or soft parts, renders the delivery difficult or hazardous, and the practitioner, in the midst of his efforts, is apprised by a convulsive jerk, or spring of the body, that a state of extreme danger exists, and that the time has come at which the child must breathe, or will speedily die. If at this period the fingers be introduced, so as to reach the mouth of the child, it will be perceived that each jerk of the body is attended with a gasp, and convulsive effort at inspiration, performed by the mouth and chest of the child. In this state of things, if air be conveyed to the mouth of the child, it will immediately breathe, and the efforts of nature, as will hereafter be shown, may, in most cases, be safely waited for to assist in expelling the head."[2]

"The method to be pursued," he continues, "in conveying air to the mouth, depends upon the situation of the head. If the chin has descended low in the pelvis, so that the mouth rests upon the perineum, or lower part of the sacrum, and can be readily reached by the fingers, the hand of the operator alone is sufficient to give the assistance required. But if the mouth is situated so high in the pelvis as to be reached with difficulty, or if, from the large relative size of the head, there is much compression, the assistance of a tube may be of use. The mode of proceeding which I have found successful in various instances is as follows: As soon as the body and arms are extracted, supposing the face towards the sacrum, an assistant supports the body, carrying it towards the pubis; or the reverse, should the position of the face be to the pubis. The accoucheur should then introduce the hand to which the face looks, till the middle fingers rest upon the mouth of the child; the hand is then to be raised from the throat of the child, making the ends of the fingers a fulcrum, and pushing the perineum backwards; the air will thus pass upwards as far as the chin of the child; the middle fingers are now to be separated about half an inch from each other, and thus a complete passage will be formed between them, by which the air will reach the mouth of the child. If the child be in a healthy state up to this period, it will immediately breathe and cry, and the delivery of the head may be safely postponed till the natural pains recur. If, from any degree of asphyxia, the child does not immediately breathe, it may often be made to do so by dashing cold water upon the body, or by other stimulating processes. It has even appeared to me practicable to inflate the lungs, in some cases, through an elastic catheter. When the mouth is so high in the pelvis as to be reached with difficulty, or when the compression is so great as to obliterate the cavity between the fingers, a flat tube will be found useful, made of metal, of spiral wire covered with leather, or of elastic gum, and having its largest diameter about half an inch. If the tube be of metal, or of any incompressible material, it should be withdrawn during a pain, to prevent contusion of the soft parts, and immediately replaced if the pain subsides without expelling the head. Such a tube may be considered as a prolongation of the trachea, and is fully sufficient to sustain life by respiration for a considerable time. The tube must be guarded and directed by keeping it between the fingers of the inserted hand."

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[1] American Journal of Med. Science, Aug. 1829, p. 288, Case 3.

[2] Ibid. p. 285.

In addition to the above remarks on the possible means of preventing fatal compression of the umbilical cord, there is one observation more which I am anxious to make. If all the preceding measures fail to avert compression, there is always still one strong hope left, namely, that if after complete compression of the umbilical vessels has occurred, no very great length of time is lost in the extraction of the head, the child may yet be born alive. For the type of vitality in the unborn infant is such that it will often survive, and be capable of being resuscitated, although the placental respiration be arrested for several minutes before its actual birth. And with others, I have been repeatedly surprised, in such cases as we are considering, both at the great amount of extractive force which the structures of the infant will sometimes undergo without life being destroyed, and at the apparently great length of time during which, in making these extractive exertions, the infant will still survive, and that too, although the cord was compressed during their continuance. If the infant be perfectly viable, and its heart sounding normally at the time that turning is adopted, there are perhaps comparatively few cases in which it will be requisite to expend so much time upon the forcible compression and extraction of the head, as to destroy during that time the child's life by simple compression of the cord alone. The following case, which occurred within the last few days, may perhaps impress this remark. In it the fœtal heart continued to beat regularly, not only after the cord was compressed for a considerable time, during which very strong exertions were made in order to extract a large and very firm head through a pelvis three inches in its antero-posterior diameter, but its pulsations went on for several minutes, even after the head was perforated, an operation that required some time for its performance, and after the cervical spine had yielded under the traction applied.

Case XXXV.—With Drs. Ziegler and Weir, I delivered at the Maternity Hospital, on April 17th, a woman aged 19, in labor with her first child. The os uteri was fully dilated, and the child's head had remained at the brim during nearly ten hours of active labor-pains. The parietal bones were riding over each other at the sagittal suture, and the right parietal bone was somewhat indented by the sacrum, upon which it rested. The conjugate diameter measured about three inches. A very deep dose of chloroform was given. Turning was effected, and the body of the child delivered. Strong traction was applied in vain to bring the head through the contracted brim, and at last the bones of the neck threatened to give way. The cranium was then perforated behind the ear, and by the aid of a crotchet in the mouth the delivery was completed. The child did not breathe, but the pulsations of its heart went on regularly for several minutes after delivery. The child weighed upwards of seven pounds. It measured twenty-one inches in length. Its head was large and firmly ossified.

CONCLUDING REMARKS REGARDING THE OBJECTIONS TO THE PROPOSED PRACTICE FOUNDED ON THE ALLEGED DANGER AND DEATH OF THE CHILD.

In terminating the present section of our subject, I would again repeat in regard to the whole, the argument which I have used against the alleged fatality of the individual sources of danger that we have inquired into. It has been averred that under the proposed practice of turning in distorted and contracted pelvis, the injury which the fœtal head must sustain would be fatal to the child—the injury of the neck would be fatal to it—the injury of the cord would be fatal to it. I have shown the principal fallacies to which these arguments are individually liable. Against the truth of the whole in a combined form, I would here again simply and strongly appeal to the evidence of facts and experience. For the results of the cases of the practice that I have given, and of others that I may yet have occasion to give, amply attest that in various instances in which it has been adopted, the indentation and depression of the skull of the infant were not fatal to it, nor was the traction of the neck fatal to it, nor the pressure upon its funis. It was submitted to one and all of these sources of supposed danger and death, and yet escaped alive from all. And having done so when the principles and details of the practice were not at all understood, may we not expect still happier and more successful results when these principles and details come to be better comprehended, and more systematically acted upon?

I have heard turning objected to by some, in arrestment of the head at the brim, on the ground of its inevitably occasional, or rather frequent, fatality to the child. But when we have any new operation proposed, with the view of replacing other operations, such as turning in distorted pelvis, instead of delivery by the long forceps or craniotomy, we can only properly judge of the real and relative value of the new operation, not by looking at its own absolute and isolated results, but by looking at its results as compared with the results of those other operative measures for which it is suggested as an alternative. Now, as regards the respective value of turning, to the infant, in comparison with the two operations for which I propose it as a substitute, the case is, of course, very different, according as we contrast it, *first*, with the long forceps; or, *secondly*, with craniotomy.

We have seen that the fatality to the child from the common operation of turning in transverse and cephalic presentations, was, in the Dublin and Parisian hospitals, nearly the same as the common operation with the short forceps, or indeed was in favor of turning, there having been in neither set of cases any very special state of pelvic contraction; and perhaps, if we had any adequate data, we should find that the same analogy in their results held good when they were respectively applied under greater and increased degrees of difficulty, as in morbid contractions of the pelvic brim, always, however, with this important difference in favor of turning—that it could be employed at a much earlier, and, consequently, as respects both the mother and child, at a much safer period of the labor than was fitted for the adoption of delivery by the long forceps; and always also, recollecting that the comparison of the results of turning, with the results of short forceps operations is imperfect in this respect, that from the direction of the compression in long forceps operations, viz., in the antero-posterior or oblique diameter of the fœtal head, the danger and mortality to the child is generally far greater than when the short instruments merely are used, the direction of the compression with them being transverse.

But delivery by the long forceps, under any conditions, is an operation entirely rejected by many eminent accoucheurs, and very seldom indeed adopted in country practice. And I have already often stated, but cannot, perhaps, too often repeat, that it is principally as a substitute and alternative for craniotomy in contracted states of the pelvic brim, that I bring forward the operation of turning. Now, as far as regards the child, the results of these two modes of delivery—craniotomy and turning—in this special and not unfrequent set of cases, is easy of comparison and settlement. For the very essence of the one operation, craniotomy, is the actual death and instrumental destruction and mutilation of the child. The object of the other operation, turning, is to offer the child, under the very same circumstances, a chance of escape and of life. Craniotomy implies the inevitable sacrifice of the infant. Turning implies its possible safety and probable survival. And the distinction between them which has latterly been always uppermost in my own mind, is this—delivery by craniotomy consists in opening and breaking down the skull so as thus to reduce it to such dimensions as will allow it to pass through the contracted pelvic brim; but the operation which I propose to substitute for it, and by which the requisite diminution of the fœtal head may likewise be produced, consists, not in fatal opening and emptying of the skull, but in compressing, and, if necessary, depressing its flexible and elastic parietes; and further, I have shown that extraction by turning affords us the means of effecting this compression and indentation without always compromising the life of the infant. As far then as regards the safety and life of the infant, I hold that any general comparison between craniotomy and turning is altogether and entirely in favor of the new practice. But how stands the comparison between the effects of the two operations in relation to the safety and life of the mother? To answer this question a separate section is required.

SECT. VIII.—SUPPOSED OBJECTIONS TO THE PRACTICE IN RELATION TO THE LIFE OF THE MOTHER.[1]

In the last section, when treating of the effect upon the infant, of the practice which I propose in distortions of the pelvis, I took occasion to state, that the only proper test or measure of value between a novel operative measure like this, and those operative measures for which it is suggested as a substitute, consists in ascertaining, not the absolute mortality attendant upon the new operation alone, but the comparative mortality attendant upon the new as compared with the other and older forms of treatment. In the same way, in the present section, the question is not—what is the absolute maternal mortality attendant upon delivery by turning in cases of contracted pelvis? but, what is the mortality attendant upon that operation, as compared with the mortality accompanying the alternative operations of delivery by the long forceps and by craniotomy?

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[1] From Provincial Medical and Surgical Journal, October, 1848, p. 533.

In the records of midwifery, there do not yet exist, so far as I am aware, any data by which it is possible to compute either the general or relative mortality to the mother, from the use of the long forceps? Nor can we obtain any satisfactory approximation to the probable results of their employment upon the mother, by studying the maternal mortality attendant upon the use of the short forceps. For the short forceps are employed only in cases in which the head is already sunk down into the pelvic cavity, and when the application of the instrument is far less difficult, and its effects less dangerous, than when the long forceps are had recourse to in consequence of the child's head being arrested high up in the passages, from the obstruction of a distorted pelvic brim.

It is principally, however, as an alternative for craniotomy that we have to consider the propriety of turning; and there exist numerous data by which we may compare the relative mortality of these two operations. On this and other points in the statistics of midwifery, the laborious and valuable investigations of Dr. Churchill have thrown much light. The following table, which I have drawn up from Dr. Churchill's collection of data, shows us the general relative mortality attendant upon craniotomy and turning, in so far as regards the life of the mother.

TABLE VIII.

Showing the Results to the Mother in Craniotomy and Turning.

Operation.	Number of Cases Collected.	Number of Mothers Lost.	Proportion of Mothers Lost.
Craniotomy,	251	52	1 in 5.
Turning,	169	11	1 in 15.

In some of these cases of turning, the deaths may, as Dr. Churchill points out, have been owing to the cause which demanded the operation, such as placenta prævia, and not owing to the operation itself; but, I believe with him, that the resulting calculations are "not very far from the truth."[1]

So far, such an imperfect estimate of the mere mortality of the two operations gives a result highly in favor of turning. But there is another element which comes into action, and which is not included in the above comparison. Most of the turning cases were instances in which the operation was performed in consequence of the child presenting transversely, and with most of them comparatively little or no difficulty was probably encountered in getting the head to pass the pelvic brim. In the class of cases in which it is my object to recommend turning—viz., with the brim narrow and distorted—would the operation, from its requiring the forcible extraction and compression of the head, be attended with much increase of peril to the mother?

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[1] Principles of the Practice of Midwifery, p. 245.

On such a question being first suggested, most accoucheurs, I believe, would be inclined, *à priori*, to argue that the compression, however transitory, of the soft tissues included in the maternal pelvis, between the interior of the maternal pelvic bones and the solid though elastic head of the infant, would be sure to be attended with inevitable danger and evil consequences to the maternal structures; that the practice would, in short, be, as Dr. Radford has expressed it, "incompatible with the safety of the mother."

Prejudgments, however, even in pathological and practical matters of this description, are often wrong. Let us lay aside argument for experience, and supposition for fact, and examine, quietly and without prejudice, the actual and probable results of forced operative extraction of the infant's head in cases of obstruction of the pelvic brim.

Dr. Denman had evidently some experience of the forcible extraction of the infant's head through contracted pelves, in the preternatural presentations of the child. The rules of treatment, which I have already quoted from his chapter "On the Complication of Breech Cases with Distortion of the Pelvis," show how entirely, or almost entirely, he trusted in these instances to the forced extraction of the head, and how very rarely, if ever, he had occasion to assist the delivery by craniotomy. And he states the results of all his observations in these instances, in terms most significant of its safety. "Nor have I *ever* known," says he, "any ill consequences follow the compression, which the soft parts of the mother undergo, between the head of the child and the sides of the pelvis, if proper attention were paid to the state of the bladder and rectum."[1]

The cases I have cited from Dr. Denman, and others adduced in the preceding sections, so far afford corroborative evidence of this important remark. Altogether, I have already referred to twelve or thirteen cases, in which the force of extraction or expulsion was so great, that the head was not only compressed but indented. In none of these does the mother seem to have suffered from the compression or contusion. But these cases, it may be argued, are too few to entitle us to speak over positively on the point. Let us turn then, to others.

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[1] Practice of Midwifery, 1816, p. 469.

It often enough happens, among cases of pelvic distortion, that when the head of the child presents, and becomes obstructed at the brim, the mere perforation of the head, or first part of the operation of craniotomy, is not adequate to allow the operator to overcome, with subsequent ease, all the resistance of the obstacle opposing delivery. In many cases, the second or succeeding part of the operation of craniotomy, viz., the extraction of the head with the crotchet, through the contracted and obstructing brim, is still attended with difficulty, and requires for its completion more or less prolonged exertion by pulling and force. Under such circumstances, and though the child presents by the head and not by the feet, we have exactly the same form of danger threatening the soft structures of the maternal pelvis, as if the excessive traction were made, not in craniotomy, but in turning. The two cases agree in this one common condition, that in both, an exertion of traction and force is required to pull the obstructed head through the contracted pelvic brim; and in both, the soft tissues lining the pelvis of the mother are equally liable to be contused and compressed, under this traction and force.

Now, in cases of craniotomy, such as I here allude to, and which require the operator to use *great exertions* in dragging the head with the crotchet through the obstructing brim, is the mortality to the mother much increased above what it is in the common run of craniotomy cases? Or, to state the question in other terms, are such cases of craniotomy decidedly more dangerous and fatal than other cases of the same operation, in which such excess of traction is not required?

Dr. Lee's "Clinical Midwifery" presents us with a series of data relative to craniotomy operations, that may enable us to arrive at some accuracy of information regarding this question. In the first and second Reports of his work, Dr. Lee details eighty-seven cases of craniotomy. Table IX. gives a condensed view of the principal facts and results connected with these eighty-seven cases.

TABLE

Analysis of 87 *Cases of Craniotomy, recorded by Dr. Lee,*

No. of Case.	No. in Report.	Cause of Obstruction and Delay.	No. of Pregnancy.	Length of Labor before Operation.
1	1	Rigid os uteri; vagina swollen and tender,	1	Three days and nights,
2	2	Head never through brim—convulsions,	1	Fifty hours,
3	4	Head jammed in brim,	. .	Above 48 hours,
4	5	Head not passed brim. Os uteri half dilated,	2	Two days (48 hours),
5	8	Head not passed brim,	1	Nearly 60 hours,
6	8	Scalp tumid; bones riding,	2	Strong, above 30 hours,
7	8	Head wedged in brim,	3	. . . .
8	9	Impacted in brim,	..	. . . .
9	16	Not through brim. Os uteri rigid, not fully dilated.	1	About 50 hours,
10	17	Head low but not through the pelvis; anterior lip of uterus locked; labia swollen.	1	About 48 hours,
11	25	Head and arm jammed in brim,	2	Above 30 hours,
12	29	The greater part of the head had passed through the brim.	. .	Forty-six hours,
13	30	Great part of the head still above brim,	1	Above 20 hours,
14	31	Head not completely in the cavity of pelvis,	. .	. . . .
15	32	Head so low that the ear could be felt,	1	Above 72 hours,
16	34	Head sufficiently low for the forceps,	1	So long that she was quite exhausted,
17	35	Head filled the cavity of the pelvis,	1	Above 41 hours,
18	38	Cavity of the pelvis occupied by a tumor the size of a cricket ball, or larger.	. .	Twenty-four hours,
19	39	Os uteri not fully dilated, thick and rigid, and pressed down with the head through the brim of the pelvis.	1	Above 50 hours,
20	40	Head firmly impacted in brim of the pelvis,	1	Above 30 hours,
21	41	Distortion of pelvis, and hard cicatrix of vagina,	. .	. . . .
22	42	Head jammed in the brim,	1	Long in labor,
23	43	Head within reach of forceps,	1	. . . .
24	56	Pelvis distorted,	1	Sixty-seven hours,
25	57	Face presentation,	. .	Above 48 hours,
26	58	Head and arm,	1	Seventy-two hours,
27	59	Head long fixed in the brim,	3	Forty hours,
28	60	Head many hours impacted in the brim; vagina swollen and tender.	1	Thirty-six hours,

IX.

in the First and Second Reports of his "Clinical Midwifery."

Difficulty or not in Extraction.	Death or Recovery of Mother.	Other Results, Inflammation, Sloughing, &c.	Remarks.
Not stated, . . .	Recovery,	Sloughing; fistula, .	Long forceps tried first.
Not stated, . . .	Recovery,	Violent inflammation of uterus,	Long forceps tried first.
Required 1½ hours, bones of head, &c., much torn.	Recovery,	Post-partum hemorrhage,	Tried first to apply forceps.
Great difficulty, .	Recovery,	Slight uterine inflammation,	Tried first to apply forceps; craniotomy in former labor.
Long cont'd efforts,	Recovery,	. . . .	Recovered rapidly.
Little difficulty, .	Recovery.		
Not stated, . . .	Death, . .	. . . .	Ergot and repeated efforts with forceps tried previously.
Strong efforts, . .	Death, . .	. . . .	Forceps tried first; vagina enormously swollen.
Force required, .	Recovery,	. . . .	Not able to apply the forceps. No bad consequences followed.
Not, except in overcoming peririneum.	Recovery,	Inflammation and sloughing of vagina and cicatrix.	
. . . .	Recovery,	. . . .	Delivered of first child by forceps.
With difficulty, . .	Recovery,	No bad effect, . .	Forceps tried first.
Not stated, . . .	Recovery,	Perineum torn by shoulders of infant,	Forceps tried first; delivered since without assistance.
Great force, . . .	Recovery,	. . . .	Forceps tried first. A face presentation.
Great force, . . .	Recovery,	. . . .	Forceps tried first.
Tedious & difficult,	Not stated,	. . . .	Forceps tried first.
Not without much difficulty.	Recovery,	. . . .	Forceps tried first.
Much difficulty, .	Recovery,	. . . .	Forceps tried first. Next child delivered naturally at full period.
Difficult, ...	Not stated,	. . . .	Not able to apply the forceps.
Difficult, ...	Recovery,	Retention of urine; violent inflammation of vagina; fever.	
Not stated, . . .	Recovery.		
Not stated, . . .	Recovery,	. . . .	Not able to apply the forceps.
Not stated, . . .	Death, . .	. . . .	Convulsions. Forceps tried first.
Tedious & difficult,	Recovery,	Slight uterine inflammation.	
Not stated, . . .	Recovery,	Hemorrhage, . . .	Face presentation.
Not stated, . . .	Recovery,	Severe inflammation of uterus,	Many efforts made at turning.
Difficult, ...	Recovery.		
Not difficult, . .	Recovery.		

No. of Case.	No. in Report.	Cause of Obstruction and Delay.	No. of Pregnancy.	Length of Labor before Operation.
29	61	Head in the pelvis, and an ear felt, . . .	. .	Twenty-four hours, .
30	62	Os uteri rigid and partially dilated. Head compressed in the brim.	1	Seventy-two hours, .
31	63	Head fixed in the brim, ...	1	Above 24 hours, . .
32	64	Head of child passed into the pelvis. Labia swollen to the size of a child's head.	. .	First stage very protracted,
33	65	Great distortion of the pelvis, ...	1	Thirty-six hours, . .
34	66	Head fixed in the brim; pelvis small, . . .	1	Forty-eight hours, . .
35	67	Head above the brim, ...	1	Great protraction (above 24 hours),
36	68	Small pelvis; os uteri rigid, ...	1	Forty-six hours, . . .
37	69	Great swelling of soft parts, ...	1	About 72 hours, . . .
38	70	. . . .	. .	. . . .
39	71	Os uteri half dilated, ...	. .	Twenty-four hours, .
40	72	. . . .	. .	. . . .
41	73	. . . .	2	Above 12 hours, . .
42	74	Distortion of pelvis, ...	. .	After great protraction,
43	75	Head half through brim, ...	1	Above 30 hours, . .
44	76	Distortion of pelvis; head not in the cavity of pelvis.	1	About 72 hours, . . .
45	77	Head scarcely entered the brim, ...	. .	About 48 hours, . . .
46	78	Head firmly compressed between sacrum and pubis.	. .	Forty-eight hours, . .
47	79	Distortion of pelvis; head in the brim, . .	2	Above 48 hours, . .
48	80	Distortion of pelvis; head above the brim, .	1	Thirty-six hours, . .
49	81	Distortion of pelvis; greater part of head still above brim.	1	Above 48 hours, . .
50	82	Head firmly jammed in the pelvis, ...	3	Above 15 hours, . .
51	83	Head fixed in the brim, ...	4	Above 6 days, . . .
52	84	Head firmly fixed in the brim, ...	10	About 24 hours, . .
53	85	. . . .	. .	About 30 hours, . .
54	86	Contraction of pelvis, ...	. .	. . . .
55	87	Distorted by rickets; head above the brim, .	1	Nearly 60 hours, . .
56	88	Head firmly impacted in the pelvis, . . .	. .	Forty-eight hours, . .
57	89	Head so low in pelvis that an ear was felt; closely surrounded by the inflamed vagina.	. .	Seventy-two hours, . .

Difficulty or not in Extraction.	Death or Recovery of Mother.	Other Results. Inflammation, Sloughing, &c.	Remarks.
Not stated, . . .	Recovery,	. . . .	Vectis and forceps tried first.
Not stated, . . .	Death, . .	Great inflammation and sloughing.	
Not stated, . . .	Recovery.		
Not stated, . . .	Death, . .	Gangrene of vagina and external parts.	
Not stated, . . .	Recovery.		
Great force, . . .	Recovery,	. . . .	Not able to apply the forceps.
Not stated, . . .	Recovery.		
Not stated, . . .	Recovery.		
Not stated, . . .	Recovery,	. . . .	First tried forceps. Twins.
Not stated, . . .	Recovery,	. . . .	Violent puerperal convulsions.
Not stated, . . .	Recovery,	. . . .	Placenta prævia.
Not stated, . . .	Recovery,	. . . .	Puerperal convulsions.
Without much difficulty.	Death, . .	. . . .	Rupture of uterus.
Not stated, . . .	Recovery.		
Not stated, . . .	Recovery,	. . . .	Retained placenta.
Great force, . . .	Recovery,	. . . .	Soft parts must have been severely contused.
Difficult, ...	Recovery,	Uterine inflammation,	Delivered by craniotomy before.
Great force, . . .	Recovery,	. . . .	Right parietal bone of the child depressed and fractured.
Not stated, . . .	Recovery,	. . . .	First child delivered by craniotomy.
Slowly, ...	Recovery,	. . . .	The operation lasted two hours, and the bones of the skull were all torn to pieces before the head could be extracted.
Difficulty,. . . .	Recovery,	. . . .	Next child delivered without aid. The breech presented.
Easily, ...	Recovery,	. . . .	Convulsions occurred.
Readily, ...	Recovery.		
Difficulty, . . .	Recovery,	Retention of Urine; sloughing of vagina; vesico-vaginal fistula.	
Easily, ...	Recovery,	Sloughing; vesico-vaginal fistula.	
Extreme difficulty,	Recovery,	. . . .	A dwarf. The operation lasted nearly five hours, and the head of the fœtus could not be drawn through the brim of the pelvis, until the bones of the base of the skull were all torn to pieces.
Delivered by turning.	Recovery,	Uterine inflammation,	Turning resorted to in consequence of an arm having been brought down by the crotchet.
Great force, . . .	Recovery.		
Some difficulty,. .	Recovery,	Retention of urine; violent inflammation and sloughing; cicatrix left in vagina.	

No. of Case.	No. in Report.	Cause of Obstruction and Delay.	No. of Pregnancy.	Length of Labor before Operation.
58	90	Head at the external parts,	1	Seventy-two hours,
59	91	Pelvis distorted; head in the brim,	. .	. . . .
60	92	Part of the head above the brim; os uteri partially dilated.	1	Very protracted,
61	93	Head wedged in the brim,	. .	Very tedious,
62	94	Greater part of head above the brim,	1	Fifty hours,
63	95	Head squeezed in the brim,	. .	Forty-eight hours,
64	96	Head firmly impacted in the brim,	1	Seventy-two hours,
65	97	Head jammed in brim; distortion of pelvis,	. .	. . . .
66	98	Head far advanced into the pelvis; vagina swollen.	1	Fifty-three hours,
67	99	Head compressed in brim; os uteri partially dilated.	1	Seventy-two hours,
68	100	Head has scarcely entered the brim; os uteri rigid and not half dilated.	1	Thirty-eight hours,
69	101	Head deep in the pelvis, great swelling of the vagina, &c.	. .	Thirty-seven hours,
70	102	Os uteri partially dilated; head above brim,	1	Forty hours,
71	103	Head and right arm presenting; os uteri half dilated.	2	. . . .
72	104	Head fixed in brim; vagina swollen and tender,	. .	About 72 hours,
73	105	Head strongly compressed in the brim; os uteri not fully dilated.	. .	Above 30 hours,
74	106	Soft parts immensely swollen,	. .	Very protracted,
75	107	Head firmly wedged in the brim; vagina swollen and tender.	. .	Above 78 hours,
76	108	Head not passed brim,	. .	Forty-eight hours,
77	109	Head impacted in the brim,	1	Forty-one hours,
78	111	Distortion of the pelvis; head compressed in the pelvis.	. .	Eighty-six hours,
79	112	Distortion of the pelvis; head not in brim,	1	About 48 hours,
80	113	Distortion of the pelvis; head above brim,	. .	Above 62 hours,
81	114	Head above the brim; os uteri not completely dilated.	. .	Above 78 hours,
82	115	Greater part of the head above the brim,	1	Above 53 hours,
83	116	The head would not pass till perforated, being distended with fluid.	. .	. . . .
84	117	Head above brim, and distended with fluid,	1	Nearly 60 hours,
85	118	Head above brim, and distended with fluid,	1	Long in labor,
86	119	Head distended with fluid,	. .	. . . .
87	120	Head distended with fluid,	. .	Above 21 hours,

Difficulty or not in Extraction.	Death or Recovery of Mother.	Other Results. Inflammation, Sloughing, &c.	Remarks.
Not stated, . . .	Recovery,	Retention of urine; sloughing.	The tumefaction of the labia and vagina was such that the blades of the forceps could not be introduced.
Not stated, . . .	Recovery,	. . . .	An unsuccessful effort had been made to deliver with the forceps.
Not stated, . . .	Recovery,	. . . .	Delirium for nearly a day.
Not stated, . . .	Death, . .	Inflammat'n of bladder	
Not stated, . . .	Recovery.		
Not stated, . . .	Recovery.		
. . . .	Recovery,	Sloughing; vesico-vaginal fistula.	
Not stated, . . .	Recovery.	. . . .	Face presentation. Bursting of a thrombus of the vulva.
Not stated, . . .	Recovery.		
Not stated, . . .	Recovery.		
Not stated, . . .	Recovery.		
Tedious, ...	Recovery.		
Great force, . .	Recovery.		
Not stated, . . .	Recovery.		
Not stated, . . .	Recovery.		
Great force, . . .	Recovery.		
Two hours required.	Recovery,	Retention of urine and sloughing of vagina.	
Great difficulty, . .	Death, . .	Uterine inflammation.	Perineum slightly lacerated; retained placenta; slight hemorrhage.
Easy, ...	Recovery.		
Much force, . . .	Recovery.		
Not stated, . . .	Death.		
Great force, . . .	Recovery.		
Not stated, . . .	Recovery.		
Not stated, . . .	Recovery,	. . . .	There was a peculiar thickening at the upper and back part of the vagina, which seemed to arrest the progress of the head.
Force required, . .	Recovery.		
. . . .	Death, . .	Inflammation of muscular coat of uterus.	
Easy, ...	Death, . .	Inflammat'n of uterus.	
Easy, ...	Death, . .	Disease of chest; inflammation of veins in pelvis.	
Easy, ...	Death, . .	Rupture of uterus, .	Placenta prævia. Child turned first.
Little difficulty, . .	Death, . .	Rupture of uterus, .	Child turned.

In most of the eighty-seven cases in the preceding table, the operation of embryulcio was performed while the volume of the head was still above the brim. In the detail of many of the cases, Dr. Lee mentions whether the operation of extraction, after the skull was opened, was difficult or not; in thirty, however, of the eighty-seven, he particularly notes the extraction of the perforated head with the crotchet as having been attended with difficulty and force. The footnote below[1] contains a list of these thirty cases, and Dr. Lee's own expressions with regard to the degree of difficulty met with in each case, in this, the second part of the operation, viz., the extraction with the crotchet. In twenty-two of these thirty cases it is stated that the head of the child was not through the brim at the time craniotomy was performed, but required to be pulled through it with the crotchet. In most of the remaining eight cases the position of the head at the time of operating is not mentioned.

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[1] Case 5.—"After perforation, great difficulty was experienced in extracting the head with the crotchet." Case 8.—"From the long-continued efforts required to drag the head into the cavity of the pelvis, it was evident that the delivery could have been accomplished in no other way with safety to the mother." Case 9.—"After perforation, strong efforts were required to complete the delivery, and she died within forty-eight hours." Case 16.—"The force afterwards required to extract the head with the crotchet made us regret that we had not interfered sooner." Case 29.—"After perforation, the head was extracted with difficulty by the craniotomy forceps." Case 31.—"The head was perforated, and the force afterwards required to extract it with the craniotomy forceps was so great, that I regretted having endeavored to deliver before lessening the volume of the head." Case 32.—"After perforation, so much force was required to draw the head through the pelvis with the craniotomy forceps, that I regretted having attempted to save the child." Case 34.—"The extraction of the head with the crotchet was a tedious and difficult operation." Case 35.—"I did not succeed without much difficulty in delivering with the craniotomy forceps." Case 38.—"I opened the head, and had much difficulty afterwards in drawing it down with the crotchet." Case 39.—"Perforation and extraction difficult." Case 40.—"Extraction difficult." Case 56.—"Extraction with the crotchet tedious and difficult." Case 59.—"Extraction difficult." Case 66.—"Great force required to extract." Case 76.—"The bones of the cranium were all torn away with the crotchet before I succeeded in drawing the base of the skull through the brim of the pelvis. . . . . Great force was afterwards required to drag the shoulders into the cavity of the pelvis." Case 77.—"The operation of craniotomy was difficult." Case 78.—"Great force was required to extract the head after being opened." Case 81.—"The difficulty experienced in extracting the head with the crotchet after it was opened, proved that delivery could not have been completed by any other method." Case 84.—"After the head had been opened, two hours elapsed before I could extract it with the crotchet." Case 86.—"The operation lasted nearly five hours, and the head of the fœtus could not be drawn through the brim of the pelvis until the bones of the base of the skull were all torn to pieces with the crotchet." Case 88.—"After the perforation, so much force was required to extract the head, as to make it certain that the head could never have been extracted without its size being reduced." Case 89.—"I opened the head and had some difficulty in extracting it." Case 102.—"Great force required to extract." Case 105.—"The head required great force to extract it after perforation." Case 106.—"Two hours required to extract the head with the craniotomy forceps." Case 107—"Great difficulty in extracting the head." Case 109.—"So much force required to extract the head that it was obvious it never could have been delivered with the forceps, or expelled by the natural efforts." Case 112.—"The extraction required great force." Case 115.—"The force required to extract the head after perforation rendered it obvious that the head could never have passed in the exhausted state in which she was."

Out of the thirty cases presenting the subjoined degree of force and difficulty in the extraction of the head, two only of the mothers died. In the remaining fifty-seven cases, either the extraction is noted as easy, or was not so difficult as to have been considered worthy of being noted; and out of these fifty-seven cases thirteen mothers died. The following arrangement expresses these results in a tabular form:

TABLE X.

Table of Eighty-seven Craniotomy Cases, reported by Dr. Lee, showing the Numbers in which Extraction with the Crotchet was—1, noted as difficult; or, 2, not noted as difficult; and the results of these two Classes.

Extraction of the Head after Perforation.	Number of Cases.	Number of Deaths.	Proportion of Deaths.
Noted as difficult,	30	2	1 in 15
Not noted as such,	57	13	1 in 4½

These interesting facts evidently point to one conclusion, viz., that in distorted pelvis, the amount of force required for the delivery and extraction of the head of the child, in cases of craniotomy, and hence, also in cases of turning, has far less influence upon the ultimate results and mortality of the operation than we could well have anticipated. But I anxiously desire not to be misunderstood on this point. I by no means wish to maintain that the degree of force used in delivery has no influence upon the resulting degree of danger and mortality to the mother. Nothing could be more different from either my intentions or my convictions. But the above table shows that in the ordinary forms of distorted pelvis, the degree of force employed for effecting the transit of the head is far less an element of danger than could have been previously imagined; and the explanation of the apparent paradox is, I believe, two-fold. For, first, *temporary* compression applied to two special points in the circle of the pelvis (as in the case when the child's head is dragged, by operative measures, through a brim contracted only in one diameter) is by no means so dangerous as more *prolonged*, though slighter, degrees of compression of the maternal structures, when the compression is equable, and diffused over the whole circle of the pelvic aperture, or the whole interior of the pelvic cavity, as in instances in which a hydrocephalic fœtal head is pressing like an hydraulic bag, equally, and on all sides. But, secondly, the effect of the law of the influence of force is counteracted by, and subsidiary to, another law of a far higher degree of practical generalization, viz., the law which I have illustrated at length in a preceding section, that "the danger attendant upon parturition increases in a ratio progressive with the increased duration of the labor." And I have little doubt that if we had proper and adequate data for conducting the inquiry, it would be found that the safe result of those craniotomy cases in which force is employed in the delivery is in a great measure owing to the *time* of the labor at which the delivery is accomplished; and that when the result is fatal, it is as much owing to the previous length or duration of the labor, as to the force employed. Dr. Collins, in his chapter on still-born children, details eight cases of craniotomy in which, after perforation, the extraction of the head with the crotchet required more or less severe exertion and force.[1]

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[1] These eight cases are the following:—1. "No. 49 was forty-eight hours in labor of her first child. Having made no progress for the last twenty-four hours, the pulse becoming extremely quick, with great general debility, the head was lessened, and delivery effected by the crotchet. Considerable difficulty was experienced in getting the head through the pelvis in consequence of the hand having descended with it."—Practical Treatise, p. 462. 2. "No. 56 was a diminutive woman, much deformed; had been twice force-delivered in this Hospital, as from the size of the pelvis it was impossible for a child to pass entire; even after the bones of the head were completely broken down, much difficulty was experienced in completing the delivery." 3. "No. 209 was admitted, reported to have been four days in labor, and attended by a midwife; the uterus continued to act strongly, yet after waiting eleven hours the labor made no progress, and, as there was no doubt the child was dead the head was lessened. Many of the bones were removed before the delivery could be completed; the child was large and putrid." 4. "No. 210; after three days' labor, the head, still at the brim of the pelvis, was lessened, and almost every bone removed before it could be delivered, and even after it was brought down much exertion was required to free the shoulders and body. The child was large, and the abdomen somewhat distended with air. The mother seemed at this time almost lifeless, having lost the power of swallowing. The hand was introduced into the uterus, which was quite relaxed, the placenta was gently removed, and the patient expired immediately. On dissection, the uterus was found healthy, but badly contracted, containing a small quantity of coagulated blood; the intestines were in the highest state of congestion, and there was about a pint of fluid in the abdominal cavity, with portions of coagulable lymph in different parts, seemingly the effects of inflammation previous to labor. On opening the chest, the lungs were observed to adhere so firmly as to require the knife in many places to separate them. Nothing was discovered to account for the suddenness of death." 5. "No. 256 was thirty-two hours in labor previous to admission; her pulse was rapid, and tongue foul. She was delivered some hours after she came in, by lessening the head, which was greatly enlarged from hydrocephalus; there was some difficulty in completing the delivery, even after the bulk of the head had been as much as possible reduced." 6. "No. 303 was admitted, reported to have been three days in labor of her first child; the head was low and firmly fixed in the pelvis; the bladder greatly distended with urine, it having been retained for thirty hours; pulse 140; tongue dry and white. The catheter was passed and three pints of urine removed. As the abdomen was free from pain, it was thought advisable to watch the effect of uterine action for some time. After waiting five hours, during which the pains were pretty brisk at intervals, still the head made no advance; it was lessened and brought away with the crotchet. There was considerable exertion required to bring down the shoulders; the abdomen was much distended with air, the consequence of putrefaction. She died on the fourth day from delivery." 7. "No. 667; the labor lasted thirty hours; the head was firmly fixed in the pelvis, and had

One of the eight patients died, but she had been seventy two hours in labor when the operation was undertaken; all the other seven recovered. In six out of these seven cases, craniotomy was adopted earlier; in one, before the labor had reached sixteen hours in length; in all, before it had passed forty-eight hours from its commencement.

The manner and extent, in which the laws of danger attendant upon this and other particular obstetric operations, are controlled by, and absorbed in, the more general law of the attendant mortality, depending upon the length or duration of the labor, have been already illustrated (see Section IV.), in reference to delivery by the forceps, and delivery by the crotchet, taken individually. The law and its results, however, are so extremely important as bearing upon the question of the present section, that I will, in further confirmation of it, and at some risk of repetition, throw into a combined form and table, all the forceps and all the craniotomy cases mentioned by Dr. Collins in which he specifies the duration of the labor. In other words, we will conjoin the two tables previously given in Section IV., into one; and examine how much the maternal mortality was modified in tedious and laborious labors, requiring instrumental aid, by the date of the labor at which that aid was given. Our former tables contained 24 cases delivered by the forceps, and 77 delivered by craniotomy; in all 101 instrumental labors. The results were as follows:

TABLE XI.

Showing the Results of 101 *Instrumental Deliveries in which the Duration of Labor at the time of the Operation was noted.*

Duration of Labor.	Number of Cases.	Number of Deaths.	Proportion of Deaths.
Under 24 hours,	32	2	1 in 16 died.
From 25 to 48 hours,	42	6	1 in 7 died.
Above 48 hours,	27	11	1 in 2½ died.
Total,	101	19	1 in 5 died.

This combined table of Dr. Collins' instrumental deliveries fully

made no progress for twelve hours. As the heart's action had some time ceased, and the mother's pulse was 140, the head was lessened. Great exertion was necessary to effect delivery in consequence of the head being much ossified." 8. "No. 725; this patient, when admitted, was reported to have been sixty hours in labor; the os uteri was very little dilated, and the head high up in the pelvis. The pains continued constant for twenty-four hours after she came in, yet the labor made little progress; the mouth of the womb was rigid, jagged, and had the feel of cartilage. The child being dead, as indicated by the stethoscope, the head was lessened and left in that state for some hours, and afterwards cautiously, brought down. Considerable force was necessary to complete the delivery, though the child was putrid."

bears out the important conclusion regarding the influence of the antecedent duration of labor, that we have already seen to be derivable from the study of the two operative measures, the forceps and crotchet, taken singly and individually. The evidence of the present table, like the evidence of those previously given (see Tables V. and VI.) amounts to this—that the maternal mortality accompanying instrumental delivery is regulated more by the period of the labor at which the artificial interference is practised, than by the nature of the operation itself, or by any other contingent circumstance—the general fatality being decreased in proportion as the operation is performed early, and increased in proportion as it is postponed and delayed longer and longer from the date of the first commencement of parturition. But, admitting that this holds true in regard to instrumental delivery by the forceps and craniotomy, it may be doubted and objected that it perhaps does not hold true in regard to artificial delivery by the operation we propose to employ, viz., turning. Let us interrogate Dr. Collins' facts, in order to obtain an answer.

In his Report of the Dublin Lying-in Hospital, Dr. Collins gives the result of thirty-three cases, in which turning was practised in consequence of presentation of the shoulder or arm. In one of these cases the placenta presented, and I exclude it as thus containing a source of danger quite independent of the turning. Among the remaining thirty-two cases, there are two deaths, or one in sixteen; and twenty-four in which the date of the duration of the labor, at the time of turning, is stated as ascertainable. In only one of these twenty-four cases is the labor noted as having exceeded thirty hours in length, and this protracted case was an instance of twins, in which the second child presented preternaturally, and required turning. In twenty-one of the cases the labor was terminated before twenty-four hours; among these, one died—a case complicated before delivery by severe accidental hemorrhage; in three it was prolonged beyond twenty-four hours, and one of these three mothers died. When arranged in a tabular form, the results are as follows:

TABLE XII.

Showing the Mortality accompanying Twenty-four Cases of Turning for Presentation of the Arm and Shoulder, as regulated by the Previous Duration of Labor.

Previous Duration of Labor.	Proportion of Deaths of Mothers.
Below 24 hours,	1 in 21 died.
Above 24 hours,	1 in 3 died.

The data in the preceding table are not numerous, but still they will probably be allowed to be sufficient evidence of the fact, that the degree of maternal danger attendant upon turning, as upon other modes of operative delivery, is regulated and modified by the date of the labor at which the delivery is practised; artificial labors, like spontaneous labors, increasing in their mortality in proportion as the previous parturient action has been allowed to be increased and prolonged in its duration and length. Hence, also, necessarily follows the same deductions which I have already more than once adduced, viz.—

1. A means of artificial delivery, such as turning, which, in cases of pelvic contraction, would enable us to finish the labor at an early date, should, as a general rule, add greatly to the safety and chance of life of the mother, as compared with a means of delivery such as the long forceps or craniotomy, which cannot be usually and legitimately practised till a much later period after the commencement of parturition.

2. The facts I have adduced in the present section show, that the exertion of force in artificial delivery is attended with comparatively little danger, provided always the delivery is early; and that the employment of it is by no means so hazardous as the simple long continuance and protraction of the labor.

3. All these deductions are only corollaries to the higher and more comprehensive law, that the degree of danger and fatality attendant either upon natural or morbid parturition, increases in a ratio progressive with the increased duration of the labor.

SECTION IX.—ON THE COMPARATIVE DANGER OF LOCAL LESIONS OF THE MATERNAL ORGANS (VAGINA AND UTERUS), IN DELIVERY BY TURNING, AND DELIVERY BY INSTRUMENTS.[1]

As far as the observations in the preceding section go, they refer to the comparative degree of actual danger to the *life* of the mother under the opposite means of treatment, by which cases of labor, with arrestment of the head, and distortion of the pelvic brim, may be treated. They refer merely to the question of the probable death or probable survival of the mother, under the two different lines of practice spoken of, viz., delivery by instruments and delivery by turning. But it is possible, that, under such a form of labor as we have been considering, the mother may survive, that she may escape with life, and yet the maternal passages and neighboring soft parts may be so much damaged and injured, as to interfere, in a more or less dangerous or distressing manner, with the state of her future health and future happiness. The cervix uteri, or the walls of the vagina, are sometimes so contused, or become so inflamed, that gangrene and sloughing supervenes, and lead to the formation of cicatrices and strictures; or, what is still more deplorable, this resulting gangrenous inflammation may produce fistulous communications between the vaginal canal, and the rectum, or urethra, or bladder. No consequence could be possibly more deplorable than this last—the occurrence of recto-vaginal or vesico-vaginal fistulæ.

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[1] From Provincial Medical and Surgical Journal, January, 1849, p. 9.

Speaking of sloughing of the urethra or neck of the bladder, as a consequence of severe labor, Dr. Collins remarks, in words dictated by the best possible feelings: "I do not know of any occurrence more calculated to render the patient's life one of endless sorrow; or, at the same time, more likely to cause the practitioner such lasting regret. When it unfortunately happens, as in some instances is unavoidable, in consequence of the protracted length to which we are at times compelled to permit the labor to proceed, owing to great difficulty in the passage of the head, the child being *alive*, here the medical attendant's mind cannot, on his *own* account, feel distressed, as the *only* means he could adopt to guard against the danger would be to lessen the head, which, in my opinion, no consideration should induce him to do under such circumstances."[1]

Are such distressing results as Dr. Collins here describes, more liable to occur under the present practices of delivery by instruments, or under the new practice which I propose of delivery by turning? In other terms the question which I wish to consider is this:

Are Local Lesions from Inflammation of the Vagina, such as Sloughing, Fistulæ, &c., more liable, in a Contracted Pelvis, to supervene after Delivery by Turning, or Delivery by Instruments?

Vaginal inflammation terminating in sloughing, cicatrices, and fistulæ, is not unfrequently observed after protracted labors that are terminated by the forceps or crotchet. In his "Clinical Midwifery," after detailing "the histories of fifty-five cases of difficult parturition in which the forceps was employed," Dr. Lee remarks,[2] "Five of the mothers whose cases have now been related died from puerperal convulsions, and four from the rash and inconsiderate use of the forceps; seven had the perineum more or less injured; one the recto-vaginal septum torn; five were left with cicatrices of the vagina after sloughing; and one with an incurable vesico-vaginal fistula." After describing the histories of sixty-five cases of "difficult labors from distortion of the pelvis, swelling of the soft parts, convulsions, hydrocephalus in the fœtus, and other causes in which delivery was effected by the operation of craniotomy," Dr. Lee again observes,[3] "In thirty-eight of the cases in this report, the labor continued from forty to seventy hours. In the cases of spontaneous rupture of the uterus and convulsions only, was the delivery effected before the labor had lasted upwards of thirty hours. In a very large proportion of the cases, the difficulty arose from distortion, or a contracted state of the pelvis. Rupture of the uterus took place in three before perforation, and the inflammation and sloughing of the uterus, vagina, and bladder, which proved fatal to eight others, were chiefly or solely produced by the long-continued violent pressure on the soft parts by the head of the child before it was opened and extracted. In those who recovered with vesico-vaginal fistulæ, or contraction of the vagina from cicatrices, the unfortunate occurrences arose from craniotomy being too long delayed."

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[1] Practical Treatise, p. 359.

[2] Clinical Midwifery, p. 32.

[3] Ibid. p. 59.

In the table which I have previously given from the same author (see Table IX.) of the results of 87 cases of craniotomy, local lesions on the part of the mother are noted as having occurred in several instances. Out of the 87 cases, eight,[1] or about 1 in every 10, suffered from vaginal inflammation and sloughing; four,[2] or nearly 1 in every 20, were left with vaginal fistulæ.

On the other hand, I am not aware of a single recorded instance in which vaginal sloughing and fistula ever followed transverse presentations of the fœtus, and where the delivery is, as a general rule, always accomplished by turning. Two circumstances lead to this immunity:

1st. In presentations of the arm and shoulder, the presenting part does not so completely fill up and compress the tissues of the brim as in head-presentations. Hence both the tendency to, and the occurrence of, congestion of the vessels and inflammation in the tissues, situated below the site of the compression, viz., the vagina, &c., is far rarer. But—

2dly. The delivery is always, or almost always, accomplished early in the labor, and no doubt the mere length and protraction of the labor is one of the great, if not the greatest, predisposing and exciting causes of gangrenous inflammation and sloughing, of the vagina.

In many cases vaginal sloughings and fistulæ have been attributed to the use of instruments, when they were more truly owing to gangrenous inflammation and sloughing, ensuing in consequence of the delay that was allowed to occur before instrumental delivery was adopted. In the history of most instances of vesico-vaginal and recto-vaginal fistulæ following labor, there is one simple fact, proving that they are not the direct effect, as is too often supposed, of lesions and lacerations by the forceps or crotchet. If vesico-vaginal fistulæ were, in this way, the direct effect of injuries produced by cutting

[1] See Cases 10, 20, 30, 32, 57, 58, 61, 74.

[2] See Cases 1, 52, 53, 64.

with the instruments, then escape of urine through the vagina would be observed immediately after labor. But this is rarely the case; the general fact being, that the escape of urine through the false opening between the urinary and vaginal canals is not seen for several days after delivery; because it is not till several days have elapsed, that the gangrenous slough in the parietes of these canals separates. No soft parts, any more than the maternal canals, could stand, without endangering their vitality, the steady pressure of a firm body upon them for twenty or thirty hours, such as that which the detained fœtal head exerts in contracted pelves upon the two points of contraction—the symphysis pubis before, and the promontory of the sacrum behind. It is not a matter of wonder that the soft structures compressed at these special points between the fœtal head and maternal bones, should sometimes inflame and slough. It is rather, perhaps, surprising, that this consequence does not oftener follow. On this point Dr. Beatty of Dublin makes one or two remarks of such importance, that I shall offer no apology for quoting them at length. "With respect," says he, "to laceration and sloughing of the vagina, bladder, &c., stated by some authors to be caused by the forceps, and used as an argument against their employment, I am of opinion, that in the majority of cases, when these lamentable results occur, the blame is unmerited; because I have seen the worst inflammation and sloughings of these parts follow in cases where the perforator had been used, and even in some where no instrument whatever was employed. The truth is, the mischief is effected by the pressure of the infant's head upon the soft parts of the mother, and after this has been continued with sufficient intensity, for a sufficient length of time, the inflammation caused thereby will run its course, no matter in what way delivery is accomplished, whether by the natural efforts or by instruments. But it frequently happens that delivery is effected in these cases by instruments, *too late* to prevent the unhappy results alluded to, and then the operation is charged with the consequences. If an accurate account of the subsequent condition of all women after delivery could be obtained, I much fear that the histories of those cases in which labor had been allowed to run too long before interference, would be anything but satisfactory. The lamentable sloughings of the vagina, with subsequent closure of the passage by the process of cicatrization, or the still more distressing sloughing of the bladder, with its attendant urinary fistulæ, are seldom mentioned in lying-in hospital reports, because the patients are usually removed from those institutions *before* such results have become very apparent; and thus, a case left to nature in which delivery is effected by the natural efforts, is set down as a favorable one, without any notice of its consequences."[1]

Dr. Beatty elsewhere soundly and correctly remarks in regard to the causation of gangrene and sloughing, &c., of parts of the vaginal walls by the pressure of the child's head, "that it is the *continuance* of the pressure that does the mischief rather than its *intensity*. We know that the soft tissues of the body are endowed with a resiliency —a power of resistance that enables them to bear a temporary compression of great amount without injury, while inferior pressure continued for a length of time will terminate in their disorganization. The malingering soldier is well aware of this fact, and acts upon it, when he wishes to produce an ulcer. He straps a piece of coin or other hard substance tightly upon the part. At first no effect is produced, and if the apparatus is removed in a short time, there is no evil consequence; but if the same amount of pressure is continued for some hours, such a degree of injury is inflicted as terminates in the destruction of the part, and a sloughing ulcer is the result."[2]

That the mere morbid protraction of labor is the great predisposing cause of gangrenous inflammation and sloughing of the vagina, as suggested in these remarks, is shown by Dr. Collins' results. Dr. Collins has recorded the duration of the labor in six cases where sloughing of the vagina was detected after death. The following table shows that the accident occurred in an immensely greater proportion in prolonged than in short labors; and that, consequently, protraction is, as I have just stated, almost a necessary antecedent to the occurrence. The case, I may remark, in which sloughing followed a labor less than twenty-four hours in duration, required instrumental delivery, but the mode of delivery, whether by the crotchet or forceps, is not specialized by Dr. Collins.[3]

TABLE XIV.

Duration of Labor in six Fatal Cases of Sloughing of the Vagina.

Length of Labor.	Number of Deliveries.	Number of Cases.	Proportion of Cases.
Within 24 hours,	15,586	1	1 in 15,586.
Beyond 24 hours,	264	5	1 in 53.

If, as these data show, the protraction of labor is a main and almost necessary element in the production of gangrenous inflammation of the vagina, it is equally evident that turning, in the class of cases we are considering, while it frees the mother from the danger of a fatal termination, as shown in the last section, would at the same time free her also from the chance of suffering from these *local* complications that are apt to follow upon protracted labors.

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[1] Dublin Journal of Medical Science, vol. xii. p. 290.

[2] Dublin Quarterly Journal, vol. xxi. p. 344.

[3] See Practical Treatise, p. 358.

In the passage which I have quoted from Dr. Collins, at the commencement of the present section, that experienced practitioner laments our being occasionally compelled to subject our patients to the chance of local dangers and complications, in consequence of our being compelled, in some unavoidable instances, to allow the labor to proceed onward to a protracted degree, from the child being still alive, "for then the *only* means the practitioner could adopt to guard against the danger, would," says Dr. Collins, "be to lessen the head, which, in my opinion, no consideration should induce him to do under such circumstances," viz., as long as the infant is still living. The proposition which I have made of turning in such instances resolves the difficulties attending upon them, in two ways. For, *first*, it enables us to deliver the patient early and at once, and thus frees us from the dread of these consequences of protraction; and, *secondly*, the continued vitality of the infant would, under the treatment by turning, not be a reason and incentive for our dangerously delaying the delivery, but would form a strong reason and incentive for our proceeding with the practice at as early a period as is proper and possible. The details of a case or two may enforce these observations; and I shall select some from Dr. Collins, calculated both to illustrate these remarks, and to show, that in protracted labors, occasionally *before* the time that the child does at last die, the dreaded amount of local mischief has been accomplished, so that if we waited always for the certainty of the infant's death, in order to deliver by craniotomy, we should wait beyond the time that was necessary, in order to save (locally) the maternal structures from inflammation and gangrene.

It will be remarked, that in the first of the following instances, the fœtal heart was heard six hours, and in the second case, eight hours, before delivery, and consequently ceased some time between these periods and the period at which the perforator was used—in short, necessarily not long before craniotomy was practised. Consequently, we have here cases which, in the severity of their local, and, I may add, in the severity also of their general symptoms, show what I have before argued, that the mother may herself be assuredly placed in the greatest possible danger and hazard from the protraction of the labor, before that protraction kills the child. I may add, that in the first case, the position of the fœtal heart, viz., in the right iliac region, showed the infant to be in the occipito-posterior position (the third position of Naegele).

CASE XXXVI.—"No. 126. This woman was fifty-nine hours in labor; it was her first child. The pains were for a considerable time very trifling, with long intervals; however, for the last twenty-four hours the uterus acted with tolerable regularity, the pains being at times strong, causing the head to press with much force against the ischia, where it remained stationary for the greater part of that time. Her pulse was very much increased in frequency, varying between 120 and 130; the external parts were œdematous. As the fœtal heart had ceased to act, having been distinctly audible in the right iliac region six hours before, the head was lessened and the crotchet applied. The placenta was expelled in forty-five minutes, immediately after which, in consequence of hemorrhage, the hand was introduced, and so it was arrested. Violent inflammation and sloughing set in, resisting all treatment, and she died on the 9th day. For four days previous she had severe diarrhœa, a succession of motions coming on suddenly, with severe pain; she had also severe hiccough. On examination after death, the vagina was found in a state of slough; the sides opposite the spines of the ischia were broken through with the slightest force, and were completely gangrenous. A circular opening the size of a shilling was found, forming a communication between this cavity and the rectum, the mucous surface of which, as also that of the colon, was softened, and had, in the vicinity of the opening, a gangrenous appearance. There was no symptom of inflammation in the peritoneum or uterus."[1]

CASE XXXVII.—"No. 1091. Was admitted August 23d, in labor of her first child, and was not delivered until the 25th, being a period of fifty-six hours. Uterine action from the commencement until within six hours of the expulsion of the child, was extremely feeble, with long intervals. The head remained high in the pelvis, and although the ear could not be reached, it was evident the head had sufficient room to pass; to effect which uterine action was alone wanting. As soon as the pains began to be brisk, the labor proceeded without difficulty. The fœtal heart was quite audible until eight hours previous to the birth. In three hours after the hand was passed to remove the placenta, it was found separated, and without the slightest effort the uterus contracted and expelled both. The perineum had suffered considerably in the passage of the head. The patient never seemed to rally after delivery; the pulse continued quick; there was considerable tenderness on pressure over the uterus, with a foul discharge from the vagina. * * * On the 7th and 8th, she had distinct rigors, followed by perspiration, after which her strength became greatly reduced. The vaginal discharges continued foul, notwithstanding the most rigid attention to cleanliness and the use of stimulating injections. She gradually sank and died on the 11th day, having for two days previously suffered from frequent hiccough. On dissection, the only morbid appearances found were in the bladder and vagina. In the bladder, the mucous surface was covered with yellow lymph, and it contained a quantity of muco-purulent fluid. In the vagina, opposite the right ischium, a portion appeared to have been destroyed by slough."[1]

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[1] Practical Treatise, p. 158.

Hitherto, under the present section, I have spoken of the dangers which the mother may avoid by employing the operation of turning as a substitute for instrumental delivery, in cases of contraction of the pelvic brim. But while turning enables us, as I have shown, to eschew some of the gravest and most imminent dangers connected with the use of the long forceps and crotchet, it is, on the other hand, by no means to be forgotten, that it subjects the mother to one great form of danger, from which, in common belief, she would be comparatively free under the adoption of instrumental delivery. The special maternal danger attending upon turning is *laceration or rupture of the uterus*, confessedly the most fatal complication that can occur in connection with delivery. Is the liability to rupture under turning so great as to make us avoid this practice and trust rather to the forceps and crotchet? Or, in other words—

Is Rupture of the Uterus in Contracted Pelvis much more apt to be produced by Delivery by Turning than by Delivery with Instruments?

Most authors describe laceration of the uterus as one of the principal hazards which the mother runs in cases of turning. But injury and laceration of the uterus are perhaps not so liable to follow this operation as is generally imagined. "Between the years 1823 and 1834," says Dr. Ramsbotham, "I delivered more than one hundred and twenty women under transverse presentations; independently of a few cases to which I was summoned where spontaneous evolution occurred. Many of these cases presented a formidable appearance; for in one, the membranes had been ruptured a whole week; in another, sixty-nine hours; in a third, fifty-eight hours; in another, fifty-five; in another, fifty-three; and in many, more than forty-eight; and as a general principle, we presume that the longer the liquor amnii has been evacuated, the more likely is the uterus to have embraced the fœtal body firmly, and the more difficulty will there be in overcoming the resistance. In none of these cases did I exhibit large doses of opium, and in those few where bleeding was practised, that operation was had recourse to, not for the purpose of relaxing the uterine fibres, but to relieve the inflammation which the soft structures were suffering, and to remove tumefaction. *In not one of these instances was any injury inflicted on the uterine structure; nor did any permanent evil arise that could be attributed to the operation.* In four cases only was the uterus so powerfully contracted as to refuse admittance to the hand, and compel me to adopt the alternative of eviscerating or decapitating the fœtus."[1]

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[1] Practical Treatise, p. 483.

Besides that injury of the uterus is thus not so apt, as is perhaps generally imagined, to follow upon turning when carefully and cautiously performed, there are other considerations, which are calculated strongly to show that the danger of laceration in the cases of pelvic contraction most fitted for the practice we suggest, is not much, if at all, increased by the proposed operation. I shall state the considerations to which I refer.

In the *first* place, I would premise this important remark, that, according to all our best obstetric pathologists, those degrees and forms of morbid contraction of the brim of the pelvis which I deem to be the cases best adapted for delivery by turning, are exactly those cases in which, under any plan of management, whether the patient be delivered by the natural efforts, or by the forceps or crochet, rupture and laceration are exceedingly apt to take place. On this subject I am anxious to adduce full evidence, and will consequently appeal to the unprejudiced testimony of Ramsbotham, Denman, Churchill, &c.

"Laceration of the uterus," says Dr. Ramsbotham, "is most likely to happen to a patient who has had three or four children, who possesses a slightly distorted pelvis, and who has been in strong labor for a number of hours. Although," he further observes, "the same rent may take place in any portion of the organ, its most frequent seat is at the neck, either at the posterior part opposite the prominence of the sacrum, or anteriorly, behind the symphysis pubis. The direction is also mostly transverse, or slightly oblique. It is not difficult to account for this being the most usual situation of the injury; for since, during the latter part of gestation, the neck of the womb rests upon the pelvic brim, if the promontory of the sacrum dip too far forward, or the ridge of the pubis be preternaturally sharp, it is reasonable to suppose that the uterine structure may be affected, that inflammation may occur as a consequence of pressure, and that a thinning or softening of the substance may be induced, and, under these circumstances, should the structure give way at all, it is likely that the weakened part will be the first to suffer. Denman, indeed, says that, independently of disease, the uterus may be *worn through* mechanically, in *long* and *severe* labors, by pressure and *attrition* between the head of the child and the projecting bones of a distorted pelvis, especially if they be drawn into points, or a sharp edge. One or other of these causes may explain," Dr. Ramsbotham concludes, "why we more frequently meet with laceration of the uterus, when the pelvis is *slightly* contracted in the conjugate diameter of the brim, than when the distortion is *excessive*."[1] And he adds one case. "In one of the last cases," says he, "of ruptured uterus to which I was called, dissection showed that the linea ileo-pectinea, where it traverses the pubis, was formed into a very sharp ridge, that there were a number of bony prominences jutting from the inner surface of the pubic bones towards the cavity, and one, especially, situated above the left thyroid foramen, which was so pointed as to pain the finger when hard pressure was made on it. The sacro-pubic diameter was two inches and three-quarters in extent. It was the woman's second pregnancy; the first child had been delivered by craniotomy. After a consultation was held, labor on this occasion was induced in the eighth month by the exhibition of four doses of ergot. The membranes broke spontaneously three hours and a half before the accident occurred. I was sent for by the gentleman in attendance, immediately, and delivered by turning; she died on the night of the fourth day."[2]

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[1] Obstetric Medicine and Surgery, p. 350.

Dr. Churchill expresses similar views in regard to contraction of the pelvis as a cause of rupture. "A certain amount," says he, "of narrowing of the upper outlet may give rise to it. This is a purely mechanical cause. The head of the child is forced down by violent labor-pains, but is unable to enter the pelvis from the contraction of the upper strait. Now, if the pains continue with great power, the head is turned to one side or the other, or posteriorly, and the only obstacle here being the uterine or vaginal parietes the head is drawn through them at the weakest part. They offer the less resistance, probably from the woman having born several children."[3]

Other authorities might be adduced to the same effect, viz., that rupture of the uterus is especially liable to happen under protracted labor or instrumental delivery, when the brim of the pelvis is slightly contracted, that is, in exactly the class of cases most likely to be adapted for delivery by turning.[4]

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[1] Obstetric Medicine and Surgery, p. 469. [2] Ibid., p. 469.

[3] Theory and Practice of Midwifery, p. 408.

[4] See, for example, Dr. Robinson, Edin. Med. and Surg. Journal, vol. xlii. p. 61. "In a great majority of instances," says he, "of slight contraction of the inlet, in perhaps three cases out of four, the diminution of space is caused by the promontory of the sacrum encroaching upon the antero-posterior diameter. On this ground, perhaps, we may explain

In the *second* place, let me observe that, while contraction of the pelvic brim is thus acknowledged to be a strong predisposing and exciting cause of rupture, it must, at the same time, be held in view as a most important correlative fact, that it seldom does lead to laceration of the uterus, unless the effects of its mischievous agency are allowed to be combined with, and increased by, a morbid degree of prolongation and protraction of the labor itself. Dr. Denman's observation is, doubtless, so far quite true, as to the possibility that the "uterus may be *worn through* mechanically in long and severe labors, by pressure and *attrition* between the head of the child and the projecting bones of a distorted pelvis, especially if they be drawn into points, or a sharp edge."[1] In fact the liability to rupture, like the liability to most other obstetric complications, increases progressively with the increased duration of the labor, as the following table, calculated from Dr. Collins' returns,[2] sufficiently attests:

TABLE XV.

Duration of Labor in 24 cases of Rupture of the Uterus.

Length of Labor.	Number of Deliveries.	Number of Cases of Rupture.	Proportion of Cases of Rupture.
Within 6 hours,	13,412	7	1 in 1,916.
From 7 to 24 hours,	2,174	10	1 in 217.
Above 24 hours,	264	7	1 in 38.

The principle inculcated by the evidence of this table is so important in its bearing upon the practice suggested in the present memoir, that I shall take an opportunity of enforcing it by adducing three instances of distorted pelvis, in which laceration of the uterus took place after the labor had been allowed to become protracted; and that, though the mode of ultimate delivery in each of the three was different, in the first, being effected spontaneously, in the second, by instruments, and in the third, by turning.

Case XXXVIII.—"No. 22. On the fifth day after delivery,

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why, in a somewhat greater number of cases of laceration, the rent is found in the posterior rather than the anterior part of the uterus."

Out of 300 cases of rupture of the uterus, reported by Dr. Trask, Monthly Journal and Retrospect for August, 1848, p. 178, contraction of the brim of the pelvis was found noted as existing in sixty cases. Drs. M'Clintock and Hardy, in their excellent work on Midwifery, observe, in regard to the pathology of rupture (p. 295), that the accident is more likely to be produced "where the deformity of the pelvis is *slight* than where it is excessive,"—that is, in precisely those cases best fitted for turning.

[1] Ramsbotham's Principles of Obstetric Medicine and Surgery, p. 469.

[2] Dr. Collins met with 34 cases of rupture during his Mastership. In 24 out of 34 he has given the duration of the labors in a table in his Treatise, p. 370.

without any apparent cause, was seized with violent hemorrhage. When we saw her, which was immediately after, no pulse could be felt, and, though most prompt and active measures were employed, she died in less than an hour. She had been delivered by the natural efforts, of a living child, after a labor, not very severe, of forty-eight hours; nor from that time was there distress of any kind perceptible.

"On dissection, the abdominal viscera appeared healthy, as did the uterus at first sight, but on raising it out of the pelvis, about the size of a shilling of its muscular substance, corresponding to the projection of the sacrum, was found to have given way, the peritoneal covering remaining uninjured. There were two spots in the vagina approaching to a state of slough."[1]

CASE XXXIX.—"No. 28. The labor pains in this case were feeble, yet the child continued to advance. The heart's action was audible with the aid of the stethoscope, the mother's pulse natural, and no unpleasant symptom. Suddenly, however, the most alarming debility came on, the pulse being scarcely perceptible, accompanied with vomiting and much pain on pressure over the uterine region. Immediate delivery was necessary, and the perforator was used. She was a feeble, delicate woman, was thirty-six hours in labor previous to the setting in of the above symptoms, and had been force-delivered eleven months ago. She died in fourteen hours.

"On dissection, an opening was found at the junction of the uterus with the vagina (exactly opposite a projection of the last lumbar vertebra), not larger than to admit the passage of one finger. The muscular substance of the uterus, anteriorly, had also given way to a considerable extent, the peritoneum being whole. The pelvis scarcely measured three and a half inches from pubis to sacrum."[2]

CASE XL.—"A patient with her spine somewhat curved, and who, in her first confinement, had borne a dead infant after several days' suffering, was taken in labor of her second child, early on the 16th of November, 1846. The pains had been constant and strong for about thirty hours, when I saw her, with Dr. Gordon, who had been called in, and the head had remained stationary and fixed at the same point of the brim for the last twenty-four hours. On examination, Dr. Gordon and I found the pelvis small, the os uteri fully dilated, and the head of the infant imperfectly entered into the brim, and apparently uninfluenced in its descent by the powerful uterine contractions that were present. The sutures of the fœtal head were strongly overlapping, and a triangular portion of the fœtal cranium terminating in the posterior fontanelle, was driven deeply in and below the level of the two other portions of bone that went to the formation of that fontanelle. On first touching the head, I thought this indented bone was, as usual, the occipital, but a more careful examination showed me that it was the left parietal bone, and that the head presented its right side, instead of the vertex. The head, though thus greatly compressed transversely, still filled very imperfectly the pelvic brim, and the right side of the pelvis was so unoccupied as to allow a long loop of the umbilical cord to prolapse. The vessels of the cord were beating, showing the child to be still living. The local and constitutional symptoms under which the patient was suffering, showed the necessity of immediate interference and delivery; but how was the delivery to be accomplished? The mass of the head was too high above the brim to allow of the successful use of the long forceps, even if there were space sufficient (which was very doubtful) in the contracted pelvis, to allow of the head passing in this form. The perforation and breaking up of the head by craniotomy would allow of delivery, but the cord showed the child to be alive, and I had doubts here, as in other cases, of the propriety of murdering the infant, when delivery by other means was possible; the other remaining means consisted in its extraction by turning, and, with Dr. Gordon's consent, I proceeded to deliver the patient by this operation; it was accomplished without much difficulty. The child was still-born, but was recovered by the usual means of resuscitation; it survived, however, only a few hours. That part of the scalp covering the parietal bone, which had infringed so long on the promontory of the sacrum, remained depressed, and pitted like an umbilical mark; the left parietal bone itself remained also deeply displaced, and indented below the corresponding edges of the occipital and right parietal, and on dissection, some blood was found effused beneath it. The mother continued well for a few hours, but then began to sink with symptoms of abdominal effusion and rupture. She died on the following day. On dissection, the neck and lower part of the body of the uterus were found lacerated, and at one part the oblique and valvular-like laceration extended through the peritoneum. The pelvic brim was obliquely ovate in form, the right side being much larger than the left, but the conjugate diameter of the brim was very narrow. At its most contracted point, a piece of wood wedged in between the promontory of the sacrum and the nearest point of the pubis, measured only two inches and seven-eighths. The pelvic joints felt loose and mobile."

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[1] Dr. Collins' Practical Treatise, p. 287.

[2] Ibid. p. 300.

The observations which I have adduced in the preceding pages seem then to point to the following deductions:

First, Morbid contraction of the pelvic brim is, whatever mode of delivery is adopted, liable to be a cause of rupture of the uterus.

Secondly, This morbid contraction principally, and almost only, becomes a cause of rupture, when in conjunction with it, the labor, as in the three cases just now detailed, has been allowed to become long and protracted in its duration, and the compressed tissues of the cervix are consequently rendered preternaturally friable and lacerable. Then—

Thirdly, It necessarily follows, that this complication would in all probability be avoided, when morbid contraction of the pelvis exists, provided either nature or art was enabled to deliver the patient early, and did not permit of the tissues of the lower part of the uterus becoming wedged in, contused, inflamed, and softened, between the presenting head of the infant and the opposing points of contraction in the pelvic brim. Hence a power of artificially terminating labors with this complication, as soon after their commencement as the dilatation or dilatability of the passages would allow, would be, so far, a means of averting this fearful accident under these circumstances. In the delivery by the operation of turning, we possess such a power; and by the exercise of it in these cases we may, I believe, not only sometimes save the mother from danger, but the child also from death. And I have already shown at sufficient length, that the degree of force required to extract the head, will depress and groove the cranial bones of the child, without necessarily injuring or tearing the soft pelvic tissues of the mother; provided always these tissues have not been previously brought into a state of congestive and inflammatory friability by the previous duration of the labor, and by the impaction of the head having been allowed to be prolonged to an excessive and morbid degree. All the evidence which I have adduced in different parts of the present memoir goes to show this fact—that the danger attendant upon turning, and other modes of operative delivery is, as a general law, regulated less by the mere performance of the operation, than by the degree of protraction allowed to elapse before operative interference is adopted. And, in addition, the evidence which was brought forward in the last section appears to entitle us to deduce as a supplement or corollary to this important general law, that (supposing the maternal passages are once dilated or dilatable) when operative delivery by the forceps, or crotchet, or turning, is accomplished with *comparative force*, it is safer, *cæteris paribus*, to the mother when performed early, than the same or other modes of operative delivery would be if accomplished with *comparative facility*, but performed late;—that, in other words, force is less dangerous than protraction;—that the hazards of operative delivery are more regulated by the time of its adoption than by the difficulties of its accomplishment.

Fourthly, It must still be held constantly and prominently in view that, in the proposed practice of turning, the great maternal danger which the practitioner has to avoid, is injury and laceration of the uterus, and he must use all due care and caution to avert any chance of this accident. The preceding observations show some of the leading points and objects which he should attend to, in order to avoid it. These points are, principally—1, the early performance of the operation; 2, when possible (as in Case I.) it should be effected even before the membranes rupture, as turning is greatly more easy and more safe before, than after, the evacuation of the liquor amnii; 3, if the liquor amnii has escaped, and the uterus has contracted, the uterine fibres should be previously, and perfectly, relaxed by the use of opium or chloroform; and, 4, the part of the operation consisting in the actual turning and version of the child should be done with extreme caution, and only and always attempted and accomplished during a perfect interval between two uterine contractions.

Other requisite rules and precautions are enumerated so fully in most obstetric works, and so well known to obstetric practitioners, that it is perhaps unnecessary to dwell here at great length upon them.

The pages immediately following appeared several years after the publication of the preceding memoir, as one of Dr. Simpson's occasional contributions to obstetric pathology and practice. We introduce them as showing still further the success of the proposed practice.—(ED.)

III.

(From Edinburgh Monthly Journal of Medical Science, Feb. 1852, p. 135.)

Formerly, medical practitioners seem to have thought little, and medical writers said little, regarding the very repulsive and revolting character of the operation of craniotomy, when performed, as it frequently was, when the child was still living. Apparently, some obstetric practitioners and writers of the present day continue to look upon the practice of craniotomy as one that should not unfrequently be adopted, and one which it is quite justifiable to adopt. Obstetric reports and collections of cases have been published within the last few years, describing craniotomy as performed forty or fifty times, or oftener, by the hand of the same practitioner. But perhaps, ere long, it will become a question of professional ethics—Whether a professional man is, under the name of a so-called operation, justified in deliberately destroying the life of a living human being? For one, I have a strong conviction that, in the kind of case in which the operation is most frequently performed—namely, where there is some obstruction from disproportion, not very great in degree,[1] between the maternal pelvic brim and the fœtal head, the operation is not one which is either morally or professionally justifiable, if the child be still living. Not many years ago, the medical practitioner had this one plea to urge in favor of the adoption of the operation—that *perhaps* the child was already dead; inasmuch as there then existed no certain means of knowing that it was still alive. Auscultation, however, now furnishes us with certain means of settling this question in practice, and has consequently removed this argument in favor of the adoption of the operation. Or perhaps the result would be more correctly given by stating, that in cases of lingering and difficult labor, from some disproportion between the size of the fœtal head and maternal pelvic brim, auscultation can now determine the instances in which the child is dead, and in which, therefore, it is justifiable and right to have recourse to delivery by craniotomy; while it shows us also, on the other hand, the strong impropriety and illegitimacy of adopting the same operation in other analogous instances, where the sounds of the fœtal heart indubitably prove, to the ear of the medical attendant, that the infant continues alive and well.

Assuredly no man would consider himself justified, on any plea whatever, in perforating, and breaking down with a pointed iron instrument, the skull of a living child an hour after birth, and subsequently scooping out its brain. But is the crime less, when perpetrated an hour before birth? Modern physiology has fully shown, that there is no such distinction between the mental and physiological life of an infant, an hour before labor is terminated, and an hour after it, as to make any adequate distinction between the enormity of the act, as perpetrated at the one or at the other of these two periods. And, as if to add to the horrors of craniotomy, when performed upon a living infant, some authors (and among them even the very latest), tell us, that whatever doubts may have existed as to the child being alive or not at the date of operating, the results of the operation itself will decide this point; for if it be alive at the time of the deadly perforation of its scalp, skull, and brain, this fearful fact will be revealed to the practitioner by warm and fluid streams of blood pouring along his fingers and hand, before any masses of broken brain escape; or the reverse.

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[1] During Dr. Collins' charge of the Dublin Lying-in Hospital, craniotomy was used in 124 cases; in 79 instances on account of tediousness or difficulty in the labor. In only one of these cases was the conjugate diameter of the pelvis as small as 2¼ inches. "This," he says, "was *by much* the most defective pelvis I ever met with in the hospital." "The only means," he observes, "of effecting delivery, where the disproportion between the head of the child and the pelvis is so great as *to prevent reaching the ear with the finger*, is by reducing the size of the head, and using the crotchet."

In most cases requiring craniotomy, the contraction at the brim is in the conjugate diameter, from the projection forwards of the promontory of the sacrum—the very kind of deformity in which turning is most likely to be the means of saving the life of the infant.

Unfortunately, no operation in morbid labors is more easy than craniotomy. "Of all instrumental operations in obstetric surgery," says Dr. Ramsbotham, "the perforation of the skull, and extraction of the mutilated fœtus, is the easiest which could be undertaken, for delivery in any case of compacted head; and much do I fear that to the facility with which this operation can be accomplished, have been sacrificed the lives of many children."[1] The operations which midwifery possesses as substitutes for craniotomy are, however, not very difficult in performance. And no conscientious practitioner would surely hold the mere difficulty of an operation, as the criterion by which he should decide upon the act of child-murder or not.

In women who, in previous labors, have had the children removed from them by the operation of craniotomy, nature has herself occasionally pointed out to us, in other labors in the same patients, various means and resources by which that operation could be avoided. Midwifery, as an art, has appropriated some of these hints, and happily applied them in practice. Ever and anon, women, who had been previously the subjects of difficult and dangerous labors, have, when parturition came on *accidentally*, at the seventh or eighth month, borne living children easily and safely. Accoucheurs have, in similar cases, taken advantage of this suggestion, and have had recourse to the *artificial* induction of premature labor. In other instances, in which previous labors have been found difficult or impossible without craniotomy, in consequence of the form of the maternal pelvis, a successful termination has occasionally occurred, when the child happened, in subsequent labors, to present with the feet or pelvic extremity, instead of the head. In such instances, the presentation of the feet or pelvis, or of a hand (requiring the presentation to be *ultimately* made footling), has sometimes, when first discovered at the commencement of labor, been regarded as a source of undoubtedly increased danger and difficulty; when it has at last in reality proved a source of increased safety to the mother, and led indirectly to the preservation of the life of the infant.

The records of cases of difficult labor left us by Mauriceau, Smellie, Hull, &c., show, that in particular forms of difficult labor and deformed pelvis, the passage of the child by the feet or pelvic extremity affords some special facility of transit, which is wanting when the head or cephalic extremity forms the presenting part.

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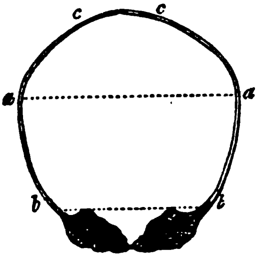
[1] Obstetric Medicine and Surgery, p. 290.

I believe that this apparent paradox is explicable by a reference to the anatomical structure or form of the fœtal head itself. At birth the whole body of the child has in its configuration, been justly compared to that of a cone—the arch, or bi-parietal diameter, of the skull forming the base of the cone—the feet forming its apex; and there being a gradual tapering and diminution of size from the former to the latter points. But the fœtal head itself, when taken alone, presents also, though more imperfectly, the configuration of a cone; the base of the skull being considerably narrower than the arch, or, in other words, when (as represented in the accompanying woodcut) we make a vertical transverse section of the fœtal skull, we find its bi-mastoid diameter, *b b*, is considerably less than its bi-parietal diameter, *a a*,—the cranium increasing gradually in breadth and size from below upwards. The difference between these two diameters in the child at birth generally amounts to from half an inch to three-quarters.

Fig. 32.



Further, it must be held in view, that at its base, or bi-mastoid diameter, the fœtal cranium is so strong, and its bones so strongly united, as to render it quite incompressible. On the other hand, in its arch, or bi-parietal diameter, the cranium at birth is generally so thin and elastic in its bony parietes, and its sutures are so imperfectly united, as to admit of the head being in its upper parts laterally compressed, or even depressed and indented, at some point, without necessarily destroying the life of the child.

In consequence of the above conformation, it happens, that when the child, in a somewhat contracted pelvis, passes as a footling presentation, the cone-shaped head of the child first enters the contracted brim by its *narrower*, or bi-mastoid diameter; and the hold which we have of the protruded body of the child, after its extremities and trunk are born, gives us, when necessary, the power of employing so much extractive force and traction upon the engaged fœtal head, as to compress the elastic sides of the *broader*, or bi-parietal, portion of the cone, between the opposite sides or parts of the contracted pelvic brim, to such a degree as to allow of the transit of the entire volume of the head. In natural labor, the mechanism consists in passing a conical-shaped body (viz., the child) through an aperture (the maternal pelvis) somewhat *larger* than the base of the cone itself (the arch of the fœtal skull); and in this case, when once the base of the cone or head does pass, a single pain is generally sufficient to expel the remainder of the infant—an arrangement by which dangerous compression of the cord is avoided by nature. But when the brim of the pelvis is somewhat less than the natural standard, or the head somewhat above that standard, either from size or malpresentation, the conditions are so far altered, that we have now a conical-shaped body (the child) to be passed through an aperture (the brim of the maternal pelvis) somewhat *smaller* than the body that is to pass. And a little reflection is sufficient to show, that under such circumstances, the passing body would be more easily dragged through the contracted aperture, by bringing the narrow apex of the cone first, than it could be pushed through that aperture by allowing the base, or broad end of the cone, to be presented to it; more particularly, if that broad end is, as we have supposed, somewhat larger than the aperture through which it is to pass.

Since writing upon this subject at considerable length a few years back, I have repeatedly had occasion to turn the child in difficult cases, where the head was not far entered into the brim, and where the long forceps failed, or were contra-indicated; and in which the alternative of craniotomy seemed the only other measure that could be adopted. A number of my professional brethren have reported to me the success with which they have also followed this practice. In the way of illustration, I adduce the three following cases which have occurred in my own practice and in that of my friends Drs. Weir and Peddie, within the last few weeks. I have the pleasure of detailing the cases recently met with by Dr. Weir and Dr. Peddie in their own words.

Case I.—On the evening of January 11th, I was asked to see a case of lingering labor, under the charge of Mr. Keeling and Dr. Cooper. The patient, æt. 26, and pregnant for the first time, had been in labor for about forty-eight hours. The first stage of labor had been terminated about thirteen hours before I saw her; and the head had remained at the brim of the pelvis, without advancing in any degree further down, for upwards of ten hours. On examining, I found the vagina fully relaxed, but its mucous membrane was becoming heated and œdematous, in consequence, in all probability, of the lengthened obstruction of the brim above. The infant's head was elongated, down into the cavity of the pelvis; but the broad part of the head had not passed the brim. The promontory of the sacrum was so easily reached by the finger, as at once to give the idea, that it projected forwards to an extent greater than natural. The direction of the sagittal suture showed that the head lay more transversely than in the usual normal presentation. The face of the infant was directed to the right sacro-iliac synchondrosis, or rather to the right ilium. The patient was put fully under the influence of chloroform, and the long forceps were easily applied. The blades of the instrument were, as we found after the birth of the child, applied as usual, obliquely over the head, and did not offer to slip in any degree under the traction that was applied. But no amount of traction that I thought it justifiable to employ could move the head downwards; and Mr. Drummond failed also in making any impression upon the advancement of the cranium, when I gave him the instrument to use. I altered, in several ways, the direction of the traction, and the position of the patient, but still without any success, and at last withdrew the forceps. Conceiving that possibly there might be some obstruction to the advancement and passage of the child, from some malposition of the arm about the neck, or from some malformation, I introduced the hand by the side of the child's head, for the purpose of ascertaining these circumstances, but found nothing that appeared to me capable of explaining the delay, and the impossibility of advancing the head by the forceps, except it were some oblique position of the head, relatively to the neck or trunk of the infant. A few years ago, I should perhaps in such a case have deemed the operation of craniotomy the only remaining resource. But the stethoscope showed that the child was still alive and well. And, under these circumstances, I resolved to attempt to extract it by the operation of turning. The patient was deeply anæsthetized, in order to relax the uterus as much as possible; and at a time when all uterine contraction seemed absent, I passed up my hand and brought down one of the lower extremities of the child. While doing so, a large loop of the umbilical cord fell down into the vagina. I now found an obstruction to the complete version of the infant, which I had met with previously in other cases of turning, when the head was the originally presenting part. For, though one foot was down at the orifice of the vagina, the version of the fœtus upon its own axis had not been complete, and the head was still at, or near the brim. Consequently, while Mr. Drummond held and retained the extruded foot, I passed up my right hand to the head to push it upwards, so as to complete the version—a part of the operation which, in this as in other cases, is always much aided by the manipulation of the left hand upon the abdomen externally. Subsequently the trunk and arms were easily extracted, and the traction requisite to make the head pass the brim was much less than I had seen in several similar cases. The child after birth had the heart still pulsating, and was readily revived by repeatedly plunging its body alternately from a warm into a cold bath. Yesterday, 21st of January 1852, I saw both the infant, which was, perhaps, rather above the usual size, and the mother quite well.

CASE II.—" On the evening of the 2d of December 1851, I was requested," Dr. Weir writes me, "by my pupil Dr. Bone, to give an opinion upon a case of protracted labor. The patient had been eighteen hours in labor of her fifth child. She had been on previous occasions the subject of tedious and difficult labor, except in one confinement, when the child was born below the usual size. Her face was flushed, eyes suffused, skin hot, pulse quick, and restlessness great. The os uteri was completely dilated, and the liquor amnii discharged. The head was still at the brim of the pelvis, and although the pains had been for many hours quick, strong, and expulsive, a very small portion of it penetrated the pelvic cavity, and so much as did, receded above the brim upon the cessation of the uterine contractions. Under these circumstances, I determined to deliver her immediately; and as it appeared a favorable case for turning, decided upon doing so in preference to using the long forceps. The chloroform was administered till deep snoring was produced, and the hand introduced in the usual manner. So completely were the uterine efforts suspended, that I grasped both the feet before the uterus contracted in the slightest degree upon my hand. No difficulty was experienced in extracting the child till the head reached the brim, when considerable force was required to draw it through, but not so much as to prevent its being born alive. The mother did well, and in a few days was attending to her household duties. The child presented after birth a deep depression of the cranium on the anterior part of the left parietal bone, immediately above the ear—the result, no doubt, of compression against the promontory of the sacrum. But this has not affected the health of the child, which now, 20th January 1852, is otherwise thriving and well."

CASE III.—" On the 30th November 1851, I was called," writes Dr. Peddie, " to Mrs. ———, aged 30, in her sixth confinement.

" At her first confinement, November 10, 1843, in consequence of contraction at the brim of the pelvis, principally on account of an exostosis projecting from the promontory of the sacrum, and to some extent also from an under-average size of the pelvis, she was delivered by embryulcio. This extreme measure was not resorted to until forcible natural pains had existed for many hours, without effecting an entrance for the head at the brim of the pelvis; until the long forceps had been applied, first by myself, and then by Dr. Simpson, without obtaining any advance of the head; and until the fœtal circulation had been ascertained to have ceased. After the perforation was employed, no small difficulty was experienced in dragging the child through the pelvis.

" At her second confinement, 26th September 1844, after allowing four hours to elapse from the time when the os uteri was fully dilated without the smallest descent of the head, I succeeded, with the long forceps and powerful traction, in delivering the child safely. This fortunate termination was the more satisfactory, that the induction of premature labor had been considered and decided against some months previously.

"At her third confinement, 20th April 1846, the labor went on naturally, and the child was expelled without artificial assistance. The explanation of this fortunate event appeared to be, that the child was a female one, and considerably under the average size, the head more particularly being small.

"At her fourth confinement, 28th October 1847, the labor resembled Mrs. ———'s first, the first stage being short, while the second had existed for many hours before I saw her. The pains were extremely forcible, conveying the impression of danger from rupture of the uterus. After trying two different pairs of long forceps, I sent for Dr. Simpson, who furnished me with a third pair —which, though easily applied and powerful, did not enable me to bring forward the head in the least degree. Dr. Simpson then proposed turning in preference to perforation—the more especially as the child was ascertained to be still alive; and this he accomplished readily, the patient being very deeply under chloroform, and delivered her after the employment of very powerful traction. The child was at first as if still-born, but was brought about after the continued use of the usual means.

"At her fifth confinement, 11th December 1848, the presentation was a footling one—as if nature was indicating the right mode of procedure; and, accordingly, when the first stage was completed, I seized hold of the other foot, brought it down, and delivered the child safely, but not without the employment of very considerable traction in bringing the head through the pelvis.

"At her sixth and last confinement, 30th November 1851, I saw Mrs. ——— at eleven A.M. Pains had begun about twelve o'clock on the previous night, and had gone on regularly, but not severely, until within two hours of the time of my visit. I found the os completely dilated, the vagina filled with tensely distended membranes, and the head above the brim. On passing the hand to make a proper examination, I recognized the old exostosis, but decidedly larger, fully the size of a small walnut, projecting from the sacrum immediately within the right sacro-iliac symphysis. I found also that the head was lying with the occiput to the sacrum, pointing somewhat to its left iliac symphysis, and the forehead to the right side of the symphysis pubis. I was also satisfied that the head was of large size, while the pelvis generally was small. . . . .

"Considering previous experience in the case of this patient, and the remarkable success of turning in her fourth labor, when she could not have been delivered otherwise than by the life-sacrificing perforator; and considering that in the present labor the peculiar position of the head would render efforts by the long forceps—however well employed—useless, I resolved at once to turn. This I accomplished, and brought down the body very satisfactorily, while the patient was placed deeply under chloroform by Dr. Harper of Leith, who kindly assisted me. As was to be expected, much exertion and pulling were necessary to drag the head through the pelvis, although the most favorable respective diameters were chosen. A towel slipt round the anterior part of the neck and chest, and crossed over the shoulders behind, gave steady purchase, and enabled me more readily to complete the delivery. The child was a large boy, and at first apparently still-born; but in about twenty minutes after unceasing attention, by giving alternate plunges in hot and cold baths, as practised in the first and second labors, and by the use of artificial respiration, he was completely restored. He ultimately did well, and Mrs. ——— made an excellent recovery. The child weighed 9 lbs. 5 oz."

In conclusion, let me briefly recapitulate some of the principal advantages which, as it appears to me, the operation of turning has over the operation of craniotomy, in cases such as we have been considering in the present communication, viz., where the pelvis is somewhat too small, or the fœtal head somewhat too large, to allow the infant to pass by the unaided efforts of nature, or even with the assistance of the long forceps, if that instrument is had recourse to.

1. It substitutes the delivery of the infant by the hand of the accoucheur, for its delivery by formidable steel instruments. And, certainly, the avoidance of instruments is, as a general principle, desirable when it is possible.

2. The transit of the cone-shaped head of the child through a somewhat narrow brim is facilitated by the narrow end of the cone (or bi-mastoid diameter of the head) being made to enter and engage first in the contracted brim; and the hold which we obtain of the extruded body of the child enables us to employ so much extractive force upon the engaged fœtal head, as to make the elastic sides of the upper and broader portion of the cone (or bi-parietal diameter of the cranium) to become compressed, and if necessary indented, between the sides of the contracted brim.

3. When the child is brought down footling, we have far more power than when the spherical arch of the cranium presents, of manually adapting and adjusting, when necessary, the shape of the head to the shape of the contracted brim; the rounded form of the cranium not affording us any sufficient hold and purchase for this purpose in cranial presentations.

4. The *lateral* and very *temporary* compression of the fœtal head by the contracted sides of the pelvis, such as we can produce and effect on artificial turning and contraction, is less dangerous to the life of the child than its *oblique* or longitudinal compression with the long forceps, or by the *long* impaction of the head itself in the contracted brim.

5. In cases where the narrowness is greater, and such as to produce a depression or indentation in the elastic and flexible cranium of the child, still this transient depression, or indentation, is not necessarily destructive to life, as the perforation of the head in craniotomy is. Children often survive and recover, when born with the head much distorted and even indented. See, for example, the child in Case II., and other similar instances recorded by Smellie, Denman, Velpeau, Duges, Jacquemier, Radford, &c. &c.

6. On these accounts, the operation of turning affords a fair chance of life to the child, while craniotomy affords none. And even when the turning and extraction require some considerable time for their performance, the resulting temporary asphyxia of the child is not necessarily so deep and fatal but that the infant may be revived by appropriate measures applied after birth. I can, for one, state that in these cases, and in instances of common footling and turning cases, I have repeatedly been astonished at the viability of the infant after traction had been applied to it, both so strong in degree and so long in duration as to leave apparently little hope of its survival; and I have heard other practitioners make the same remark as the result of their experience.

7. The operation of turning, under the circumstances we speak of, will, I believe, be found also to be more safe to the life of the mother than the operation of craniotomy. In every instance the operation of craniotomy is necessarily fatal to the infant; but in a very large proportion also, this operation is fatal to the mother. The statistical results collected by Dr. Churchill and others show that craniotomy is fatal to the mother in about 1 in every 5 cases in which it is performed; while turning does not generally prove fatal in above 1

in every 15 or 16 patients, even including complicated cases.[1] Besides, it affords this great source of safety to the mother, that, *cæteris paribus*, delivery by turning can be, and is, as a general rule, adopted far earlier in the labor than delivery by craniotomy; and in proportion as it is practised earlier, so far also will it be practised with greater safety and greater success—the maternal mortality attendant upon parturition, whether natural or operative, increasing always in a ratio progressive with the increased duration of the labor.

Among the operative deliveries which occurred in the Dublin Hospital when Dr. Collins was master of the institution, the duration of the labors at the time of operating is stated in 125 cases. Among these 125 instrumental and operative deliveries, only 1 in every 17 of the mothers was lost, when the delivery was accomplished within twenty-four hours from the commencement of labor; 1 in every 7 of the mothers died when the delivery was delayed till from twenty-four to forty-eight hours; and nearly 1 in every 2 mothers perished when the delivery was delayed till the labor had gone on above forty-eight hours. Obstetricians have often argued, that if, in cases of obstructed labor, the delivery is delayed for a sufficient length of time, the child will be ultimately destroyed by the uterine action and compression, and that thus craniotomy will be at last performed upon the dead infant—the child being killed by an act of omission, and not of commission. But even such very protracted delay is not always fatal to the infant—some continuing to survive when the labor is prolonged for sixty, or seventy, or more hours.[2] And it is always to be remembered, that the delay itself, if dangerous to the life of the child, is also, as the above and other evidence shows, almost equally dangerous to the life of the mother. In such cases of long obstruction and delay, even after the head is perforated by craniotomy, much traction is often required to drag the shoulders through the contracted brim, and that at a time when the structures at the brim are so damaged by previous pressure as to be little able to bear compression with safety. And I do think that we have most ample grounds for believing, that the *long* compression of the soft parts, such as occurs in very protracted labor, is more truly dangerous to the structures than a *short* compression of them, greater in amount, such as occurs in the operation of turning when early performed.

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[1] Out of 303 craniotomy operations, 60 of the mothers died, or 1 in 5.—Churchill's Midwifery, p. 314. Out of 192 cases of turning, 12 mothers died, or 1 in 16.—Ibid. p. 250. "Between," says Dr. Ramsbotham, "the years 1823 and 1834, I delivered more than 120 women under transverse presentations, independently of a few cases to which I was summoned, where spontaneous evolution occurred. Many of these cases presented a formidable appearance; for in one the membranes had been ruptured a whole week; in another, 69 hours; in a third, 58 hours; in another, 55; in another, 53; and in many, more than 48; and, as a general principle, we presume, that the longer the liquor amnii has been evacuated, the more likely is the uterus to have embraced the fœtal body firmly, and the more difficulty will there be in overcoming the resistance. In none of these cases did I exhibit large doses of opium, and in those few where bleeding was practised, that operation was had recourse to, not for the purpose of relaxing the rigidity of the uterine fibres, but to relieve the inflammation which the soft structures were suffering, and to remove tumefaction. In not one of these instances was any injury inflicted on the uterine structure by the hand, nor did any permanent evil arise that could be attributed to the operation. In four cases only was the uterus so powerfully contracted as to refuse admittance to the hand, and compel me to adopt the alternative of eviscerating or decapitating the fœtus."—Obstetric Medicine and Surgery, p. 362.

[2] Out of 27 cases reported by Dr. Collins, in which labor was prolonged to 60 hours and upwards, in 16 the child was born dead; and in 11 it was still alive at birth. Of the 27 mothers, 1 in 4 died.

In not a few cases, in which the operation of turning is resorted to in consequence of the complication which we have been considering, the practitioner must be prepared to meet with such resistance to the passage of the head through the brim as will require some adjustment and considerable physical exertion on his part in order to overcome it. But if the head be so adjusted in the brim, that the shape of the one is, as much as possible, adapted to the shape of the other; if the chin be kept depressed towards the sternum; and if the traction applied be made in the proper axis of the brim itself, no small amount of extractive force may be used without compromising the safety of the mother or infant. The degree of traction which the structures of the infant's neck will, in this way, undergo, is much greater than one would *a priori* suppose. And, as a very general rule, the elastic lateral walls of the cranium of the child will become compressed or indented, before any dangerous injury is inflicted upon the structures of the neck. But on this subject I most willingly substitute, for any remarks of my own, two or three sentences from the work of an author—always practical and always cautious—Dr. Denman, who, in speaking of the occasional difficulty of extracting the head in common pelvic and turning cases when the brim is somewhat contracted, gives, among others, the following directions:

"The force with which we endeavor to bring down the head of the child must then be gradually increased, till we are convinced that a greater degree is inconsistent with the safety of the child, or induces the hazard of separating the body from the head. Should the head descend in ever so small a degree, we must not act precipitately, and increase the force in order to finish the delivery suddenly; but we must proceed with circumspection, or we shall add to the danger which the child is already in, and run the risk of doing injury to the mother; though, when the head begins to advance, there is seldom much remaining difficulty, the cause usually existing at one particular part of the pelvis. It has been said that children have sometimes been born alive, when the strongest efforts, and these continued for many hours, have been made to extract the head detained in this position. But I have not been so fortunate as to meet with any such instances, a short space of time having generally been sufficient to frustrate my hopes, and convince me that the child was dead. Though, when the head has been detained a considerable time, a few cases have terminated more favorably than I could have expected, and I have been agreeably surprised with the discovery of some faint signs of life, which by the assiduous and careful use of the common means, have been improved, and the life of the child at length perfectly recovered. * * * When we have in vain exerted all the force which we think reasonable and proper, and which, in some cases, must be more than any circumstance would be thought to require, it will be expedient to rest, for the purpose of gaining all the advantage to be gained by the compression of the head. On this account the mother will actually suffer no more inconvenience than would have been purchased if the head had originally presented, and been locked in the pelvis. After waiting some time, we must renew our attempts to extract, and thus proceed, alternately resting and acting with efficacy and resolution, and if the hold we may have of the body or extremities of the child does not suit, a silk handkerchief or other band may be passed round its neck, and this will be found a very handy and convenient instrument. It must," Dr. Denman adds, "be a very great disproportion between the head of the child and the pelvis, which is able to withstand this method of proceeding, if we persevere in it with prudence and steadiness; because the integuments of the head will burst, or the bones be bent inwards in an extraordinary degree, or even broken. * * * If it fail, it then only remains that we should lessen the head of the child; and the operation may be as easily performed in this as in the natural presentation of the head. When," he continues, "the perforation is made, and the brain evacuated, the head may be readily extracted either by pulling by the body of the child or by inserting a crotchet in the opening made by the operator as in other cases. But it will be scarcely believed how seldom this operation is necessary under these circumstances, if we have not been in a hurry, but have acted with prudence. Nor," concludes Dr. Denman, "have I ever known any ill consequences follow the compression which the soft parts undergo between the head of the child and the sides of the pelvis, if proper attention were afterwards paid to the state of the bladder and rectum."[1]

REMARKS ON THE OPERATION OF CRANIOTOMY.[2]

(From British and Foreign Medical Review, Oct., 1841, p. 486, etc.)

No surgical operation whatever is, abstractly considered, more revolting to human nature than that of craniotomy or embryulcio; it is, at the best, a dreadful expedient. In too many instances it implies the direct and deliberate murder of a fellow-being by the hand of the accoucheur. It is one of the few operations, the propriety or non-propriety of which has engaged all the logical subtlety of the metaphysicians. Some of the greatest foreign authorities of the present day doubt if it ever ought to be performed so long as the child is alive. But we have no desire to discuss the question of the morality of the operation.

In performing the first part of the operation, namely, the perforation of the head, Drs. Ramsbotham and Davis both advise us to use Smellie's scissors, made with a cutting edge to both sides of each blade; Dr. Rigby recommends the perforator of Naegele, the chief peculiarity of which is that, like the analogous instruments invented by Steidele and Von Busch on the continent, and by Drs. Bone and Holmes in our own country, the blades do not cross at the lock, and the operator can, by closing the handles together with one hand, make their cutting points sufficiently diverge, thus leaving free the fingers of the other hand in the vagina to guard and protect the maternal parts from any threatened injury. Both authors properly advise the perforation to be always made in the presenting parietal bone, and not at a suture. Dr. Rigby, after perforating, throws a strong stream of water into the cavity of the skull through a long elastic tube, in order to break down and wash out as much as possible of the substance of the brain, and thus allow the cranial bones to collapse more readily and completely.

In extracting, after perforation, Dr. Ramsbotham uses the common straight crotchet, and well remarks that simple as the instrument is, it is one of the most difficult in midwifery to procure good, as the least variation of the degree of sharpness of its point makes a considerable difference in its value. He states further that the use of the blunt hook may sometimes supersede the employment of the crotchet when the bones are too loose and fragile to afford a purchase to the latter. The craniotomy forceps are in his opinion more dangerous, and less to be trusted to than the crotchet, but the several objections which he makes to these forceps refer to the instrument in its worst form, with a Smellie joint and immense teeth, and scarcely apply to the simpler and more efficient varieties of it.

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[1] Introduction to the Practice of Midwifery, p. 495, &c.

[2] From a Review of the works of Rigby, Ramsbotham, and Davis.

We beg to add only one remark in regard to extraction. Whatever means or whatever instruments are employed to effect it, we have long been of opinion that one great principle should direct their application, namely, they ought to be fixed in such places and used in such ways as will enable us to bring down the head in its most natural and consequently most favorable *position*. We have long been convinced that the great peril, not merely of injury but of death to the mother from this operation, arises in a great measure from the head being so often so altered in its presentation, and hence necessarily so increased in its diameters by the mode and place in which the crotchet is fixed, that the cranium is at last torn as it were through the pelvic passages by pure physical force alone; while a little attention to the proper adaptation of the instrument so that the head may be brought down in its most favorable position, will generally be found to lessen immensely both the difficulty and the danger of the operation.[1] We speak here of the more common cases of embryotomy. We are perfectly aware that when the pelvis is much contracted, we are obliged, as has been well pointed out by Dr. Hull, to alter in different ways the presentation of the head, always, however, bringing it into such positions that its diameters are in each case those requiring the least possible space.

As regards the *mother*, craniotomy has, in general, proved an infinitely more dangerous operation than the forceps, and we have the greatest doubts of the soundness of the rule laid down by some accoucheurs, that in individual cases the presence of signs of the death of the child ought to determine our having recourse to the perforator and crotchet in preference to the forceps. The rule is a dangerous one in two points of view: first, because, as we have just stated, craniotomy seems attended with actually greater risk to the mother; and secondly, because it is exceedingly difficult to make ourselves certain, in special instances, of the death of the infant. All negative symptoms of its death are very equivocal; even if the cord be felt pulseless, still we may not be certain that it is the cord of the child whose head is presenting. Dr. Ramsbotham has properly shown this one source of error, that it may happen to be the cord of another twin infant. The stethoscope in some of these doubtful instances is of the greatest use in determining the presence of *life* in the infant; and in thus encouraging us to persevere as long as possible in other means, as in the use of the long or short forceps, before we have recourse to the final and fatal operation of craniotomy. By its aid in this way we have been induced to use the forceps, and with the happy result of saving the child, when, previously to auscultation, its head had been doomed to perforation by others. We do not, however, on the other hand, hold that because the sound of the fœtal heart cannot be heard, the indication is of equal value. We do not perform craniotomy in individual cases because the child is dead, but because the mother's life is in danger, and delivery cannot be completed by other means less safe to her. That the employment of the forceps is, as a general rule, safer to the mother herself than the employment of the crotchet, is a proposition which Dr. Churchill of Dublin and others have proved by ample statistical data. Our German and French neighbors have of late years, in consequence of this and other reasons, been employing the forceps much more, and the crotchet much less, frequently than we do in Great Britain. We trust yet to see the same reform work its way into English obstetric practice.

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[1] When the crotchet is fixed upon the posterior part of the parietal or upon the occipital region, the infant's head can be brought down through an aperture an inch or so less in diameter than when the crotchet is fixed upon the frontal region. In fact, when the crotchet is fixed on the forehead, it brings the head down in the increased diameter required by an ordinary forehead presentation. Yet Dr. Ramsbotham (plate 59) and Dr. Churchill (Fig. 89) have both inadvertently represented the crotchet as usually fixed to the frontal region.

RELATIVE STATISTICS OF ARTIFICIAL DELIVERY.

VIENNA—PARIS—DUBLIN.[1]

(From Edinburgh Monthly Journal of Medical Science, June, 1851, p. 546.)

THE following are the results to the mothers and children, from artificial delivery by the forceps, vectis, craniotomy, and turning, in the three largest lying-in hospitals of Europe, as respectively reported by B er and Arneth relatively to the Vienna Hospital, by Lachapelle and Boivin relatively to the Paris Hospital, and by Drs. Collins, M'Clintock, and Hardy, relatively to the Dublin Hospital.

1. The proportion of cases of operative or artificial delivery of the child is very nearly the same in all these three great hospitals. In the Vienna Hospital, under Böer, 1 out of every 55 women was delivered by one of the operations we have named, namely, by the

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[1] From a Review of Arneth's Midwifery.

forceps, vectis, craniotomy, or version; and during the time of Dr. Arneth's report, operative delivery was resorted to in 1 out of every 69 cases. In the Paris Hospital, Madame Boivin reports 1 out of every 61 labors as requiring delivery by operation. Madame Lachapelle found that in the 10 years preceding 1810, 1 in 57 mothers was delivered artificially, and during the subsequent 10 years, 1 in 82 required such a procedure. In the Dublin Hospital, Dr. Collins reports 1 out of every 86 women as having been delivered artificially; and Drs. M'Clintock and Hardy describe 1 out of every 52 of their cases as having been similarly assisted.

2. Though the total proportion of operations is thus not very different in these three large hospitals, yet the results to the children are very diverse. We shall give these results in round numbers, and without copying the minute fractions that Dr. Arneth has appended. In the Vienna Hospital, in his operative deliveries, Böer lost nearly 1 in 2 of all the children; Arneth, 1 in 3, or 33 out of 95 cases; in the Paris Hospital, in the same kind of cases, Lachapelle reports less than 1 in 2 of all the children as having been still-born, or 207 were still-born out of 541 deliveries; while Madame Boivin reports about 1 in 4 of the children as having been lost, 95 having been still-born in 334 artificial deliveries. In the Dublin Hospital, in his cases of artificial or operative delivery, Dr. Collins reports about 3 in every 4 children as having been lost—150 of the children out of 193 cases having been still-born; and Drs. M'Clintock and Hardy return, under the same circumstances, a nearly similar amount of infantile mortality out of 128 operative cases reported by them—the infants being still-born in 98 instances. Or perhaps we may state these results, as regards the infantile mortality in operative cases, more intelligibly in percentage proportions. Taking this method, the results are as follows: Out of every 100 operative cases, Böer lost about 47 of the children; Arneth 34; Lachapelle 36; Boivin 28; Collins 77; and M'Clintock and Hardy 76.

3. The maternal mortality in operative cases is a matter of even still greater moment and greater diversity. Unfortunately, we have no records on this point from the Parisian Hospital, as Lachapelle and Boivin have not published any general statistics relative to the fate of the mothers. The maternal results, however, in these cases, are given with sufficient fulness in both the Vienna and Dublin reports. Böer lost 1 out of every 17 mothers in his cases of operative delivery; Arneth 1 out of every 9. In his cases of operative delivery, Dr. Collins lost 1 out of every 4 mothers; and Drs. M'Clintock and Hardy nearly 1 out of every 5. Or, to state the

result in percentage proportions, out of every 100 operative or artificial deliveries, Böer lost 6 mothers; Arneth 12; Collins 25; and M'Clintock and Hardy 22.

As some explanation of these differences in cases of morbid labor, in the results to the mothers and children, we may state one fact, without entering further into the discussion of the subject. In almost all, or indeed all, operative cases in the Dublin Hospital, in which the head of the child has not completely passed down into the pelvic cavity, craniotomy is employed—an operation always, of course, fatal to the child, and extremely dangerous to the mother.[1] In the Vienna Hospital, on the other hand, they deliver the child by turning, and not by craniotomy, when the head is above the brim; and they apply the forceps in other cases, where the head has *partially* descended through the brim, but which in Dublin would still be regarded and treated as crotchet cases.

We cannot but express our regret, first, that such statistical returns from our larger hospitals should not appear more frequently; and, secondly, that some general and uniform system of statistical observation has not yet been agreed upon among the accoucheurs of the different lying-in Hospitals of Europe and America—an arrangement that would add greatly to the practical value and importance of such reports.

We have thrown the preceding statistics into a tabular form, to render the contrast they present still more evident.—(Ed.)

Reporters.		Proportion of Operative Cases.	Percentage of Infants Lost.	Percentage of Mothers Lost.
Vienna,	Böer, ...	1 in 55 labors,	47 in 100	6 in 100
	Arneth, . . .	1 in 69 "	34 in 100	12 in 100
Paris,	Boivin, ...	1 in 61 "	28 in 100	
	Lachapelle:—			
	1800–10, . .	1 in 57 "	86 in 100	
	1810–20, . .	1 in 82 "		
Dublin,	Collins, . . .	1 in 86 "	77 in 100	25 in 100
	M'Clintock and Hardy, .	1 in 52 "	76 in 100	22 in 100

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[1] The two great causes of maternal danger and death attendant upon craniotomy appear to be—1st. The very long duration to which the labor is generally allowed to go on before this mode of delivery is adopted—(see antecedently, p. 473); and 2d. The mode and position in which the head is liable to be seized and dragged down—(see p. 553).—(Ed.)

ON THE STATE OF THE FŒTAL PULSE DURING LABOR AS AN INDICATION OF DANGER TO THE CHILD.[1]

(From Edinburgh Monthly Journal of Medicine, April, 1855, p. 346.)

Dr. Simpson made some remarks on the indications afforded by the stethoscope for expediting delivery, and especially directed the attention of the Society to the fact, that while danger was usually indicated to the mother during labor, by the increased rapidity of her pulse, the death of the child was most frequently threatened when the fœtal pulse became slower and slower.

It was known that in cases where, during labor, pressure was exercised upon the cord, as in presentations of the cord, the pulsations of the fœtal heart became feebler, and were at length suspended by the continuous pressure. This was apparently the way in which the fœtus[2] generally perished, during severe and prolonged labors—the aëration of the blood by the placenta being thus rendered imperfect, or entirely suspended.

There were, however, rarer cases in which danger was indicated to the child, by the fœtal pulse becoming much more rapid than ordinary, reaching 150 or 160 beats in the minute, at the same time being often very irregular or intermittent. Dr. S. believed the danger in these latter instances did not result from pressure on the umbilical cord, as in the cases where the pulsations became slower and slower, but arose from pressure or some source of irritation acting on the brain.

TRANSVERSE PRESENTATIONS.[3]

(From Lond. and Edin. Monthly Journal of Medical Science, February, 1845, p. 109.)

At this Meeting I propose to offer a few observations on the instance of transverse presentation and turning, which you witnessed in the Maternity Hospital two days ago. First of all, allow me to read to you the case as it has been reported by my excellent friend Dr. Martin Barry.

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[1] Proceedings of Obstetrical Society of Edinburgh, Session xiii. 1854–55.

[2] For some practical deductions and remarks on this subject, see a preceding paper, pp. 476, 479.—(Ed.)

[3] A Clinical Lecture. Reported by Mr. C. D. Arnott.

"Christina Anderson, aged 28, married—third pregnancy—last catamenia in the beginning of March, continuing for two or three days—time of quickening uncertain.

"On the morning of Saturday, 7th of December, had a fall, in which the left hypochondrium was violently dashed against the corner of a large stone; says that she did not feel the movements of the child for longer than a day or two after this. On Tuesday the 17th—ten days afterwards—the child, when born, presented the separation of nearly the whole cuticle, with sero-sanguinolent infiltration of the scalp, and a state of looseness of all the cranial bones. The abdomen was swollen, and on laying it open, the spleen was found much enlarged, and patches of coagulable lymph were seen on its peritoneal surface; similar patches were present in the iliac and pelvic region connecting the peritoneum of the abdomen with that of the intestines; other portions of a similar inflammatory effusion were scattered over the intestines, more especially in the lower part of the abdomen.

"Labor commenced on Monday, 16th of December, and was completed in seventeen hours. The membranes ruptured twelve hours before the full dilatation of the os uteri.

"During the labor the patient was bled, and had an ounce of antimonial wine exhibited in ʒss doses, to promote the dilatability of the cervix uteri. Shortly before turning, ʒi of laudanum was exhibited, in order to moderate the uterine contractions, and facilitate the process of turning.

"The presentation was the right shoulder with the cord, and the child was removed by turning and extraction. This was performed by Professor Simpson, who, observing from the feet and legs, when they had been drawn down, that the position of the pelvic extremity of the child corresponded to the third position of the head with the toes pointing forwards, stated that the body as it came into the world would present a corresponding movement of rotation—a movement forthwith seen by the writer and several others who were present. The head was then born in the second, or right occipito-cotyloid position."

Most of you are aware that we cannot, except in the rarest cases, leave instances of shoulder presentations to nature. Experience abundantly shows us, that if we do not offer the required aid in these presentations, the woman, in general, either dies exhausted from the long-continued but fruitless efforts of the uterus to empty itself of its contents, or the organ, by the violence of its own contractions, lacerates its own structure, and thus establishes one of the most fatal complications to be met with in the practice of midwifery, namely, rupture of the uterine walls. In these presentations, therefore, interference is imperatively required, and this interference consists in so far altering the morbid position of the infant, that, instead of allowing it to remain with its long axis across, or at right angles to the long axis of the uterus, we introduce our hand, and bring down to the os uteri one of the two extremities of the *ovoid* mass of the child (the child in the latter months being, as you know, folded up into an irregular ovoid form), in order to make the long axis of the body of the infant and the long axis of the body of the uterus parallel to each other. For this purpose, we may bring down either the cephalic or the pelvic extremity of the infant.

The cases are few in which it is proper and possible to bring down the head of the child to the os uteri, or, in other words, in which we can and should perform what is termed *cephalic* turning or version.

In almost all cases, but especially in one like Anderson's, with the child dead, and the passages well dilated or dilatable, we bring down the pelvic extremity of the infant, and so far change the presentation from one of the shoulder or arm into one of the breech, feet, or knees.

Many modifications of this last operation—*podalic* version or turning, as it is termed—have been proposed, and different varieties of it are advocated by different practitioners. By some we are advised in all cases, to bring down *both* feet of the infant. Others, again, assert that to bring down *one* is always sufficient. Some advise a foot or feet; others one or both knees. Let us inquire what method would be the most simple and most practicable in a case like the one before us.

I would first, however, remark, that in turning, in any case, where the membranes are still fortunately entire, the female passages dilated, and the fœtus perfectly mobile in the liquor amnii, it matters, I believe, little whether we seize one or both feet, or one or both knees. All these parts are, under such conditions, so easily reached, and the whole operation of seizure and version of the child is then so simple, that it is almost needless to lay down set and formal rules (as some authors have done) for the exact part of the child which you should seize, and the exact mode in which you should seize that part. There is so much room to turn, as the child still floats in the liquor amnii, that, if you can seize *firmly* almost any part of one or both of the lower extremities with the introduced hand, you will readily effect your purpose. The flexure of the knees affords a firmer and better hold than the foot or feet, and is hence probably preferable. It is, however, I repeat, sufficient to seize firmly any of these two parts, whichever of them first comes within your reach.

But the operation of version, in a shoulder or arm presentation, becomes a much more difficult, delicate and dangerous operation, when you require to turn, as in Anderson's case, with the waters already evacuated, and the uterus tonically contracted, and folded around the body of the child. Under such circumstances, we require to know, and to study beforehand, the easiest method of operating for ourselves, and the most safe mode of operating for the mother.

Let us consider, therefore, what method of turning is the most simple, and the most practicable, under such conditions as I have just stated; and first of all let us inquire—

SHOULD WE SEIZE BOTH FEET OF THE INFANT?

You see this method of procedure represented in the plates before us of Moreau, and it is a mode which is strongly inculcated by some of our latest authorities in midwifery. "Never be content," says Dr. Lee, in his lately published lectures on midwifery, "never be content with one foot, when it is at all possible to grasp both." In most cases, I hold this method to be improper and unjustifiable, because it is almost always more difficult to seize both extremities than to seize one; because one is quite sufficient for our purpose, and more safe for the life of the mother; and because by pulling at one extremity, when pulling does happen to be required after the version is accomplished, we more perfectly imitate the natural *oblique* position and passage of the breech of the infant, than when we drag it down more directly and more upon the same plane, by grasping and dragging at both limbs equally. The infant also assuredly incurs less risk of impaction of the head, and above all, less chance of fatal compression of the umbilical cord, when the os uteri and maternal canals have been dilated by the previous passage of the breech, increased in size by one of the lower extremities being doubled up on the abdomen, than when both extremities being seized and extended, these same passages are more imperfectly opened up by the lesser-sized wedge of the breech alone.

Notwithstanding, however, the greater difficulties, and consequently the greater dangers attendant on the operation, when we search for, and grasp both lower extremities, instead of one, it is still so dogmatically laid down as a rule by most obstetric authorities, that many practitioners seem to deem it a duty not to attempt to turn the child without having previously secured both feet.

About two years ago, I was sent for by an accoucheur, enjoying a deservedly extensive practice, in order to assist him in the operation of turning in such a case as Anderson's. He had introduced his hand, and brought down one foot into the passages. For a long time he had been attempting to reintroduce his hand into the uterus, for the purpose of seizing and bringing down the remaining foot. His continued efforts, however, had proved perfectly useless, and he requested my attendance under this supposed difficulty. Without endeavoring to effect what he had labored to accomplish, namely, the seizure and extraction of the retained extremity, I contented myself with dragging at the one foot already protruded, and with it alone pulled down the child without difficulty.

In few or no cases of turning is it proper or requisite to bring down both extremities, unless in the complication of turning under rupture of the uterus. In that case, but in that only, ought we to follow at once this procedure—and here we follow it, because, if we left the other extremity loose in the uterus or abdomen, it would be apt to increase the lesion in the walls of the organ, if it happened to get involved in the aperture, or impacted against its edges. In some very rare instances in which, after version by one leg has been effected, and immediate delivery is necessary, the cervix and os internum occasionally contract so forcibly and strongly upon the protruded limb, whenever we drag upon it, as not to allow of a sufficient amount of traction being applied to this extremity without fear of lacerating its structures. In such cases also it may be well to attempt to repass the hand to secure the other extremity, for then by pulling at both extremities together, we incur less chance of injuring them than if we applied the same required amount of force to either of them singly.

SHOULD WE SEIZE ONE EXTREMITY ONLY?

From what I have already stated, you know my opinion, as to this being the proper method of proceeding in almost all cases of difficult turning. The method was long ago spoken of by Portal; and within the present century, Hoffman and Joerg in Germany, and my friend Dr. Radford in this country, have severally written on the subject, and upheld, that in *no* case of turning ought we to lay hold of more than one extremity, for the purpose of effecting the version of the infant. I have just pointed out what I conceive to be two—perhaps the only two—exceptional conditions to this general rule.

Another question meets us. Supposing we intended, in turning, to bring down the pelvic extremity of the ovoid mass of the child, *what part of that extremity* should we lay hold of in order to make it the presenting part at the os uteri?

Some accoucheurs have thought, that if we seized and brought down the *breech* of the infant, it would be the safest mode of turning. This method, however, presents so many disadvantages, that it seems now entirely rejected from practice as a common mode of performing version; for it is difficult to reach up to the breech of the child—and it is difficult to take a sufficiently firm hold of it when you have once reached it—and it is difficult, when once you have taken hold of it, to perform the evolution or version of the child with such a purchase.

We may limit, then, the part which we ought to lay hold of to the lower extremities, and I have already told you that I believe one extremity sufficient. The remaining question, therefore, is this—What part of that extremity should we seize upon?

SHOULD WE TAKE HOLD OF THE FOOT OR THE KNEE?

I believe the seizure of the knee to be preferable in most, if not in all cases, to the seizure of the foot, or rather, as it should be more correctly stated, to the seizure of the ankle of the child. I speak, you will recollect, of turning in cases of shoulder or arm presentation, in which the liquor amnii has been for some time evacuated, as in Anderson's case, and the uterus, by its tonic contraction, has clasped itself around the body and head of the child. Under such circumstances, it is an object of importance not to be obliged to introduce our hand farther than is absolutely necessary, into the cavity of the uterus, because the contraction of the organ, in many cases, opposes its introduction, and the forced introduction of it is apt to produce laceration. It is an object also of equal moment to attempt to turn by a part which produces as little change as possible in the figure and form of the infant; because, if we thrust any of the angulated parts of the child against the interior of the contracted uterus, we would also thus be still more liable to produce rupture of that organ. Now, holding these points in view, it appears to me, that the turning of the child by seizure of the knee presents several decided advantages over turning of the child by seizure of the foot. For—

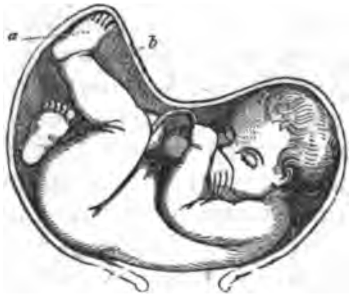
First. The knee is more easily reached. As we slip our hand along the anterior surface of the protruding arm, and along the anterior surface of the thorax of the child, we always, if the attitude of the child has not been altered by improper attempts at version, or very irregular uterine action, find the knees near the region of the umbilicus of the infant—the lower extremities, as you are aware, being folded up in utero so that the knees are brought up to that part, and the legs flexed upon the thighs in such a manner that the heels and feet lie nearly in apposition with the breech of the child. To seize a foot, therefore, we would require to pass our hand about three inches, or, in fact, the whole length of the leg, *further* than we required to do in order to seize a knee.

Secondly. The knee affords the hand of the operator a much better hold than the foot. By inserting one or two fingers into the ham or the flexure of the knee, we have a kind of hooked hold which is not liable to betray us. Every one, on the other hand, who has turned by the foot or feet, knows how very apt the fingers are to slip during the required traction, and how much in this way the difficulties of the operation are sometimes increased.

Thirdly. We produce, I believe, the necessary version of the body of the child more easily by our purchase upon the knee—because thus we act more directly on the pelvic extremity of the infant's spine, than when we have hold of a foot.

Fourthly. Turning by the foot appears to me to endanger greatly more the laceration of the uterus than turning by the knee. The reason of this is sufficiently evident. When we turn by the foot we have to flex the leg round upon the thigh, and thus at *one* stage of the operation, and during one part of the flexion of the leg, we are obliged to have the leg bent to a right angle with the thigh, and the foot of the infant thus projected and crushed against the interior of the uterus. You see this when, on the infant before me, I seize hold of the foot, and turn it round from its position at the breech, till I bring it up to the shoulder, the part which we are supposing to present at the os uteri. You can easily thus perceive (as in the woodcut) that when the leg of the child is thus brought round, it must rasp and scratch, if I may so speak, along the interior of the contracted uterus, and endanger to a fearful degree, the laceration of the organ. It is needless to say how much this danger is increased, when after having brought down one foot, we pass again our hand, and attempt to bring down a second foot, as is recommended by some authors, for thus we only double the danger of laceration of the uterus, from the forced and obstructed passage along its interior, of this other extremity.

Fig. 33.



One point remains for our consideration. Granting that it is proper to seize a knee, I think it a matter of the very first moment to know

WHICH KNEE SHOULD BE SEIZED?

On this point you will find no directions in any of our modern obstetric works, British or foreign, so far as I know them; and yet I believe the secret of turning with facility and safety in such a case as Anderson's—with the waters evacuated and the uterus contracted—depends upon the knowledge of which of the two lower extremities of the infant should be seized. If we turn with one of the extremities—and whether the foot or the knee—it should be the foot or knee of the limb on the *opposite side of the body* to that which is presenting. Thus, if the right shoulder or arm presents, we should take hold of the left knee or foot; and if the left arm or shoulder presents, we should take hold of the right knee or foot. I repeat, that I believe this point to be one of the most essential importance: and the reasons for the rule are simple.

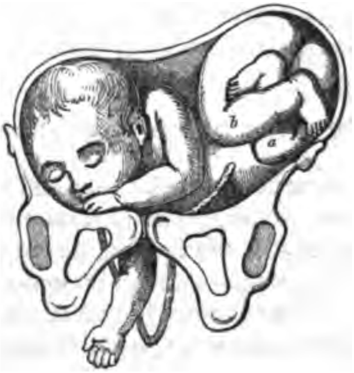
In bringing down the fœtus in the operation of turning, we may, and should, produce two kinds of alteration in its position and figure. Thus, we may bend or flex the body forwards upon the *transverse* axis of the trunk; and we may rotate or turn the body round upon the *longitudinal* axis of the trunk. If we merely flex it, the operation of version will be one of difficulty; if we both flex and rotate the trunk at the same time, the operation will be one of comparative facility. By merely flexing the body upon its transverse axis, we are liable to bring down one of the lower extremities, whilst we do not displace the upper extremity, which is primarily presenting at the os uteri. If we *both* rotate and flex the body—that is, turn it both on its transverse and longitudinal axes—at the same moment, whilst we bring down the pelvic extremity of the child, the turning of the body of the infant carries away from the os uteri the part originally presenting.

Many of you must be acquainted with the fact, that obstetric authors have proposed various methods of removing away from the cervix uteri the presenting arm or shoulder, in order to allow of more space for the part which is brought down, and to produce the necessary evolutions of the child. Thus some recommend the presenting part to be pushed up before we seize the feet; others advise the foot to be seized with one hand, and the presenting part to be pushed up with the other; and others again counsel us to bring down one or both feet, secure them with a tape (as you see in a plate of Moreau), and, whilst pulling with this tape, to introduce the hand, after the lower extremities are brought down, for the purpose of pushing up the presenting portion of the upper extremity.

All these rules and complications are at once avoided by following the principle that I have just stated to you, of bringing down, whenever it is possible, the knee opposite that of the presenting arm or shoulder. When we do this, by carrying the knee diagonally across, if I may so speak, the abdomen of the child to the os uteri, we both, as I have said, *flex and rotate* at the same time the trunk of the infant, and in doing so, the semi-rotation of the trunk inevitably carries up the presenting arm, in proportion as the knee which is laid hold of, is pulled down by the operator. I would add this, as another of the advantages of turning with one extremity, for if we pulled down both knees or both feet, or the foot or knee which was nearest to us, we should produce in many instances mere flexion of the body, *without* that rotation of it which is necessary to carry up and out of the os uteri, the presenting part of the infant.

I have insisted upon the advantages of taking hold of the knee that is highest and furthest from you, and believe this to be a matter of the very first moment. Now, it may appear to some of you that it would be more difficult to seize this knee than the knee of the side corresponding to the presenting arm; but if you reflect for a moment you will see that this difficulty is more imaginary than real. Both knees of the child, as the infant lies folded up in utero, are generally in juxtaposition, and lying upon the abdomen of the infant, near the umbilicus. If, therefore, in passing your hand into the uterus, you insinuate it, as you ought to do, along the anterior surface of the thorax and abdomen of the child, you come in contact with both knees at the same time (see the woodcuts). And the rule which I would give you is this, that instead of hooking your finger or fingers into the flexure of the lower or nearer knee (*a*), you hook them instead into the flexure of the upper, more distant, or opposite one (*b*). Both are so far, in general, equally near, or equally distant,

Fig. 34.



and you seize the one or the other, according as you take care to turn your finger so as to hook it into the flexure of the lower or the flexure of the upper. It has been strongly objected by some authors to the mode of turning by the knee or knees, that we cannot with the hand in utero make a sufficient

DIAGNOSIS BETWEEN THE KNEE AND THE ELBOW.

It is averred that the operator is exceedingly liable to mistake the elbow of the child for the knee, and thus to bring down the opposite arm instead of lower extremity—a result that would certainly render the case only the more complicated. I believe, however, that we might be, and are, equally liable to confound the foot with the hand, and thus lead to the same consequence, while the following simple rule will always enable you to escape the error and danger of mistaking an elbow for a knee: Suppose the child to be lying, as it does, with its extremities doubled up in the uterus, the elbow and knee may be always distinguished from each other by the *opposite directions* in which they point. You have the salient angle, convexity, or rotundity of the knee, always looking upwards towards the upper or cephalic extremity of the child, whereas you have the salient angle, convexity, or rotundity of the elbow, always looking towards the lower or pelvic extremity of the infant. A glance at the sketches will show you this. Hence, if we know the position in which the infant is lying (and this we always do, for other reasons, ascertain previous to commencing our operations), we may, by attending to this single fact, easily distinguish in all cases a knee from an elbow.

There is one point worthy of every attention in regard to the operation we have been considering, which I have not yet alluded to. It is this, that—particularly in cases of difficulty—you require to

USE BOTH YOUR HANDS FOR THE OPERATION OF TURNING.

In making this observation, I mean that whilst we have one hand *internally* in the uterus, we derive the greatest possible aid, in most cases, from manipulating the uterus and infant with the other hand placed *externally* on the surface of the abdomen. Each hand thus assists the other to a degree which it would not be easy to appreciate, except you yourselves were actually performing the operation. It would be extremely difficult, if not impossible, in some cases, to effect the operation with the single introduced hand; and in all cases it greatly aids and facilitates the operation. The external hand fixes the uterus and fœtus during the introduction of the internal one: it holds the fœtus *in situ*, while we attempt to seize the necessary limb or limbs, or it assists in moving those parts, when required, towards the introduced hand; and it often aids us vastly in promoting the version or evolution of the body of the child, after we have seized the part which we search for. Indeed, this power of assisting one hand with the other, in the different steps of the operation of turning, forms the principal reason for introducing, as I have sometimes done, the left as the operating hand into the uterus—for thus we can, by passing the right hand between the thighs, and over the abdominal surface of the uterus (the woman lying on her left side), have more perfect control over the child by our external manipulations with the latter hand. Besides, the left hand, when the patient is in the position I have described, more readily adapts itself to the configuration and axes of the pelvis. If we introduce and turn with the right hand, it is more awkward and difficult to manipulate the abdominal surface of the uterus with the left.

The mode of turning which I have recommended to you was followed in the case of Anderson. When I was first called to see her, all things were in a condition suitable for undertaking the operation. The membranes had burst some hours previously, when the os uteri was still comparatively rigid. It is, no doubt, a matter of moment to be able to turn *before* the waters have escaped, because, as I have already stated to you, the operation is then greatly more simple; but when the membranes do break—as in the case before us—with the os uteri still undilated and undilatable, we must wait with patience, and employ such measures as tend to relax that part. In the present instance, bloodletting was had recourse to for this purpose, and antimony exhibited. A short time before I saw her, Dr. Barry had very properly given her a large dose of opium, in order to moderate the spasmodic or clonic contractions of the uterus, so as to admit of the more easy performance of the version of the child. The os uteri was quite dilatable when I reached the hospital, but the tonic contraction of the organ was so great that I feared there might be some delay and difficulty in the introduction of the hand, and evolution of the child. Such, however, was not the case to the degree anticipated, and the version was accomplished in two or three minutes.

The child was lying with its head towards the right iliac fossa, and its pelvis towards the left. The face, abdomen, and extremities of the infant, were looking forwards. (See Fig. 34.) I therefore, as we always do in such cases, passed up my hand between the anterior or pubic surface of the uterus and the infant, in order to reach more easily the knee which I wished to seize.

As to the place or side where we ought to pass our hand—that is whether between the infant and the pubis, or between the infant and the sacrum—the rule is simple and evident. We pass our hand, as in the case before us, between the pubis and the infant, when we know the extremities of the child are looking anteriorly. We pass our hand between the sacrum and the infant, in those other cases in which we know the limbs of the child to look posteriorly. We do so for two reasons—*first*, because there is always the greatest space for the introduction of the hand on that side of the fœtus to which its face, abdomen, and angulated extremities look; and, *secondly*, it is only, of course, on this side of the fœtus that we can find and secure the lower extremities in order to produce the required evolution and extraction. Dr. Lee lays it down as one of his "two most important rules" in turning, that—however the trunk and extremities of the child may be situated—that is, whether the extremities look anteriorly or posteriorly—you ought "to pass up the hand between the *anterior* and shallow part of the pelvis, and the presenting part of the child." If the back of the infant were turned, as it sometimes is, to this "anterior or shallow part," it would be impossible, by any search in that situation, to get hold of one or other of the legs of the child, which are situated in the very opposite part of the uterus, and on the opposite side of the body of the fœtus; and if the feet were thus really seized, they would be brought over the back of the child, and the spine so far greatly endangered. But Dr. Lee's rule is so very obviously erroneous, and the result of some misconception or disregard of the mechanism of transverse presentation and turning, as not to require any formal refutation. His second important rule, "that when one hand is rendered powerless by the pressure of the uterus, you ought to withdraw this hand and replace it by the other," will doubtless be in many cases, I fear, an unavoidable result of the mismanagement advocated by the first—and is rarely if ever required when you employ the proper hand at once. And every new and unnecessary introduction of the hand must be reprobated, as inflicting a new and additional source of danger upon the patient—and hence, should be avoided as far as possible.

You saw, in this instance, what are the usual preliminary measures adopted in connection with the operation of turning. We kept, as I showed you at the time, the patient placed upon her left side, with the trunk of her body situated across the bed. The right knee was raised by the hand of an assistant, and the uterus kept fixed and steady by pressure on the external surface of the abdomen. In preparing the hand and forearm for the operation of turning, these parts were perfectly uncovered, and everywhere anointed with lard, with the exception of the palmar aspect of the hand and fingers. These latter parts require always to be kept free, in order to secure a proper hold of the presenting part.

After cautiously passing the hand into the uterus along the anterior surface of the protruded arm, I glided it between the anterior surface of the thorax of the child, and the anterior wall of the uterus, laying it flat during one slight pain that supervened, and insinuating it onwards immediately afterwards, till I reached the two knees lying in the umbilical region of the infant. I passed one finger into the flexure of the upper knee, and thus got sufficiently firm hold of that part, without displacing the lower extremities to any extent. Assuring myself, as we always do in the process of version of the child, of the *complete absence* of all clonic contraction of the uterus, I easily turned it round with the purchase I had, and brought down the left lower extremity, the infant making a complete rotation during its evolution, as we shall see immediately from the direction of the toes, abdomen, and face.

We thus, you observe, make the version or evolution of the child during the *absence* of any uterine pain, but, after bringing down the extremity, should any further assistance be required towards the extraction of the child—it must, I beg you especially to remark, be made not during the absence of uterine contractions, but during the *presence* of them. In most cases, indeed, after the version is performed, we allow the uterine contractions themselves to expel the infant, but it was not necessary to wait in such a case as Anderson's, the pains being trifling, and the child being dead. I therefore pulled gently at the protruding leg during each subsequent contraction, and ultimately extracted the head from the pelvis by a purchase obtained by passing two fingers of the right hand over the neck of the child, and introducing two others into its mouth.

You will recollect that, in Dr. Barry's report of the case, as I read it to you, he states an interesting and important fact with regard to the evidence which it afforded of the

NATURAL MECHANISM OF LABOR IN FOOTLING PRESENTATIONS.

In this instance, as occasionally happens in pelvic presentations—both spontaneous and artificial—we found the toes looking towards the pubis, from which we knew that the face and chest of the child must be turned in the same direction—and I stated to the gentlemen present, that this would not interfere with the process of extraction of the infant, for the child, during its further transit, would rotate round with its toes, &c., towards the nearest sacro-iliac synchondrosis, and not require any manipulation on our part to make it come out in that—its proper and most easy, position. This is a subject I would wish particularly to impress upon your minds, for you will find it stated in almost every text-book, that in all pelvic presentations, whether they occur spontaneously, or whether they are artificial, as in cases of turning, it is proper and necessary for us to rotate so far the body of the child, that its toes, and hence its face, should be turned towards the posterior aspect of the pelvis, or, as some more properly advise, towards the nearest sacro-iliac synchondrosis.

We are recommended, for instance, by Dr. Hamilton, to observe, as soon as the child is born as far as the knees, whether the toes point forwards or not—and if they do point forwards, as they did in Anderson's case, we are further directed immediately to turn the child round, so that they may look to the nearest sacro-iliac symphysis.

But no such manipulation is required as a general rule in spontaneous pelvic presentations, and none in artificial pelvic presentations, especially when you bring down only one extremity. In all the cases which I have watched, the infant, as in Anderson's, during the passage of the trunk, rotates and assumes this position itself, without any interference on our part, and upon the principle which I stated to you at lecture this morning, when speaking of similar rotations in head cases, of the mutual physical relation of the child and pelvis, the former turning within the latter, as the helix of a partial screw will partially turn within its nut.

In natural or spontaneous pelvic presentations, this rotation that I allude to, occurs normally and naturally during the passage of the trunk; and that the rotation itself is a mere physical result, and independent of any vital actions on the part either of the child or mother, is proved to demonstration by an experiment which I have several times taken occasion to make—namely, that of dragging by one foot a dead fœtus through the pelvis of a dead mother. If, at the commencement of this experiment, we place the child with its toes, abdomen, and face, looking forwards, we shall find that, as the trunk passes through the pelvic cavity, it regularly rotates round, so that these parts come to present at the nearest sacro-iliac synchondrosis.

Now, what holds good in regard to the mechanism of natural pelvic presentations, holds good also in regard to the mechanism of artificial pelvic presentations, or of those which we make in the process of turning. In artificial, as in natural footling and breech cases, we ought, I believe, to discard in a great measure from our mind any necessity for interfering, so as to turn the infant from one position to another as it passes through the pelvis; for in 9 out of 10, or in

19 out of 20 cases, it will sooner or later assume for itself the easiest and best position for the passage of the head, namely, with the face turned towards the nearest sacro-iliac synchondrosis. This will almost invariably be the case if we do not hurry the extraction of the child by pulling at the protruded extremities, or if we pull merely by one—and that the one *nearest* the pubis. *By dragging, when it is required, at the limb of the child lying next the pubis, and at this limb only*—we pull down the pelvis of the infant obliquely, so far imitating the presentation and easy transit of this part of the body in normal pelvic cases where the anterior ischium is always the lowest; and by the same means we promote the necessary rotation of the child's body, while we should prevent it, on the other hand, by dragging at the sacrum or posterior limb—or by grasping and dragging simultaneously at both.

All obstetric authors seem to me to err, in advising direct interference, for the rotation of the child in artificial pelvic cases to be made by the hands of the practitioner. In speaking of the duties required in such circumstances, even my friend Dr. Rigby (who, in other points, inculcates such sound and excellent doctrines with regard to the mechanism of parturition and non-interference with it on the part of the attendant), observes, "the uterus must be emptied as slowly as possible, and the anterior part of the child *must* be directed more or less backwards." It will, as I have stated to you, be directed more or less backwards by the physical relations between its own trunk and the interior of the maternal pelvis, so as to require no such adaptation to be attempted by any intermeddling on our part. A simple experiment will convince you that this will occur. I have already stated to you, that in repeated experiments made by dragging a dead child by one limb through the pelvis of a dead mother, I have seen the rotation spontaneously take place. But it will even take place, if I perform the same experiment much more rudely, by attempting to drag the leather fœtus before us through the bony cavity of this dry pelvis. If I place, thus, the fœtus with the feet in the pelvis, and the toes directed towards the pubis, or, as is the case in nature, somewhat obliquely to one side of the pubis, say towards the right foramen ovale, and drag the fœtus through by pulling only at the anterior or pubic limb of the child, it turns, you observe, spontaneously, as the trunk passes through the pelvis, so that the face looks at last to one ilium, or even backwards towards the nearest, or right, sacro-iliac synchondrosis; and this extent of rotation will more surely occur if the right arm chance to pass into the unoccupied space of the left sacro-iliac symphysis, or rather if that arm be removed so as not to prevent the rotation of the shoulders and head. The curious and illustrative experiment I have just shown you, was first pointed out to me by my esteemed friend Dr. Ziegler.

SPONTANEOUS EVOLUTION OR EXPULSION OF THE FŒTUS.[2]

DECAPITATION AND EVISCERATION IN TRANSVERSE PRESENTATIONS.

(From Edinburgh Monthly Journal of Medical Science, May, 1847, p. 869.)

CASE I.—Dr. Simpson described a case of spontaneous *pelvic* evolution which he had seen in the second child of a twin case. He was called in about an hour after the first child was born, when the arm and chest of the second child were already protruding through the vagina, and the process of its spontaneous expulsion going rapidly forwards. It was born dead by the unassisted efforts of the uterus, in the course of a few pains, and by the usual mechanism. He further alluded to a case some years ago, attended here by Dr. Cowan, the arm presenting, and the breech at last passing first.

CASE II.[3]—Dr. Simpson subsequently stated another case of its occurrence. The patient was under the charge of a midwife. Mr. Alexander, who was called in, requested Dr. S. to see the case, with the view of turning the child. The mother had already borne four children. The present pregnancy had advanced to between the seventh and eighth month. When born, the child weighed 3 lbs. 9 oz., and measured 17½ inches in length. The labor commenced at 8 A. M., but made very little progress till the afternoon. At 7 P. M., when Dr. S. arrived, the shoulder of the child was pressing deeply into the pelvis. The rectum seemed loaded; and they left the room for a short time, in order that the nurse might empty the bowel with an enema, before turning was practised. On being recalled, they found the pains strong, and the *body* of the child beginning to press down through the pelvic brim—indicating the commencement of the usual process of spontaneous evolution. Dr. S. remarked that this peculiarity and change in the presentation had

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[1] In the section on the Pathology of the Products of Conception, Vol. II. will be found some remarks on peritonitis in the fœtus, which originally formed a portion of this lecture. —(ED.)

[2] Extracted from Proceedings of Edinburgh Obstetric Society.

[3] See Edinburgh Monthly Journal of Med. Science, Feb. 1849, p. 560.

taken place during the ten or fifteen minutes since they had left the patient—the fœtus had made a slight turn upon its long axis, so that it presented much more of the back, and less of the side, than before, the shoulder still, however, maintaining its deep position in the cavity of the pelvis. The child's body came down, in other words, not with the spine or trunk bent and doubled up laterally, but bent and doubled up in a great measure anteriorly. The anatomical structure of the spinal column admitted far more easily of this anterior flexion than of lateral flexion. Was this a common turn or rotation in the mechanism of spontaneous expulsion? If so, did the want of it delay or prevent the process of spontaneous expulsion in some cases. And could it be imitated by art so as to allow the process to go on in appropriate cases? After being forced into this new position, the child was very quickly expelled by the natural efforts, and according to the common mechanism of spontaneous evolution. As usual, it was born dead.

He had also met with two cases of *cephalic* spontaneous evolution or expulsion, the arm presenting, the head and thorax becoming impacted together in the pelvis, and the head ultimately passing out before the lower part of the body. One of these cases occurred in the Maternity Hospital; the other he saw in consultation with Dr. Skae.

General Deductions regarding Spontaneous Evolution.[1]

1st. Spontaneous evolution in transverse presentations was not so rare as some authors averred, and it would probably occur oftener if proper and timely assistance were not rendered.

2d. Under some circumstances, arm and shoulder cases should probably be left to be expelled by the mechanism of spontaneous evolution, assisting, if necessary, this mechanism by art.

3d. This ought to be our practice, if, in an arm or shoulder case, the chest and trunk of the child be *already* thrust down into the cavity of the pelvis; for to turn under such a complication, and with that object, attempt to push back the body of the child from the cavity of the pelvis into the cavity of the contracted uterus, would necessitate the redilatation of the uterus, and hence, in all probability, produce a rupture of its coats.

4th. If the process of spontaneous evolution failed, two operations had been recommended to effect delivery, viz., evisceration and decapitation; and they had always been described as applicable to the same set of cases; but they were individually applicable in two different sets.

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[1] See Edinburgh Monthly Journal of Medical Science, May, 1847, p. 870.

5th. Evisceration was only applicable to cases of *pelvic* spontaneous evolution, demanding operative interference; and *Decapitation* principally applicable to cases of *cephalic* spontaneous evolution.

6th. Of course, in all common transverse presentations seen before the body and bulk of the infant was doubled and thrust down into the cavity of the pelvis, and while it was still in fact in the cavity of the uterus, *turning* was the proper practice, and to wait for the prospect of spontaneous evolution would be utterly wrong; and—

7th. A child of the common size could never, in a transverse presentation, be forced and doubled down into the cavity of the pelvis, unless the mother's pelvis were large in its dimensions; and hence, when the process of spontaneous evolution is found in an *advanced* stage, it is almost a certain sign that the pelvis is of such measurements as to give a chance of its completion.

Of the two preceding operations, evisceration and decapitation, the former is generally extremely tedious, an hour or more being usually necessary—1. To empty the contents of the thorax and abdomen; and, 2. To drag down the breech with the crotchet. Perhaps the division of the bony spine would facilitate the whole matter, and allow the body more easily to double up. Decapitation, on the contrary, is usually rapid in its execution, and far more simple. Dr. Ramsbotham's decapitating hook (an instrument the same as a large polyptome) enables the neck to be surrounded and severed with little or no difficulty in cases threatening cephalic evolution, and perhaps could be applied also, if used early, in most cases threatening pelvic evolution. From the facility with which we have found decapitation to be effected in two or three instances of transverse presentation, where the child was distinctly *dead*, and turning was rendered difficult either by the contraction of the uterus, or by the actual advance of the child downwards into the pelvic brim or cavity, we have a strong conviction that it is a mode of delivery infinitely to be preferred, under such circumstances, to violent and repeated efforts at version.

VAGINAL HYSTEROTOMY, ETC., IN LABORS OBSTRUCTED BY UTERINE AND VAGINAL CANCER.[1]

CARCINOMA in the walls of the cervix uteri and vagina is occasionally found coexisting with pregnancy and parturition. Such deplorable cases have been seen to terminate variously. Sometimes the cervix has been still found so very slightly diseased and indurated, at the time when labor at last supervened, that it has spontaneously relaxed and opened for the transit of the child. Far more frequently its unyielding structures have fissured and torn under the pressure of the presenting part of the infant; the labor, however, thus terminating ultimately without artificial aid, after sufficient space was obtained by the spontaneous lacerations. In some instances the patient has, some days after labor supervened, died undelivered, in consequence either of pure exhaustion, or of laceration of the walls of the body and fundus of the uterus. And in one or two rare cases, the pains of parturition, after coming on regularly at the full term of pregnancy, have after a time ceased—and as in the "missed labors," that so often happen in the cow and sheep—the dead fœtus has been retained in utero for weeks, or even months, beyond the normal period of delivery.[1]

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[1] See Proceedings of Edinburgh Obstetric Society, February 10, 1847, in Monthly Journal of Medical Science for that year, p. 794.

Case I.—In a patient pregnant, and with extensive cancer of the cervix uteri, whom I saw at Hamilton, efforts at labor seemed to come on more than once, when and after the mother herself calculated the term of utero-gestation to be complete. She died at last undelivered, apparently of peritonitis. The fœtus was found decomposing in utero. There was an extensive effusion of lymph on the peritoneal surface of the uterus; but apparently no rupture. She refused to submit to any treatment.[2]

What *treatment* should we pursue in cases of parturition morbidly delayed and obstructed by cancerous disease of the cervix uteri or vagina? It has been long laid down as a principle in British midwifery, that when in labor it is found impossible, from the amount of obstruction which exists, that the lives of both the mother and child can be preserved—the life of the infant should be sacrificed by craniotomy, for the safety of the mother, provided there is space to extract the mutilated child through the maternal passages. In accordance with this principle, I have known the mother, in a case of labor obstructed by carcinoma uteri, delivered by the perforation and breaking down of the head of the infant; and cases of delivery by craniotomy, under the same complication, have been recorded by Denman, Dorrington, and others.

But surely we have a true and important exception to this principle when parturition is rendered difficult or impossible, by cancer of the cervix uteri or vagina. In consequence of the fatal disease under which the mother is suffering, her own life is not worth more than a few weeks', or at most a few months', purchase; while the child, if saved and not sacrificed, may possibly grow up and become a useful and important member of society. Under such circumstances, we are assuredly justified in preserving the life of the child, even were it at the expense of some additional immediate risk to the life of the mother. When, however, in this complication, the unassisted efforts of nature prove insufficient, and operative measures come to be really required, those that are best, are, I believe, of a kind that usually do not add to the danger of the mother, while they are calculated to preserve the child. In most cases the cancerous part at last spontaneously fissures and lacerates in order to allow the child to pass. By practising vaginal hysterotomy in these same instances, we arrive at the same end; but more certainly and safely. For, instead of allowing the muscular contractions of the uterus to make, by long and exhausting efforts, the necessary lacerations, and gain for us the necessary space, we make these lacerations or incisions with the bistoury; and further, when we do so, we can select the safest time for effecting them—namely, early, and before exhaustion sets in; and we can select also the safest locality for the division of the tissues of the cervix, instead of leaving this entirely to chance. After the incisions are made, the expulsion of the child may be left to nature, or it may be extracted artificially by turning or the long forceps. In the two following cases the long forceps were used after the necessary incisions were made.

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[1] In a most interesting case described by Dr. Menzies, in the Glasgow Journal for July, 1843, p. 229, gestation seemed in this way prolonged to the 17th month.

[2] For more details of the case, see a communication by Dr. Miller, in the Edinburgh Monthly Journal of Medical Science for 1844, p. 279.

Case II.—In a woman in the Royal Infirmary, six months pregnant, the septum between the rectum and vagina was already perforated by carcinomatous ulceration. She went on to the full time. As the disease did not extend to the uterus, but affected only the vagina and surrounding textures, the first stage of labor was completed naturally; the child was then extracted by the forceps. But it was necessary, first, to incise freely the carcinomatous mass obstructing the vagina; and, in bringing down the head, the perineum, which was quite indurated and tuberculated, tore in its whole extent. The infant was alive and healthy. The woman had a rapid convalescence, and lived for more than two years afterwards: the carcinomatous ulceration gradually excavating and destroying almost the whole contents of the pelvis.

Case III.—A patient, the subject of extensive cancerous disease of the cervix uteri, was seized in the Infirmary with premature labor near the eighth month. After the parturient efforts had lasted for a considerable time, without any prospect of successful dilatation and delivery, I enlarged the os uteri by lateral incisions, and extracted the infant with the long forceps. The child survived. The mother suffered no special aggravation of her symptoms in consequence of delivery. The cancerous disease proceeded on its usual course, and proved fatal a few months subsequently.

In the following instance the uterine efforts very speedily expelled the child, after the obstruction from the indurated cervix was removed by incision.

Case IV.—I saw this patient with my esteemed friend Dr. Martin Barry, when he was attached to the Maternity Hospital. The woman had been ill for three days. She was very much exhausted, and her pulse extremely rapid. The cervix was indurated by carcinomatous degeneration at one side, and did not seem at all inclined to yield. Two or three small incisions were made through the indurated portion. This allowed the head to pass, and the delivery was completed after five pains. It was too late, however, to save the patient. Her pulse never fell, and she sank in two or three days afterwards.

The cancerous disease at the time of labor supervening may be found not so great or extensive as to prevent the os uteri opening to nearly its full extent, and yet it may prevent the head from entering the brim. In the following case I delivered the patient by turning, instead of the long forceps—the head being detained so very high up, as to suggest the former as preferable to the latter mode of delivery.

Case V.—A patient, under the care of Dr. Burns, who had previously borne a large family easily, had her last labor very protracted, in consequence of carcinomatous induration of the posterior lip of the uterus. Symptoms demanding artificial delivery supervened by the time the os uteri was nearly dilated. The child was extracted by turning, and survived. The diseased cervix tore slightly as the head passed; and perhaps it would have been better to have determined the seat of this laceration by a previous incision. The cancerous disease proceeded slowly onwards, and she died in about a year.

Case VI.—In another patient of Dr. Burns', premature labor came on spontaneously between the seventh and eighth month; and though the whole circle of the cervix uteri seemed affected with the cancerous disease, the os at last dilated and fissured sufficiently to allow a living child to pass. The disease proved fatal to the mother a few months subsequently.

In cancer complicating pregnancy, the preservation of the life of the child is, we have ventured to state, the great object which the practitioner should desire to effect—especially if he can accomplish this object by means not directly detrimental to the mother—and such cases as this last have suggested the propriety of sometimes attempting to attain this double end by the artificial induction of premature labor. This mode of delivery ought probably to be adopted, if the disease is so severe or acute as to threaten to destroy the life of the mother before the full completion of pregnancy; or if we fear that the mechanical obstruction, from the rapid growth and development of the disease, is likely to prove too great for the possible passage or extraction of a child allowed to reach the full term. At the same time, as our calculations are specially directed to the preservation of the child, it would be wrong to peril its life by bringing it with any unnecessary prematurity into the world; and certainly the idea suggested by one or two authorities, of treating this complication by inducing artificial abortion or premature labor, before the infant was visible, seems to us a practice indefensible either on moral or professional grounds.

In almost all the known and recorded instances of cancer uteri complicating parturition, the obstruction to delivery has arisen far more from the *induration*, and consequent non-dilatability of the structures that were the seat of the disease, than from their increased physical *volume* or bulk. And hence the reason why we may hope to overcome the difficulty, in a great majority of cases, by the division, when necessary, of the affected tissues. But where, unfortunately, in the exceptional case, there exists, from the mere size of the carcinomatous deposit, such obstruction of the maternal passages as to prevent delivery entirely, *per vias naturales*, unless the child be destroyed and mutilated, then it does certainly appear justifiable to extract the infant, if it is still alive, by the Cæsarean section. In this complication of labor obstructed by carcinoma uteri, craniotomy, when adopted, besides proving of necessity directly fatal to the child, has almost always resulted also in the very speedy death of the mother. Few or none in the instances recorded have survived above a few hours, or a few days at most. The Cæsarean section offers every possible chance of preservation to the life of the child, and is scarcely more rapidly fatal to the mother. Some years ago, my friend, Dr. Oldham, published a case of large carcinoma uteri obstructing labor, in which the child was saved by this mode of delivery, and the mother did not die in consequence of the operation, as has happened in most instances in which the Cæsarean section has been performed in British practice.

ON THE DANGER OF RUPTURE OF THE UTERUS FROM HYDROCEPHALUS IN THE FŒTUS.[1]

MODES OF DIAGNOSIS AND TREATMENT.

(From Edinburgh Monthly Journal of Medical Science, June, 1848, p. 885.)

Dr. Simpson described two cases to which he had recently been called, and in which fatal rupture of the uterus had previously occurred in consequence of hydrocephalus in the fœtus. Both mothers had borne large families without difficulty. In both cases their present labors were very prolonged, and the pains severe before the laceration occurred. And, it might perhaps be laid down as a general principle, that when a mother who had previously borne children with the usual facility and safety, suffered a long and difficult labor, with the head never properly entering the brim, dangerous enlargement of the head from hydrocephalus or other causes should be suspected, and the most careful means instituted in order to make out a correct diagnosis. In this, as in other important cases, if the patient were put into an anæsthetic state, great facilities would be given for making a careful and leisurely diagnostic examination of the child's head by the hand, without suffering or resistance on the part of the mother. The unusual size and dimensions of the head might be thus ascertained; but one source of fallacy was to be guarded against, namely, that the sutures and fontanelles were not, as was usually described, always preternaturally open and enlarged in hydrocephalic cases; for the cranial bones were in some instances, where the internal effusion was very great, so largely and abnormally developed as to destroy this supposed pathognomonic sign, and to form an almost complete osseous covering for the enlarged head. In one of the two cases described by Dr. Simpson, the cranial bones were, in this way, as large as in a child two or three years old. When hydrocephalus was attended with imperfect ossification, the enlarged and fluid head sometimes moulded itself so readily to the maternal passages as to pass without much difficulty even when of very great size. But the danger and difficulty was much increased when to this was added the impediment arising from enlargement of the bones themselves.

Mechanism of Rupture in Cases of Hydrocephalus.

The danger of rupture of the uterus, and consequent death of the mother, was much greater under hydrocephalus than obstetric authors commonly described. In a collection of cases of intra-uterine hydrocephalus made this year by Dr. Thomas Keith for his thesis, out of 74 instances of the disease which he had found on record, in 16 the uterus ruptured during labor. In fact, the diseased head of the infant, distended by the effused fluid, acted, under the impression conveyed to it by the body and spine of the infant during labor, like a hydraulic bag or machine, pressing equally and in all directions on the cervix uteri, or parts with which it was in contact, with a force under which these compressed structures were almost certain to rend, provided the pressure was of any great duration; because the force itself contained the sum and concentration of the whole power of the uterine contractions bearing on each point with which the bag of the head was in contact. Under such compression, any weak or fissured point, was almost certain to yield. And if any hydrocephalic head or fluid bag of this kind passed into the vagina and remained there, the kind of universal compression of the vaginal walls to which it gave rise, was apt, if it did not lead to direct rending and laceration, to produce, if long continued, sloughing inflammation in these parts.

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[1] Extracted from Proceedings of Edinburgh Obstetric Society, March 12, 1848.

Removal of Hydrocephalic Head by Trocar.

Hence labors, in which the child's head was hydrocephalic, should not, if possible, be allowed to become prolonged; but when the delivery was not effected by the natural efforts, it was not perhaps necessary to destroy entirely such an infant during labor, by using, as was universally recommended, the crotchet or other mortal operative procedure. In some instances it was a matter of legal importance for the parents that the child should be born alive, though not capable of surviving. And a small trocar passed into the hydrocephalic collection was capable of evacuating the contained fluid as efficiently as a more extensive and fatal laceration by the usual obstetric perforator or scissors; nor was such an evacuation by the trocar necessarily fatal to the fœtus, for we know that in the operation for hydrocephalus on the child after birth, by Dr. Conquest and others, the simple puncture of the head was by no means a fatal operation. Those operated on seldom or never died of the operation itself, but of the subsequent reaccumulations and consequences.

Evacuation of Fluid by the Spinal Canal when the presentation is Pelvic.

The hydrocephalic fœtus not unfrequently presented by the feet or breech. In a case of this kind which occurred some time ago to Mr. Girdwood of Falkirk, and where the child was dead and putrid, the spine and skin over it lacerated at the neck, in making the usual tractions to bring down the head. A great quantity of water escaped from the laceration, and the enlarged head, which had previously resisted the force applied to it, was now easily extracted in its collapsed state. This case afforded a suggestion, that under a similar complication, with the body already born, instead of passing our hand and perforator to the brim of the pelvis in order to puncture the head, which was sometimes a matter of no small difficulty, we might attain the same end, the evacuation of the hydrocephalic effusion, by the more simple and safe measure of opening the vertebral canal, in any part of the course of the spine.

CASE OF DELIVERY WITHOUT OPERATIVE AID, THROUGH A PELVIS EXTREMELY NARROWED BY MALACOSTEON;

WITH PRACTICAL REMARKS.[1]

(From Edinburgh Monthly Journal of Medical Science, July, 1847, p. 22.)

DIFFERENT classifications of the morbid conditions and deformities of the maternal pelvis have been proposed by obstetric pathologists. Some have arranged them in relation to their causes, others in relation to their nature, seats, &c. But the most *practical* classification consists in dividing the morbid states of this part of the skeleton into several grades, according to the actual degree and amount of physical contraction in the pelvic apertures, which is induced by these states; and consequently, according to the kind and modification of measures required to extract through the defective apertures, a child arrived at or near the full term of utero-gestation.

Drs. Denman and Ramsbotham, and Professor Paul Dubois, have, for instance, followed this principle of classification in discussing the effects of diseased and deformed conditions of the pelvis upon the process of parturition. Laying aside all pelves of the normal form and standard dimensions, as requiring no accessory aid from art for the transit of an infant through them, these authors have divided all varieties of pelvic contractions into the three following gradations.

I. Pelves somewhat diminished below the natural standard, but still admitting of the child being expelled through them by nature, or extracted by instruments safe (such as the forceps and vectis) both to the mother and infant.

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[1] Read before Edinburgh Medico-Chirurgical Society, June 2, 1847.

II. Pelves contracted to such a degree as not to allow a child, at or near the full term, to pass through their apertures till its head was reduced in size by craniotomy.

III. Pelves so very small as not to admit of the extraction through them of a child even after it was mangled and mutilated by embryulcio, and where that last resort of the obstetric art, Cæsarean section, constituted the only possible mode of delivery.

It is evident that the reference of individual cases of labor to one or other of the preceding degrees of difficulty in the process of delivery, must be often regulated by other circumstances than the mere physical dimensions of the maternal pelvis. The strength and powers of endurance of the patient, the activity of the uterus, and above all the dimensions and compressibility of the infant's head, as modified by its actual volume, and by its state of ossification, the width of its sutures and fontanelles, the separation or not of the bones by previous death and putrefaction, &c., are so many matters forming important points and elements of difference in different instances. But still, however, most of our highest authorities in midwifery have attempted to lay down, in the form of general principles, more or less exact and fixed pelvic admeasurements between the several classes or gradations of pelvic deformity that I have mentioned. In other words, they have endeavored to reduce to abstract arithmetical formulæ, as it were, the boundaries and limits between morbid states of pelvic contraction, admitting respectively of delivery at or near the full time, *first*, without the necessity of embryulcio; *secondly*, by means of embryulcio; and *thirdly*, where embryulcio is inadequate and insufficient for the purpose.

For the purpose of illustrating these points and statements, I shall collect and throw into a condensed tabular form the opinions of some of our most eminent British accoucheurs on the pelvic dimensions relatively fitted for these different modes of management and delivery. It is necessary, however, to premise one observation in order to understand the figures and formulæ which I shall quote. The apertures of the brim and outlet of the human pelvis are normally of an irregularly ovoid form, and they maintain more or less of this configuration under all kinds of morbid contraction and deformity. Indeed it generally happens, more particularly in cases of rickets and malacosteon, that when either of these pelvic apertures is morbidly contracted in one diameter, it comes to be elongated in the other. One of the diameters, either of the brim or outlet, however, is always contracted; and hence in speaking of the relative size of different deformed pelves, accoucheurs often note their dimensions by simply stating, as in the first of the following tables, the length of their shortest or narrowest diameter.

Smallest size of pelvis through which a child may pass without mutilation.—In the standard and normally formed pelvis, the narrowest diameter of the brim (the conjugate), and the narrowest diameter of the outlet (the transverse), are each, on an average, about four inches in length. A child may, however, pass through these pelvic openings, though their narrowest diameter be reduced very considerably. The following table shows the opinion of some of the most eminent British accoucheurs on this point.

Smallest Pelvic Diameter admitting of the Passage of a Child without Embryulcio.

3¼ inches—	Joseph Clarke,[1] Burns.[2]
3 "	Denman,[3] Davis,[4] Ramsbotham.[5]
2¾ "	Osborne,[6] Hamilton,[7] Barlow.[8]

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[1] "Having examined, by dissection, the bodies of many women who died after tedious and laborious labors, I am enabled to state, with some confidence, that three inches and a quarter from pubis to sacrum is the least diameter through which I have known a full-grown fœtus to pass *entire*; but it was a very putrid fœtus, consequently the head was soft and pliable.' —Dr. Clarke in Transactions of the Irish College of Physicians, vol. i. p. 374.

[2] Dr. Burns considers Dr. Clarke "correct when he says that the head cannot pass entire if the diameter be under 3¼, and even this will generally require the perforator."—Principles of Midwifery, 1843, p. 471.—See also p. 473.

[3] "Should the capacity of the pelvis be reduced under three inches, we have no good reason to expect a living child of its full growth, however small, to pass through it, either naturally or by the assistance of art; though the head of one that is dead, especially if it be putrified, or one much below the common size, may be pressed through a pelvis of these dimensions, even without artificial assistance. Should the capacity of a pelvis not exceed, according to our judgment, two inches and a half, then the head of a child, unless the contents be evacuated, cannot pass or be extracted through it."—Denman's Introduction to Midwifery, pp. 457–58. Edition of 1816.

[4] "Pelves of somewhat less dimensions than those of a standard pelvis may occasionally admit of children being born alive at the full period of gestation. For example, a living child of average size at that period might be born alive, provided the conjugate diameter of the brim of the mother's pelvis was three inches and three-quarters, and the head presented in the best possible position. But, if it amounted to no more than three inches, a well-grown child at full period could not be expected to pass without an operation to reduce the bulk of its head."—Davis' Principles and Practice of Midwifery, vol. i. p. 25.

[5] "Some practitioners have thought that a pelvis measuring only two inches and three-quarters in the conjugate diameter would allow of the head passing whole, provided there was sufficient room laterally. My own conviction, derived from clinical observations, is, that the dimensions I have just mentioned (three inches in the conjugate by four in the lateral diameter) are the smallest which will grant the passage to a full-grown fœtus."—Ramsbotham's Obstetric Medicine and Surgery, second edition, page 30.

[6] "Whenever the capacity of the pelvis is only two inches and three-quarters or certainly less than three inches," there is, Dr. Osborne conceives, "an utter impossibility for a child of ordinary size, at full time, being born alive by any means, either of nature or art, through so small a pelvis."—Osborne's Essays on the Practice of Midwifery, p. 223. At p. 194, he states that, "when the bones approach much nearer than three inches it is utterly impossible for a living child at full maturity, by any means to pass," and embryulcio should be performed early in the labor.

[7] Dr. A. Hamilton, in his Letters to Dr. Osborne, adduces several cases in which delivery, took place safely and spontaneously, "where the deformity of the pelvis seemed to be such

I am not aware that any English writer on midwifery has stated it as practicable that a child could pass, without mutilation, through a pelvic diameter less than that mentioned in the last line of the preceding table; namely, two inches and three-fourths. Instances, however, in which children, at or near the full time, have been expelled by the uterus, or extracted with forceps, through pelves measuring only two inches and a half in their narrowest diameter, have been mentioned by some foreign authors. Solayres, Baudelocque, and Capuron,[1] each advert to cases illustrating the possibility of this occurrence.

In an obliquely deformed pelvis in my Museum, the conjugate diameter of the brim is one or two lines below 3 inches; and yet through it a dead full-sized child passed as a head presentation, after a long labor, but without instruments. I extracted a second infant by the feet through the same pelvis. Further, I believe, that when the child presents by the feet, and thus the apex, instead of the base of the cone formed by the head and body of the fœtus, comes first, that it may make its transit *without* embryulcio through a pelvis, the smallness of which would otherwise have necessitated mutilation or the operation of craniotomy. In this way I have through a pelvis probably not above 2½ inches in its narrowest diameter, extracted a child by the operation of turning, the parietes of the skull becoming compressed and indented to allow of its passage.[2]

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that acording to your (Dr. O.'s) data, the head of the child should have been opened at the beginning of labor," p. 119. In the first case detailed by Dr. Hamilton, a living child passed, though the pelvic brim was ascertained by admeasurement after death, to be "sensibly under three inches."—P. 101.

[3] Dr. Barlow, in his Essays on Surgery and Midwifery, p. 354, gives the following Synoptical Table of the various degrees of distortion of the pelvis and their appropriate treatment:—

Degrees of Deformed Pelvis.	Conjugate Diameter of Brim.	Modes of Delivery.
First degree, . . .	From 4 to 3 or 2¾ inches,	Efforts of Nature, or assisted by forceps or lever.
Second degree, . .	From 2¾ to 2½ inches, . . .	Premature delivery.
Third degree, . . .	From 2½ to 1½ inches, . . .	Embryulcio.
Fourth degree, .	From 1½ to the lowest possible degree of distortion,	Cæsarean operation.

[1] Baudelocque's Midwifery, Heath's Translation, vol. ii. p. 370. Capuron's Cours des Accouchemens, Brussels Edit. p. 221.

[2] See p. 450. I have elsewhere stated the advantages, in deformed pelves, from turning in comparison with embryulcio to be, that—1. It gives the child a chance of life; 2. It is more safe to the mother, because it can be performed earlier in the labor, and more speedily; 3. It enables us to adjust and extract the head of the child through the imperfect pelvic brim in the most advantageous form and direction, the head flattening *laterally* under the traction; 4. The neck of the child, if it be living or only lately dead, is so strong as to allow us to exert such a degree of traction upon the obstructed head, that the sides of the cranium may become very greatly compressed, or even indented under it, and that without necessarily de-

Highest pelvic dimensions necessitating the performance of the Cæsarean section.—It is well known that Continental practitioners have sometimes recourse to the Cæsarean section under degrees of pelvic contraction that are not considered by British accoucheurs to demand an operation of such great severity and hazard.

In one of the last and best works upon Midwifery, published in Germany, the following observations occur in reference to the degree of pelvic deformity, indicating the Cæsarean operation:—

"When the smallest diameter amounts to only 2¾ inches, the termination of labor is possible only by making an artificial passage, or by breaking up the child. The possibility of terminating it in the latter manner ceases whenever the small diameter amounts only to 2¼ inches or less, and the Cæsarean section is then the only possible mode of delivery, and that to which we must have recourse in all cases, whether the child be dead or not. If the contracted pelvis measures from 2¼ to under 3 inches, then the Cæsarean section is indicated when the child is alive, while, if it is dead, perforation is to be had recourse to."[1]

Jacquemier, the latest French writer on Midwifery, states, that when the pelvis is below two inches in its narrowest diameter, the Cæsarean section is the only justifiable mode of delivery, even when the child is dead; and when it varies from two inches to two inches and a half, and the child is alive, the Cæsarean operation should be adopted in preference to embryotomy, not only for the sake of the life of the child—but as, perhaps, not more dangerous to the mother than a protracted and difficult delivery by embryulcio, generally proves to be.[2]

So far as I am aware, Velpeau is the only author who has hitherto attempted to collect, in a generalized form, the measurements of the pelvis of patients who have been submitted to the Cæsarean section. He tells us that out of 80 cases in which the cause for the operation was specified, in 62 cases it was required by contraction of the pelvis, particularly in the antero-posterior diameter of the brim. Thus it was[3]

1	inch	. . . .	in 1	case.
1	"	to 1½ inches	in 8	cases.
1½	"	to 2 "	in 23	"
2	"	to 2½ "	in 25	"
2½	"	to 2¾ "	in 5	"

In a number of these cases, the Cæsarean operation was adopted

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stroying the child; and, 5. It is a practice which can be followed when proper instruments are not at hand, and the avoidance of instruments is generally desirable when it is possible.

[1] Busch and Moser's Handbuch der Geburtskunde, Berlin, 1842, Bd. iii. p. 108.

[2] Manuel des Accouchemens, Paris, 1846, tom. ii. p. 162.

[3] Traité Complet de l'Art des Accouchemens, tom. ii. p. 458.

under degrees of pelvic contraction, in which delivery by means of embryulcio, would have been followed by British practitioners. This remark especially applies to the thirty cases included in the last two lines of the table; and it applies to them the more strongly when we further recollect that the sizes of these pelves in English measurements would have been somewhat more than they appear under French measurements, the French inch being about $\frac{1}{12}$ longer than the English inch.

In choosing, in any case of contracted pelvis, between the alternatives of craniotomy and the Cæsarean section, Continental practitioners generally look upon the life of the child, as well as the probable degree of difficulty and danger likely to ensue to the mother from a painful and protracted delivery by embryulcio, as important points and elements in deciding between these two methods of delivery. In this country, little, or indeed no, attention has hitherto been given to these considerations in forming a practical conclusion on the question. In fact, British accoucheurs have never deemed themselves entitled to have recourse to the Cæsarean section, unless the pelvic apertures were so much reduced as to prohibit the practicability of the extraction of the child through them by embryulcio. With them the *propriety* of delivery by the Cæsarean section begins exactly with that degree of pelvic deformity at which the *possibility* of delivery by embryulcio terminates.

Hence, in order to fix and determine the highest limit of pelvic contraction which necessitates the performance of the Cæsarean section, we have merely, in the first instance, to fix and determine the lowest limit of pelvic contraction at which delivery by embryulcio is capable of being effected.

The following table presents, in a condensed form, the opinions of various British and American obstetricians upon this question, namely, the actual degree of pelvic contraction *above* which it is considered still possible to deliver by embryulcio, and *below* which it is deemed proper, and absolutely necessary, to extract the infant by the Cæsarean section.

Smallest Pelvic Diameters admitting of the passage of a Child by Embryulcio.

3½ inches by 2 inches—Dewees,[1] Bedford,[2] &c.

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[1] "By a sufficient diameter I mean, where there is at least two inches in the antero-posterior, and at least three and a half in the transverse; below this, delivery *per vias naturales*, I repeat, I believe to be impossible."—Dewees' System of Midwifery, 1837, p. 579.

[2] "I do not believe it is possible to remove a child by embryotomy, when the antero-posterior diameter of the superior strait measures less than two inches, without subjecting the mother to severe hazard, provided the child be of the ordinary size. I am satisfied, that even with the space of 2¼ inches, all the dexterity which the operator can bring to his aid,

3 inches by 1¾ inches—Burns,[1] Hull,[2] &c.
3 " by 1½ " Barlow,[3] Hamilton,[4] &c.

During the last ten or twenty years, various improvements have been proposed in our embryulcic instruments, particularly with the view of rendering that operation more safe and easy in cases of unusual difficulty and deformity. Lest, with such additions and improvements, the preceding table be supposed to misrepresent the existing rules and doctrines of our modern British schools of Midwifery respecting the degree of pelvic contraction necessitating the adoption of the Cæsarean section, I shall cite, in reference to the indications for this operation, the opinions expressed upon the subject in the last three obstetric text-books that have issued respectively from Edinburgh, Dublin, and London.

In his "Introduction to the Study and Practice of Midwifery," Edinburgh, 1843, Dr. Campbell observes, "unless we have a clear space of two inches, or nearly so, in the transverse (conjugate), and fully three in the lateral diameter of the brim, embryotomy must be abandoned, as not likely to insure the safety of the parent.[5]

In his work "On the Theory and Practice of Midwifery," 1843, Dr. Churchill, of Dublin, after an elaborate investigation of the subject, draws the following deduction. "We may therefore," he observes, "safely conclude, that when from any cause, the antero-posterior diameter of the upper outlet, or the transverse diameter of the lower, is not more than 1½ inches, there is no possibility of delivery *per vias naturales*, but that we must have recourse to the Cæsarean section."[6]

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will be required to protect the mother from serious, if not fatal injury. I, without hesitation, would prefer the Cæsarean section, if I had certain evidence that the child lived, to any attempt to extract it, *per vias naturales*, if the antero-posterior diameter measured less than 2½ inches."—Dr. Bedford in his Translation of Chailly's Midwifery, note, p. 386.

[1] "The crotchet cannot be used, when the head is of the full size, unless we have a passage through the pelvis and its linings measuring fully an inch and three-quarters in the short diameter, and three inches in length; or if the child be premature and soft, an inch and a half broad, and two inches and three-quarters long."—Dr. Burns' Principles of Midwifery, 1843, p. 508.

[2] "I am of opinion, that it still remains to be proved whether a mature fœtus of the ordinary size has ever been extracted, with safety to the mother, through a pelvis, in the superior aperture of which there was not in any point, from the fore to the hind part, a space equal to 1¾ inch."—Dr. Hull's Defence of the Cæsarean Section, p. 391.

[3] See Dr. Barlow's Synoptical Table, quoted on page 584, from his Essays on Surgery and Midwifery, p. 354.

[4] "Whenever the short diameter of the pelvis, either at the brim or at the outlet, will not admit above one ordinary sized finger, or really falls under one inch and a half, no other means are justifiable for the delivery, if the child be arrived at the full period of gestation, than that tremendous expedient, the Cæsarean operation."—Dr. A. Hamilton's Letter to Dr. Osborne, p. 139

[5] Introduction, &c., p. 319.

[6] Midwifery, p. 314.

Dr. Francis Ramsbotham of London, in his "Obstetric Medicine and Surgery," London, 1844, observes, when speaking of deformity of the pelvis,[1] "I am quite convinced, that unless there be at the brim one inch and three-eighths in the conjugate, by three and a half in the iliac (diameter), or 1½ inches in the conjugate by three in the iliac, it would be useless to attempt delivery *per vias naturales.*" And again, when treating of difficult labors, he remarks, "If upon a measurement conducted with the utmost care, we find there is less space at the brim than three inches and a half laterally by one inch and three-eighths in the conjugate diameter; or three inches by one inch and a half; we ought to consider it our duty, however painful and appalling that may be, at once to propose the Cæsarean section as the only means by which it is possible to save the mother's life; and as offering also the sole chance of safety to the child."[2]

Our highest, as well as latest, authorities in British Midwifery seem thus to have fixed upon a degree of pelvic contraction in which the dimensions varied from 3½ to 3 inches in the long diameter, and from 2 to 1½ in the short diameter, as the lowest limit at which delivery by embryulcio can be performed,[3] and below which it is always and invariably necessary to have recourse to the Cæsarean section, when the child has reached the full time. In the case, which it is my object in this communication to detail, the inferior pelvic aperture of the patient was so deformed and contracted from Malacosteon, that it was considerably less in its dimensions than the lowest limit stated in the preceding table and extracts, as capable of permitting delivery by embryulcio, and yet the mother after arriving at, if not past, the full time of utero-gestation, was delivered, not only without embryulcio, and without the Cæsarean section, but in fact without instrumental interference of any kind. Further, the child after its birth, was passed through an unyielding aperture measuring under one inch in its short, and two and a half inches in its long diameter; or, in other words, this aperture was fully half an inch in all its dimensions *below* the limits supposed to demand the necessity of the Cæsarean section. Before showing the solution of such a seeming obstetric enigma, I shall briefly state the anterior history of the patient.[1]

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[1] Loc. cit. p. 30.

[2] Obstetric Medicine and Surgery, p. 179.

[3] I do not stop to discuss the question, whether, in all cases of great deformity, embryulcio is always *proper* where it is *possible.* The difficulty attendant upon its performance in instances of marked contraction has sometimes been extreme. In a case where the narrowest diameter of the brim was thought to measure *two inches*, Dr. Meigs required many hours to break down the cranium, and afterwards three or four hours' pulling to extract the child with the crotchet. The mother was altogether three or four days in labor.—Philadelphia Practice of Midwifery, p. 322. In a woman with a pelvis, the conjugate diameter of which "was estimated at *little more than an inch and a half*," Dr. Hamilton effected delivery by embryulcio. The extraction alone occupied four hours, and "required such an exertion of force, that he was literally obliged from exhaustion to be carried home in a sedan chair."—Practical Observations, p. 263. Lately, my friend Professor Murphy extracted a child by embryulcio through a pelvis, the brim of which measured *one and a half inches* in its conjugate, and four and a half in its lateral diameter. The operation required about seven hours' work on the part of Dr. M. The patient was in labor from Tuesday to Friday.—Lancet for April 3, 1847. "I question much," observes Dr. Burns, "if extreme cases of embryulcio be not as dangerous to the patient as the Cæsarean operation; certainly they are more painful."—Midwifery, p. 501. And he elsewhere adds (p. 509), "I shall not be surprised, if in a few years, British practitioners come to resort more frequently, especially in extreme cases, to the Cæsarean section."

DETAILS OF THE CASE OF MALACOSTEON, AND INFERENCES FROM IT.

Mrs. D——, Cupar in Fife, now 34 years of age, was in early life regarded as a robust and large child. She became a dressmaker in Edinburgh at fourteen years of age. The sedentary habits of this profession, betimes rendered her catamenia very irregular, and brought on so much general delicacy of health that she was advised to forego the occupation. She went to Cupar, and lived there with a relation. In 1837, when about 24 years of age, she married. Two years subsequently to that event she began to complain of pains in the back and sides, and stiffness about the knee-joints. From this attack she never recovered so fully as to be able to walk without support, and continued to suffer much with shifting pains. In 1840 she fell, while walking with a staff across her room, and after this the pains in the limbs were for a considerable time far more severe, and the lameness greatly increased. Since recovering she has been able to walk out of doors with the assistance of crutches. But various bones of the trunk and extremities have become shortened and deformed under the effects of the malacosteon. The spine is bent backwards and outwards in the form of a bow, with the ribs and sternum correspondingly displaced. Some of the phalanges of the fingers are bent; the right thigh bone is curved forward into a semicircular shape. And from being a handsome and somewhat tall woman, she has shrunk down, during the course of the last seven or eight years, into a deformed dwarf-like figure, measuring about four feet in height.

From the time of her marriage up to last June (1846), Mrs. D. had never become pregnant. Some time, however, during the course of June—but she is not certain at what precise period of the month—the catamenia appeared for the last time. She was delivered on the 28th of April, or ten months after the last menstruation.

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[1] For these particulars I am much indebted to the kindness of Mr. Wiseman of Cupar, the patient's medical attendant.

For some time after pregnancy commenced, Mrs. D. feared that the swelling and increased size of the abdomen were the result of dropsy, and did not watch her own feelings so as to be aware of the period of quickening. At last, however, the motion of the child, &c., became so unequivocal, that she applied to Mr. Wiseman to attend her in her approaching confinement.

On examining into the condition of the pelvis, Mr. Wiseman at once found that its outlet was extremely contracted, but the pregnancy was already so far advanced as to preclude, under such a degree of deformity, the idea of delivery by the induction of abortion or premature labor. Besides, all attempts to touch the os uteri proved ineffectual, so that none of the ordinary means of arresting pregnancy, and exciting uterine action, could have been put into practice.

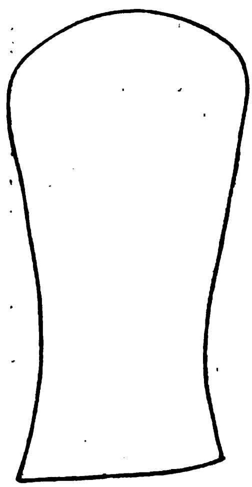
Early in March I visited Mrs. D. with Mr. Wiseman, Dr. Graham, and Dr. Grace. She was quite incapable of moving or turning in bed without assistance. The uterine tumor was high, and pressed over to the right side. On applying my stethoscope to it, I readily detected the usual rapid pulsations of the fœtal heart. I found fully borne out, the correct description which had previously been given me of the excessive deformity and contraction of the pelvis. The sacrum was straight above, so that its promontory did not probably encroach on the brim; but its inferior extremity was strongly and anormally curved forwards. In front, the walls of the pelvis felt doubled or collapsed together; and the outlet, which was the only part that could be very accurately examined, was exceedingly deformed and diminished in size. The transverse diameter was particularly contracted. I found it impossible to introduce two fingers between the tuberosities of the ischia. Hence this diameter of the outlet was evidently under an inch. Posteriorly, or opposite the sacro-sciatic ligaments, there was transversely more space, but the strong anterior curvature of the coccyx and lower end of the sacrum seemed to curtail the conjugate diameter of the opening, and to prevent the probability of its admitting, when fully dilated, more than three, or, at most, four fingers even in this direction.

Under these circumstances, with a living child advanced to the eighth month, and a pelvic outlet so extremely contracted, I had no hesitation in coming to the same conclusion as the patient's medical advisers had all previously done, namely, that the Cæsarean section was the only practicable mode of delivery. I was aware that Barlow, Conradi, Sprengel, and others, had seen and published cases of mollities ossium, in which the softened and still flexible pelvic bones had bent and yielded during labor so as to permit the passage of the child without operative interference; but in Mrs. D. the osseous tissue appeared far too firm to permit us to indulge even in this faint hope. Nature, however, provided, and was perhaps already preparing, for the mother a mode of delivery that was still more safe and easy.

Having agreed to operate in case the Cæsarean section was required, I anxiously waited in the daily expectation of being called to Mrs. D. At last, on the morning of the 28th of April, I received from Mr. Wiseman a note, dated the previous night, intimating that our patient had begun to complain of labor pains—that the os uteri could be felt projected low down into the vagina—that it was not opened more than two or three lines—and that the presenting part of the child could not yet be detected. With some professional friends who had agreed to accompany me, I forthwith proceeded to Cupar, a distance of about thirty miles. On arriving there, we were surprised to hear that the patient was delivered, and our surprise was only increased by learning that no kind of instrumental aid had been required. A visit, however, to the room in which the child was, readily solved the apparent riddle.

The infant had been dead for some time in utero. It looked nearly the natural length, and as it lay extended on the table, it measured 18½ inches from the crown of the head to the heels. But its limbs and body were thin, lank, and atrophied; and its weight was only 3 pounds 2 ounces. Its head appeared very large and disproportionate in size, and indeed had been considered hydrocephalic. This seemingly increased volume, however, was not the consequence of effusion, but the result of putrefaction. The encephalon was in a dissolved and semi-fluid state; and as the head lay on the table, it was extended and flattened out laterally and superiorly, as if it consisted of a bag or bladder, half filled with liquid or semi-liquid contents; and such in fact it really was. For all the bones of the arch of the cranium were separated from their attachments, and floated about in the dissolved and liquefied cerebral matter. The bones of the basis of the skull were also loosened, and more or less separated from each other—a rarer occurrence. On handling the vertex, the first piece of bone which I touched was the orbital portion of one of the ossa frontis. The symphysis even of the inferior, as well as of the superior, maxillary bones, were loosened, and admitted of free motion. And the component parts of the head and face were so easily displaced and compressed that, on placing one of my fingers in the lower occipital region behind, and another on the nose or cheek in front, I found that, without any considerable degree of exertion, the two could be made to approximate to within a few lines of each other. Yet the skin of the scalp and face was continuous and entire, the epidermis only being separated at different parts. The chest and abdomen of the child seemed quite soft and pliable, though not in so disintegrated a state as the head.

Fig. 35.



Thus far the macerated and diffluent state of the fœtus appeared to afford an easy explanation of the possibility of its transit through the very contracted pelvic outlet of the mother. But I was anxious to have more full and complete proof that the fœtus, even in this state, was capable of passing through an aperture, of dimensions so small as we knew the pelvis to present in the case of Mrs. D. In order to obtain this proof, I got oblong openings of two or three different sizes cut in plates of white iron. The smallest of these perforations, however, though only 3¼ inches long by ⅞ broad, proved unnecessarily large for the experiment. We diminished it by filling it up at one end with strong, thick and perfectly unyielding sole leather, so that the whole opening measured only 2⅜ inches in its largest, by ⅞ of an inch in its shortest diameter; and yet through this aperture, of which the accompanying woodcut gives the exact outline,[1] the child was pulled without any great degree of force or difficulty. In dragging the infant through this aperture, no particular resistance was met with from the bones of the head and face; but the size of the liver impeded its transit for a minute or two, as the lower part of the thorax was passing through the metallic opening. Dr. Graham, Dr. Ziegler, Dr. Weir, Mr. Wiseman, &c., witnessed these experiments with me; and, if our limited time had allowed us to remain longer in Cupar, and to have got another plate perforated, probably we should have found the child capable of passing through an aperture one or two lines smaller in some of its dimensions.

The placenta had been preserved. It was small and atrophic, and contained scattered through it a number of those white tubercles, as they are sometimes improperly termed, which we so often see connected with, and causing marasmus and death of the fœtus in utero. Some of these tubercles or fibrinous deposits were of the size of hazel-nuts, or larger.

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[1] The perforated iron plate and superadded piece of leather, are preserved in the Obstetric Museum of the University.

The history of the delivery had been this. Slight labor-pains had come on during the afternoon of the 27th. She was seen in the course of the evening by Drs. Graham and Grace and Mr. Wiseman, who found the os uteri beginning to dilate, but the pains were not severe, and the husband and attendants of Mrs. D. all went to bed. About one o'clock in the morning of the 28th, Mr. Wiseman was raised, the waters having escaped about an hour previously, and the uterine contractions having become strong, and bearing down. When Mr. Wiseman reached the house of the patient, he found the soft scalp of the child already bulging through the external parts. Some detached bones, included in the portion of scalp that had passed, allowed him to obtain a firm hold of the protruded portion of the head, and thus enabled him to use some extractive force. By thus assisting the effects of the pains, the child was entirely born about half an hour after Mr. Wiseman's arrival. The mother has made a very good recovery, and declares that "having a child is nothing."

There are no sufficient data to determine at what precise time the infant died. Milk had been discharging from the nipples for three or four weeks before delivery; but whether the child had perished about that period, it is impossible to say. Mrs. D. deceived herself with the idea that she felt it living and moving up to the time of delivery.

The preceding case is, I believe, unique in the annals of midwifery. It will perhaps, therefore, require no apology if I add one or two brief inferences which the history and details of it appear to suggest.

1. It has taught me, and is, I conceive, calculated to teach others, a strong lesson of caution in regard to our *prognosis*, under apparently even the most desperate circumstances. In such rare forms of complication as this, we must depend for our prognosis, &c., upon the anterior observations and recorded facts of others. Here all such records led me to expect a very different result, and offered no hope whatever of such a fortunate termination as actually occurred.

2. The case affords a new and striking illustration of the just and well-known remark of Dr. Denman, that "the resources of nature in everything which relates to parturition, are infinite, and constantly exerted for the preservation of both the parent and child; yet, when the two objects are incompatible, the life of the child is almost uniformly yielded to that of the parent."[1] And the mode

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[1] Practice of Midwifery, p. 415.

and mechanism by which nature brought about this unexpected result in the present instance are highly worthy of special notice. For, *first* she set up a diseased condition of the placenta, which prevented the full and proper nourishment of the fœtus, and thus restrained as far as possible its development and growth. *Secondly*, she carried this state of marasmus to such a degree, as at last proved slowly fatal to the child, without superinducing that expulsive action which generally soon follows the death of the infant. *Thirdly*, the dead infant was subsequently retained for so long a time in utero, that not only the bones of the cranium, but the bones of the basis of the skull and face were loosened and separated from each other, and the head and other parts of the body thus rendered readily and easily compressible.[1] And, *lastly*, the emaciated, dead, and highly putrefied infant, after being thus reduced to this diffluent and compressible mass, and now capable of being moulded to the contracted apertures of the pelvis, was ultimately and without difficulty expelled through them by the supervention of natural uterine contractions. Each stage and step in this mechanism was necessary for the success of that which followed it, and the imperfection or omission of any one of them would probably have entirely subverted and prevented the very fortunate and very unlooked for result that occurred from the combination of the whole.

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[1] No individual case in Midwifery has given rise to so much discussion, as that of Elizabeth Sherwood. Her pelvis was estimated by Dr. Osborne as below the lowest standard which I have given in a preceding page, as capable of allowing of the passage of a child by embryulcio, being only about ¾ of an inch from the sacrum to the pubis; and yet he delivered her successfully by craniotomy after working and pulling three hours with the crotchet. Many authors have stated that Dr. Osborne must undoubtedly have under-measured the pelvis of Sherwood;—and Drs. Hull, Burns, Hamilton, &c., have denounced the operation of embryulcio as "impracticable" if Dr. Osborne's measurements were at all true and accurate. Dr. Campbell, for instance, observes, "It would be idle to enter largely on the refutation of this extraordinary case, since Dr. Osborne's narrative of what he thought he had accomplished is irreconcilable with common sense; for how could the base of the cranium which is 1½ inches in thickness, and nearly three in breadth, be brought through the aperture which he describes. A fair estimate," Dr. Campbell continues, "of the utter impossibility of effecting it may be afforded by the simple experiment of forming in a plate of hard wood, an opening, in shape and size exactly corresponding to the pelvis of Sherwood, and attempting to force through it the base simply, divested of the other portions of the skull."—Midwifery, pp. 317 and 318. In the case of Mrs. D., I obtained the corroborative evidence afforded by the very experiment which Dr. Campbell here properly suggests; and I have already stated the facility with which the child was passed through the perforated plate. In Sherwood's case there was, I believe, the same reason for the practicability of delivery, for the fœtus seems to have been in the same putrid and decomposed state as Mrs. D.'s child, and perhaps the bones of the face and basis of the cranium were in a similar way loosened and compressible. "The whole body" of the fœtus was, to quote Dr. Osborne's own words, "in the most putrid and almost dissolved state." See p. 101 of his Essays.—But, besides, the brim of the pelvis in Sherwood was in reality not so small as the measurement of its conjugate diameter would seem to indicate. During delivery the os uteri was pulled by Dr. Osborne over to the right side, or to the *space* intervening between the line of the conjugate diameter and the right ilium. *Here* there was an oblong aperture 3 inches long (as measured from the ilium to the symphysis pubis), and 1¾ inches broad: and hence in fact an aperture as great as Drs. Hamilton, Burns, Churchill, Ramsbotham, &c., deem necessary for the peformance of embryulcio; and greater than that through which we pulled Mrs. D.'s child.—See drawing of the brim of Sherwood's pelvis in Dr. Hull's Defence of the Cæsarean Section, pl. v. fig. 1.

3. Does the mode in which the delivery was effected in this instance by nature, suggest any measures of practice which, under similar complications, we could induce and imitate by art? I put this question, because, in the greater deformities of the pelvis, all the standard operations and means which we employ for delivery are in fact imitations of processes and operations which nature herself employs under the same conditions. When the pelvis has been much contracted, abortion has occasionally come on in the earlier months and saved the mother; or premature labor has supervened about the seventh month, and saved both the parent and child. These natural processes we imitate successfully in the artificial induction of abortion and premature labor. If, in morbid contraction and deformity of the pelvis, the pregnancy goes on to the full time, nature is still sometimes capable of delivering the mother by other and various measures. Occasionally, during labor, the symphysis pubis has been rent asunder under the intense and wedge-like pressure of the infant's head; or the uterus has been lacerated, or, as has happened now in two recorded cases, both the uterus and abdominal parietes have simultaneously ruptured and allowed the escape of the child through this double opening; or the bones of the child's cranium have become deeply compressed and fractured, so as at last to allow the reduced head to pass; or the same has been effected by the infant dying, putrefying, and at last its scalp and sutures bursting, so as to produce the necessary diminution in the size and dimensions of the encephalon. These several operations of nature are all imitated by art in the respective operations of symphyseotomy, the Cæsarean section, cephalotripsy, and craniotomy. And while art thus adopts the operative principles of nature, she attempts to improve both upon their facility and safety, by selecting an earlier and hence less dangerous period for their performance; and by making the required openings and lesions by cutting instruments, instead of submitting to the chance of their being made by nature by means simply of an enormous and hazardous expenditure of muscular effort and compression on her part.

But, I repeat, does the mechanism of the delivery in Mrs. D.'s case suggest any principles for imitation? Let us consider the answer, as it might be varied by the *date* of the pregnancy of the mother; and according as she had reached the periods, *first*, of artificial abortion; or, *secondly*, of premature labor; or, *thirdly*, had already advanced to the full time.

The case shows that through an opening of very small dimensions a child may pass, provided it be in a very compressible state. So far it evidently suggests that the induction of *abortion* at the fourth or fifth month, when the head of the fœtus is still small, soft, and very easily reducible, would, as long ago proposed by Cooper, &c., succeed in such extreme deformities in saving the mother from many of the dangers accompanying delivery at a later period of utero-gestation. I have stated above that Mrs. D. applied to Mr. Wiseman at a time when it was already considered too late to have recourse either to artificial abortion or premature labor; and further the high position of the os would probably have rendered either of them impracticable.

The induction of *premature labor* at or about the seventh month would not, of course, have sufficed with a pelvis of such small dimensions, unless we could modify the operation so as both to produce the death of the child and *retain* it in utero, for the purpose of allowing its structures to become dissolved and disintegrated before labor at last supervened. Now, we have no known means of inducing that diseased state of the placenta which produced the attendant emaciation and death, in the case of Mrs. D.'s infant; nor am I acquainted with any measures which would destroy the life of the child in the later months, without superinducing labor. The retention, however, in utero of the infant and its maceration or putrefaction, would be as necessary for success as its death, under such great degrees of contraction. And the result shows, that when the pelvis is much deformed, and labor with a *dead* child is threatened, or the infant is destroyed by craniotomy, the longer we can retain it in utero without danger to the mother, and the more it thus becomes putrefied, and disintegrated, the easier will its ultimate expulsion or extraction prove.

Lastly, suppose a patient with a very diminished and deformed pelvis to have arrived at the *full time* of utero-gestation, does Mrs. D.'s case suggest any new principles or modifications of treatment for the delivery of the mother? I believe that, under these circumstances, our conduct and practice should be, in a great degree, regulated by the state of the child. If it be *alive*, as ascertained by auscultation, &c., and the pelvis is as small as in Mrs. D.'s case, or even half an inch larger in its measurements, then I am decidedly of opinion that it is our duty to perform the Cæsarean section. We have two human lives committed to our charge, and it is our duty to try to preserve both, provided we can attain that object without subjecting the mother to a degree of danger, much greater than she would otherwise undergo. Let us take, however, the other alternative, and suppose the child already *dead*. With this complication most British accoucheurs would attempt delivery by craniotomy, if the dimensions of the pelvis permitted at all of the possibility of it. And the case of Mrs. D. seems to me to suggest one means of rendering it thus possible, under states of contraction, where it is at present properly regarded as in the highest degree unsafe or totally impracticable. The grand obstacle to the delivery of the child by embryulcio, in greatly contracted pelvis, arises from our want of means to reduce the size, or alter the shape and compressibility, of the bones of the base of the skull and face. In Mrs. D.'s child, nature had disintegrated and separated these bones, had removed in fact this obstacle, and thus rendered the delivery not only possible but easy. Could we imitate or induce this same fortunate result by artificial means? All our present means of reducing the size of the fœtal head in embryulcio, are limited to the destruction of the arch of the skull. I am not aware that with any proposed form of osteotomist[1] we are capable of cutting or disintegrating the base of the cranium or face, when the pelvis is diminished to 1¼ or 1½ inches in its shortest diameter. Yet, probably, some modification of mechanical means would give us the power of effecting this desirable object. The common perforator might enable us to loosen and break up the bones of the base in some cases, in the same way as with it we break up the arch, of the skull. The disjunction or fracture of these bones, without their removal, might prove sufficient to permit the required degree of compressibility and alteration of shape. Or the common bone-forceps of the surgeon, or a modification of such powerful pliers as are used in dividing the needles in the operation for harelip, &c., might answer. At all events, the object seems anything but a hopeless one, more especially when we call to recollection that modern surgeons are now provided with mechanical means, which sometimes enable them to seize, break up, and extract from the cavity of the bladder, large and solid stones—and that too, through a canal relatively so small and elongated as the male urethra.[2]

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[1] The Kephalepsalis of Dr. Campbell appears to me to be an instrument preferable to, and more powerful than, any of the forms of osteotomist invented by Dr. Davis or others. Yet, as we have already seen, Dr. Campbell believes embryulcio to be impracticable, even with its assistance, in a pelvis less than 3 by 2 inches. It is almost unnecessary to add, that the French operation of Cephalotripsy is founded on the idea of crushing and compressing the bones of the base, as well as of the circle, of the cranium. It is used by some of the leading accoucheurs of Paris, instead of craniotomy, particularly in cases where the child is dead, and the pelvis below the dimensions that would admit the use of the forceps. But the instrument with which the operation is performed—the Cephalotribe—is of such enormous dimensions, its blade being of solid iron, 1¼ inches wide, the whole above 2 feet long and several pounds in weight, that it could not of course be applied in cases where the pelvis was contracted to any extreme degree.

[2] For some additional remarks on this subject, see Lancet, for Oct. 1847, p. 381, and March 1, 1851, p. 284.—(Ed.)

The subsequent history of the patient, whose case is described in the preceding pages, was as follows:[1]

Mrs. D. remained in much the same state of health for several years after her delivery. The deformity of the skeleton did not seem to advance in any degree during that period, and was considered by herself and friends to be quite arrested in its progress. In 1853 she again became pregnant. But she did not inform her medical attendant, Mr. Wiseman, of her state, nor consult him in any way in regard to it; and he was not called to see her till she had been a considerable time in labor, under the charge of a midwife. Parturition supervened at the supposed full term of gestation. She was not aware of the exact date of the last catamenia; but she had felt the movements of the child for five months before labor commenced. Mrs. Adam, the midwife, was called to Mrs. D.'s assistance about 10 or 11 o'clock on Wednesday, the 21st of June, 1854. Mr. Wiseman was first sent for next morning, about four o'clock. The patient continued in labor during that and the two subsequent days. On Friday morning a summons was sent for me to Edinburgh; but in consequence of some mal-arrangement at the telegraph office, it was not delivered; and a second message by railway arrived that evening. Accompanied by my friends Drs. Malcolm and Storer, I reached Cupar early next morning. The patient was by this time very weak and exhausted, and, indeed, almost moribund; but the child's heart could be still heard beating, though feebly. I performed the Cæsarean section, more with the hope of still saving the child, than of being of any possible use to the mother. No special difficulty occurred in the operation, except from an artery in the divided walls of the uterus bleeding profusely by a per-saltum stream, till it was arrested after a time by torsion. The patient died about two hours subsequently. The child, the care of which was entrusted to Dr. Graham, breathed only once or twice after birth; but its heart continued to pulsate slightly for ten or fifteen minutes. It seemed to die from cerebral effusion.

A post-mortem examination was permitted. Mr. Wiseman has kindly furnished me with the following notes of it:

"The entire length of the body was found to be 4 feet 4 inches. The left femur was curved forward, rendering that limb shorter by an inch and a half than the right limb. The measurement from the superior anterior spinous process to the heel, being in the one case 31 inches, in the other 32½. The ribs projected much forward, from the point of the insertion of the clavicles, which bones were greatly twisted, and the sides of the chest were thrown down upon the bones of the pelvis. This arched line, from the upper end of the sternum to the scrobiculus cordis, measured 6 inches. From the junction of the clavicles to the superior anterior spinous process of the ilium, 10½ inches.

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[1] A short notice of the termination of this case was read to the Medico-Chirurgical Society on the 15th of November 1854.—(Ed.)

"Posteriorly, the line of the spine from the end of the os coccygis to the nape of the neck, measured 20 inches. The spine projected to a considerable extent at the upper part of the back, and again to a smaller extent in the sacral region.

"We found the length of the incision made in the operation to be 6 inches; and, the stitches being removed, about 4 ounces of coagulated blood were found in the abdomen.

"The distance between the two superior and anterior spinous processes of the ilia was nearly 10 inches, or more precisely, 9⅝. The brim of the pelvis was triangular, or cordate, in form; the transverse and antero-posterior diameters measuring each nearly 3¾ inches at their widest points. The capacity of the brim, however, was not in proportion to the above measurement, from the great projection forward of the sacrum and pubis, and the collapse inwards of the sides of the ilia. The outlet of the pelvis was far more contracted. From the symphysis pubis to the coccyx, it measured 3 inches, but the transverse diameter, or distance between the tuberosities of the ischia, was only half an inch. The depth of the pelvis at the symphysis pubis was two inches."

At the Medico-Chirurgical Society's Meeting, June 5, 1850, Dr. Simpson mentioned four cases, two of them collected by Dr. Hull, in which the Cæsarean section had been performed accidentally upon healthy women by the goring of cows. All these four mothers recovered; and he considered that the successful result of such cases, and under such rude surgery, showed that the soundness of the constitution of the patient formed the most important element in the result.—See Edinburgh Monthly Journal of Medical Science, December 1854.—(Ed.)

ON THE

SPONTANEOUS EXPULSION AND ARTIFICIAL EXTRACTION OF THE PLACENTA BEFORE THE CHILD,

IN PLACENTAL PRESENTATIONS.[1]

(From Lond. and Edin. Monthly Journal of Med. Science, March 1845, p. 169.)

I.

SECTION I.—DANGERS OF PLACENTAL PRESENTATIONS: OPINIONS OF AUTHORS: STATISTICAL EVIDENCE OF THE FATALITY OF THESE PRESENTATIONS.

ALL obstetric authors seem to agree on this point, that there is no one complication in midwifery attended with more anxiety to the practitioner, and few, if any, with more real danger to the patient, than cases of unavoidable hemorrhage from presentation of the placenta.[1]

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[1] The substance of the following memoir was, on the 4th of December 1844, laid before

"Placental presentations," says Dr. F. Ramsbotham, "are always fraught with extreme peril."[2] "The attachment of the placenta," observes Dr. Collins,[3] "to the mouth of the womb, is one of the most dangerous complications to be met with in midwifery." "There are few dangers," to quote the words of Dr. Edward Rigby,[4] "connected with the practice of midwifery, which are more deservedly dreaded, and which are wont to come more unexpectedly, both to the patient as well as to the practitioner, than that species of hemorrhage which occurs in cases where the placenta is implanted, either centrally or partially, over the os uteri." "It is," says Dr. Dewees,[5] "confessed on all hands, that no accident attendant on conception, is equally menacing, as unavoidable hemorrhage; and it also emphatically declares to the physician, that much depends on him, that it shall not be very often fatal. It is one," he adds, "of those extraordinary cases, in which nature does less for the preservation of the individual than in almost any other." "That form of hemorrhage," remarks Madame Lachapelle,[6] "which depends upon the implantation of the placenta upon the internal orifice of the uterus, is one of the most dangerous accidents to which pregnant women are exposed." "It is perhaps," long ago observed Deleurye,[7] "of all labors, that in which the mother and the child run the greatest danger." Still earlier, Lamotte states,[8] that "amongst all the accidents of child-birth, there is not any one more perilous (*il n'y en a point un plus perileux*) than that in which the after-birth presents before the child."

The actual results of practice fully bear out the observations which I have selected from the preceding authors upon the danger of unavoidable hemorrhage from placental presentation. My friend

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the Medico-Chirurgical Society of Edinburgh.—See Edinburgh Monthly Journal, February 1845, pp. 158–161. Since that time a number of additional cases have been incorporated with the essay, and the deductions altered in a corresponding degree.

[1] "During the last months of gestation, and at the commencement of labor," observes Dr. Churchill, "patients are exposed to *two* forms of hemorrhage, differing in their causes, but depending upon the *situation* of the placenta. The first has been called 'accidental hemorrhage,' because it arises from a partial and accidental separation of the placenta, which occupies its *usual* situation; and the second is justly termed 'unavoidable hemorrhage,' because the placenta being placed partially or wholly over the os uteri, the dilatation of this will *unavoidably* separate the after-birth, and give rise to hemorrhage."—Theory and Practice of Midwifery, p. 383. Our investigations in the present memoir refer to the last of these two forms of uterine hemorrhage.

[2] Obstetric Medicine and Surgery, 2d edition, p. 391.

[3] Practical Observations on Midwifery, p. 90.

[4] System of Midwifery, p. 248.

[5] Midwifery, p. 390.

[6] Pratique des Accouchemens, tom. iv. p. 362.

[7] Traité Complet des Accouchemens, p. 404.

[8] Traité des Accouchemens, p. 36.

Dr. Churchill, in his late excellent work upon the *Theory and Practice of Midwifery*, has collected from different sources the records of 174 cases of this complication. Amongst these 174 cases, 48 proved fatal to the mothers; or nearly one in every three of them died. I have attempted to make a still more extensive analysis of recorded cases of placenta prævia, or placental presentation, and the consequences of this complication as bearing on the life of the mother. The following table[1] shows the results of the inquiry:—

Table of Maternal Mortality in Placental Presentations.

Reporters.	No. of Cases.	Mothers Lost.
Mauriceau,[2]	18	3
Portal,[3]	14	1
Giffard,[4]	19	5
Smellie,[5]	18	7
Rigby,[6]	42	10
Clarke[7] and Collins,[8] . .	16	4
Busch,[9]	13	2
Schweighauser,[10] . . .	65	16
Lachapelle,[11]	21	10
J. Ramsbotham,[12] . . .	129	41
F. Ramsbotham,[13] . . .	189	49
Lever,[14]	34	8
Lee,[15]	46	14
Wilson,[16]	30	10
Total, ...	654	180

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[1] We print this table as published by Dr. S. in the Lancet for September 1847, p. 276, in a corrected form, and with additional data furnished to him by Dr. Ramsbotham, Dr. Wilson, &c. See also Lancet for 1847, p. 517, in reference to the correctness of the data.—(Ed.)

[2] Observations sur la Grossesse et les Accouchemens, vol. ii. pp. 8, 48, &c. &c.; and Edinburgh Medical and Surgical Journal, vol. li. pp. 383–84.

[3] La Pratique des Accouchemens, 1785, Observ. ii-xxix., &c. &c.

[4] Cases in Midwifery, pp. 22, 36, 38, 52, 87, &c. &c.

[5] Collection of Cases, &c., vol. ii. pp. 307-315; and vol. iii. pp. 141-178. I have, with Dr. Churchill and others, here and in a subsequent table, marked all those cases of Dr. Smellie's as recoveries, where an opposite result is not directly stated. The context seems to warrant this.

[6] An Essay on Uterine Hemorrhage, 6th Edition, p. 262.

[7] Transactions of King and Queen's College of Physicians, vol. i. p. 380.

[8] Practical Observations in Midwifery, p. 96. The returns of Drs. Clarke and Collins are classed together, as both coming from the Dublin Lying-in Hospital.

[9] Forbes' British and Foreign Review, vol. v. p. 587.

[10] La Pratique des Accouchemens, p. 224.

[11] Pratique des Accouchemens, tom. ii. pp. 415–461.

[12] Practical Observations in Midwifery, part ii. pp. 195–233, and MSS. Notes.

[13] Principles of Obstetric Medicine and Surgery, 2d Edition, pp. 395–96. Dr. F. Ramsbotham kindly drew out for Dr. S., in 1847, a list of all the cases of placenta prævia attended by his father and himself, with their results, as given in the above table.—(Ed.)

[14] Guy's Hospital Reports, vol. vi. p 66.

[15] Lectures on the Theory and Practice of Midwifery, p. 371.

[16] From MS. notes of Dr. Wilson, formerly Lecturer on Midwifery, and deservedly one of the most highly esteemed and distinguished obstetric practitioners in Glasgow. Many of the fatal cases were instances which Dr. W. saw in consultation.

From the preceding Table, it thus appears that out of 654 cases of placental presentations which are collected into it, the result was fatal to the mother in 180 instances, or, in other words, 1 *in every* 3$\frac{6}{10}$ *of the mothers perished in connection with this complication.*

The dangers of placental presentations to the mother may appear stronger to some minds if I state them in other terms. Two of the most fatal epidemics of modern times, are yellow fever and Indian or malignant cholera. In the well-known yellow fever of Gibraltar of 1828, the mortality among those attacked was nearly 1 in 4½.[1] In 1832-33, about one in 3⅓ of those affected in England with the epidemic cholera, died.[2] Hence those mothers who are the subjects of placental presentations, are submitted to as great peril of life from this obstetric complication, as they would be if seized with yellow fever or malignant cholera. Further, the operation of lithotomy is generally regarded as one of the most formidable in surgery, and is calculated to be fatal in the proportion of 1 in every 6 or 8 subjected to it.[3] The occurrence of placenta prævia is twice as dangerous and fatal as the operation of lithotomy—1 in every 3 perishing under the first, and 1 in every 6 or 8 perishing under the last.

Looking at these results, it will, we believe, be readily conceded, that any attempt—such as is the professed object of the present memoir—to diminish this fearful maternal mortality in placenta prævia, is entitled at least to the consideration of the obstetric profession, even should it fail to be so fortunate as to secure their concurrence and conviction.

Section II.—Recognized Principles of Treatment:—1. Evacuation of the Liquor Amnii; and, 2. Delivery by Turning. Proposal of a Third Principle: The Complete Separation of the Placenta; Grounds for Proposing it: Illustrative Cases.

Hitherto, two great principles of treatment—if we leave out the minor details of management—may be said to have been pursued by obstetric practitioners in the treatment of placental presentations. And the two modes of practice I allude to are supposed by many to be applicable to two different stages or degrees of the complication. They consist of the two following measures:—

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[1] Out of 5383 persons attacked, 1183 died.—See Researches on the Yellow Fever of Gibraltar by Dr. Louis of Paris, Boston, 1839, p. 259.

[2] Dr. Merriman has calculated, from official returns, that 49,594 individuals were affected with epidemic cholera in England, and that 14,807 of them died, giving the proportion in the text. In Scotland and Ireland the mortality was greater. See Medico-Chirurgical Transactions, vol. xxvii. p. 416.

[3] "The average mortality from lithotomy, on all hands, appears at present to be about 1 in 8."—Dr. Willis' Urinary Diseases, 1838, p. 347. Mr. Inman has calculated the mortality from lithotomy to be 1 in every 7¾ cases, 765 patients having died out of 5900 operations which he had collected.—See Lancet for October 5, 1844.

1. *The Evacuation of the Liquor Amnii.*

In some cases of placenta prævia, and under some circumstances, the artificial evacuation of the liquor amnii is recommended to be had recourse to, and thus the same treatment is followed for "unavoidable" hemorrhage, as is followed by most practitioners in instances of "accidental" hemorrhage. This mode of practice has been especially applied of late to cases in which the presentation of the placenta was only *partial*, and where, consequently, a portion of the membranes was within reach, and to instances in which the hemorrhage was comparatively slight in its degree and effects. About a century and a half ago, the same treatment seems to have been employed also by some practitioners, in instances in which the placenta presented completely over the os uteri, the placental structure being perforated artificially by the finger or an instrument, in order to permit the liquor amnii to escape. After recommending that, in placenta prævia, the membranes should be pierced, or the fingers thrust through the placenta, "that at last it be perforated, and instead of the constant flux of blood which appeared before, the humors will presently flow out," Daventer,[1] writing about the year 1700, adds, "some penetrate the secundines with a *hair needle*,[2] which I do not approve of if it can be done with the fingers, because the infant is easily hurt." Under some conditions in placenta prævia, Deleurye[3] recommends the piercing of the placenta with a trocar, in order to allow the liquor amnii to be drained off. Baudelocque[4] speaks of the same practice as probably useful in instances of complete or central presentation of the placenta, when the cervix will not allow of turning; and in later years the same plan has been again proposed by the elder Dr. Ramsbotham,[5] and successfully put in practice by Gendrin[6] of Paris.

2. *The Delivery of the Child by Turning.*

In the generality of cases of unavoidable hemorrhage from placental presentation, the practice which is adopted consists in forcing the delivery, by passing the hand through the os uteri up to the feet of the infant, and extracting the child by the operation of podalic turning. This last mode of practice is the one universally followed when the hemorrhage is very severe, and whether the artificial evacuation of the liquor amnii has preceded it or not, and it is the plan of treatment usually pursued where the presentation of the placenta over the os uteri is central or entire. By some accoucheurs indeed, as Drs. Burns and Hamilton, Baudelocque, Capuron, and others, the forcible delivery of the woman by the operation of turning is the *only* mode of treatment that is thought advisable under any circumstances in connection with *placenta prævia.* It is, according to Plenck, "nullo remedio sed solâ extractione fœtûs curanda."[1] "All the best practical writers are," says Dr. Merriman, "unanimous on this point, that the case of placenta adhering over the cervix uteri is not to be trusted to nature. In all cases of attachment of the placenta over the os uteri, it is incumbent upon the accoucheur to make up his mind to the operation of turning the child, and bringing it into the world by the feet."[2] "This is a case," Dr. Conquest remarks,[3] "in which we ought never to confide in the powers of nature, because expulsatory uterine efforts only augment the peril of the patient; and therefore the hand must be either bored through the substance, or, what is better, passed by the edge of the placenta, and the child turned." "It is completely established," to quote the words of Dr. Dewees,[4] "that the only chance the woman has for life, is by a well-timed and well-conducted delivery, in every case of placental presentation." "When hemorrhage," says Dr. Denman,[5] "from this cause (placental presentation) comes on, though all women without proper assistance would not die, none are free from danger till they are delivered. As there is a very doubtful chance of the delivery by the pains of labor, and as experience has fully proved the frequent insufficiency of all other methods intended to suppress the hemorrhage, and how little reliance ought to be placed on them, though they are always to be tried; it is a practice established by high and multiplied authority, and sanctioned by success, to deliver women by art, in all cases of dangerous hemorrhage, without confiding in the resources of the constitution. This practice is no longer a matter of partial opinion, on the propriety of which we may think ourselves *at liberty* to debate; it has for near two centuries met the consent and approbation of every practitioner of judgment and reputation in this and many other countries. See Mauriceau and almost every succeeding writer."

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[1] The Art of Midwifery Improved, London, 1715, pp. 153–54.

[2] "Placentam vel secundinam *acu crinali* perfodiunt," to quote the original Latin. See p. 138 of the second edition of Daventer's Novum Lumen, &c., Leyden, 1733. The first edition was published in 1701.

[3] Traité des Accouchemens, Paris, 1777, p. 369.

[4] System of Midwifery, translated by Heath, vol. ii. p. 38.

[5] Practical Observations, part ii. p. 189.

[6] Traité Philos. de Médecine Pratique, tom ii. p. 548.

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[1] Elementa Artis Obstetricæ, 1781, p. 133.

[2] Synopsis, &c. of Difficult Parturition, 1826, pp. 126, 127.

[3] Outlines of Midwifery, p. 157.

[4] System of Midwifery, p. 394.

[5] Introduction to Midwifery, p. 527.

Cases of Placenta Prævia not unfrequently occur in practice in which neither of the two preceding plans can be successfully adopted —where the artificial evacuation of the liquor amnii is insufficient to moderate the hemorrhage to a safe degree—and where forced delivery by turning is inapplicable or extremely dangerous if adopted. In these and other cases, I would beg to submit to my obstetric brethren an additional principle of treatment, viz.:

3. *The Complete Separation, and, if necessary, Extraction of the Placenta before the Child.*

I shall first state the grounds on which I venture to found the propriety of this proposed addition to the treatment of the very anxious and very dangerous cases of which we speak.

Obstetric pathologists seem unanimous in the opinion that all the more formidable varieties of hemorrhage which occur from the uterus in the latter months of utero-gestation, or the earlier periods of labor, are attributable to the separation of the vascular connections between the placenta and the interior of the uterus, and the escape of blood from the vessels which are laid open in consequence of this separation.

Paradoxical as it may appear, there are sufficient grounds and facts for believing, that when the placenta is separated slightly and partially, the chance of fatal hemorrhage to the mother is greater than when the disunion of the organ is entire and complete. Various authors have detailed cases in which the death of the mother speedily took place, though the portion of the placenta separated from the uterus was exceedingly small. Thus Dr. Hamilton mentions that in several cases which had fallen under his observation, and where he was called too late to afford proper assistance, it was discovered that the fatal hemorrhage had proceeded from the separation of "a very small portion of the placenta." In one instance of fatal hemorrhage between the 7th and 8th month of utero-gestation, he found on dissection that "the area of the separated placenta was less than a square inch."[1]

On the other hand, I believe I have collected a sufficient number of data to prove that, when the disjunction of the placenta from the uterus is *perfect* and *complete*, the degree of maternal hemorrhage that occurs is in general exceedingly slight and trifling, or it is altogether arrested. The details of a few cases may illustrate and impress the fact which I wish to point out.

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[1] Practical Observations, 2d edition, p. 314.

CASE I.—*Placenta expelled upwards of three hours before the child; no hemorrhage in the interval; child removed by decapitation and extraction.*—In 1840 I was requested by my friend Dr. Graham Weir to see a patient at about the 5th month of pregnancy, who had been attacked with very severe hemorrhage. It was her third or fourth pregnancy. After the flooding had continued for some time, the placenta was expelled. *From the time of its expulsion the hemorrhage ceased.* The shoulder and neck of the infant were presenting over the os uteri. The os uteri was so contracted and the whole organ so small, as to prevent the possibility of the introduction of the hand for the operation of turning. At my suggestion, Dr. Weir severed the neck of the infant. Its body was then easily extracted by pulling at the presenting arm; and its head was immediately afterwards expelled by the unassisted action of the uterus. From three to four hours elapsed between the protrusion of the placenta and the complete delivery of the woman, yet during that time she lost little or no blood, and her recovery was speedy and perfect.[1]

CASE II.—*Placenta expelled about two hours before the child; elbow of the child presenting.*—In 1841, I was requested by Dr. Lewins of Leith to visit a case of complicated unavoidable hemorrhage. I saw the patient shortly after 9 o'clock in the morning. Labor pains had come on about 4, and a considerable degree of hemorrhage had accompanied them. Shortly after 7 o'clock, Dr. Lewins, on visiting the patient, found the placenta expelled through the os uteri. When I saw the woman, nearly two hours afterwards, the placental mass was lying between her thighs, and attached to her by the umbilical cord. She was weak from the hemorrhage that had occurred previous to the expulsion of the placenta, but from the time that organ had been extruded, *the flooding had almost entirely ceased.* I found the elbow of the child presenting; and as the os uteri was well dilated, it was easy to bring down a lower extremity, and terminate the labor. The patient recovered without a bad symptom.

CASE III.—*Placenta expelled some minutes before the child; no intervening hemorrhage; child expelled by natural pains, and revived.*—For the details of this case I am indebted to Dr. Dewar of Dunfermline, and shall give the circumstances in his own graphic words. "Some blood," he says, "had been lost as nearly as we could calculate at what would have been the seventh and eighth menstrual periods, and several times between the eighth and ninth months, and that in spite of an entire cessation from all exercise. Labor took place at the full time, and, as was dreaded, was accompanied with severe hemorrhage from the beginning. When I saw her, about an hour after pain had begun, the orifice of the uterus was pretty well dilated, and a soft spongy mass, apparently the centre of the placenta, protruded from it. There was no time for interference, for almost instantly a strong pain forcibly expelled the whole of the placenta from the vagina. *To my surprise the flooding ceased.* Pains continued active, and the child was born in less than ten minutes. After a little time the infant revived, and the mother recovered well, though considerably exhausted."[1]

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[1] See Subsequent General Table, No. 17.

[2] Table, No. 21.

CASE IV.—*Great hemorrhage, and expulsion of the placenta under strong uterine action; child extracted by turning some hours afterwards.*—Mrs. H., during her second pregnancy, her first child having been premature, had a slight flooding about the seventh month. When in the eighth month, labor commenced early on the morning of the 18th of May, with slight pains, and sanguineous discharge. These continued more or less severely till the evening of the 19th, when, as Mrs. H. was resting upon her knees and elbows, an immense gush took place, along with an unusually strong pain. Immediately afterwards, on being laid down, the placenta was found protruding from the external parts. The attendant midwife immediately sent off to a distance of several miles for two medical gentlemen, who arrived about half-past one o'clock on the morning of the 20th. In the meantime, the hemorrhage was inconsiderable. The medical men attempted to turn, and deliver the child, but encountered great difficulties in doing so, the head having remained fixed in the pelvis for an hour or two after the body was born. The recovery was tedious. The patient, now one of the most respected and intelligent midwives in Edinburgh, has had three children since the above period.[2]

CASE V.—*Unavoidable hemorrhage terminated by the expulsion of the placenta; child allowed to be delivered by the natural pains.*—"About half-past six on the morning of April 29th, 1818, a messenger," says Dr. Ramsbotham, "arrived at my house, sent by two medical gentlemen, with a note to this purport: 'We are in attendance upon Mrs. H., whose situation is involved in great uncertainty, from a placental presentation; the bleeding is going on pretty actively, and we wish for your immediate opinion.' On my arrival at the house of the lady, about eight, I was told by one of the gentlemen, 'that since the note was sent off, some strong expulsive pains had come on, which had expelled the placenta through the external parts before the head of the child, and that it was lying upon the bed. That before this occurrence the hemorrhage had been violent, yet not to that extent as apparently to endanger the woman's life; but that since the appearance of the placenta *the flooding had very much abated.*' During our conversation on this unusual occurrence, the gentleman more immediately in attendance, who, at my arrival, was in the bed-room of his patient, came down stairs and reported, 'that the head was presenting at the brim of the pelvis, with a hand down by its side; that there was no want of uterine action; *that the flooding had ceased;* and that his patient did not seem much exhausted.' An appeal was now made to my opinion, as to the further management of the case, to which I replied, 'that as the flooding, the most dangerous symptom, had abated, as the labor-pains continued active, and especially as the woman's strength kept up, there did not appear to be an immediate necessity for a recourse to any means for hastening delivery; watch your patient for a short time, and wait the result; if the flooding should return, or if any dangerous symptom make its appearance, let us know.' In about half an hour after this interview, the gentleman returned with a cheerful countenance, and stated that the child was expelled without further loss of blood, and that his patient was promising to do extremely well," &c.[1]

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[1] Table, No. 48.

[2] Table, No. 2.

CASE VI.—*Placenta gradually coming down, during the labor, into the os uteri, and being at last expelled four hours before the child; with no intermediate hemorrhage.*—Mrs. C., in the eighth month of her fourth pregnancy, was taken in labor on Sunday evening, about nine o'clock. Mr. Chapman was called to her about twelve o'clock. He was informed that the membranes had been ruptured for some time. The os uteri was dilated to the size of a crown-piece, and the head presenting, but still very high. The pains were very strong and regular. On a second examination, an edge of the placenta was discovered "beginning to protrude through the os uteri," with a hemorrhage which was trifling, but increased upon the return of the pains, though still so inconsiderable as not to be directly alarming. Mr. C. did not hence conceive himself justified in proceeding to immediate delivery. But as upon every return of pain the placenta became more and more protruded through the os uteri, without the head advancing, the advice of another practitioner was sought. Previous, however, to his arrival, the pains proved so strong that the os uteri became dilated, and the placenta was completely expelled through the os externum, about three o'clock on Monday morning, with very little hemorrhage. From this moment the pain entirely ceased. The other practitioner did not arrive till five o'clock. "*There had not*," to use Mr. Chapman's own words, "*been the least hemorrhage since the expulsion of the placenta.*" It was now resolved to turn the child; but after two prolonged attempts the feet could not be seized, the uterus being spasmodically contracted in the longitudinal direction, and the circular fibres appearing to act without the consent of the longitudinal. "During the whole of this time the hemorrhage had not in the least increased." Twelve drops of the tincture of opium were now administered. In a very short time the patient became easy and comfortable; and in less than half an hour the natural pains returned, and speedily expelled the child, with the head and arm presenting. Nothing remarkable happened in the convalescence, except a trifling attack of phlegmasia dolens, an affection from which the patient had likewise suffered after her first labor.[1]

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[1] Table, No. 36.

CASE VII.—*Placental and shoulder presentation; placenta expelled; turning.*—The patient, in the sixth month of her fifteenth pregnancy, was attacked with hemorrhage that was alarming in extent, but not so great in quantity as to produce syncope. "I saw her," Dr. Ramsbotham writes, "two hours after the first attack of flooding. The placenta was now lying completely in the vagina, *and there was not the least hemorrhage.* The membranes were ruptured. The shoulder of the child presented. The cervix uteri was unexpanded and rigid, and it was consequently impossible to get my whole hand into the uterine cavity, but I succeeded in reaching the ham of the infant, and was by this means enabled to turn and deliver." "An hour or more" elapsed between the complete detachment of the placenta and the birth of the child. It had been dead for some time. The mother recovered perfectly.[2]

CASE VIII.—*Placental and arm presentation; placenta in the vagina, and without hemorrhage, for about eight hours before the child was born.*—In a woman who had completed the full time of pregnancy, Dr. Macaulay found the placenta expelled from the uterus, and lying in the vagina. She had been flooding previously, but it had ceased about eight o'clock on the morning of the 13th of February, 1816. The late distinguished Dr. Kellie of Leith visited the patient with him. The woman peremptorily refused to allow Drs. Macaulay and Kellie to deliver her. About four o'clock, P.M. the pains quickened, the placenta was expelled from the vagina, and about half an hour afterwards an anencephalous infant followed. The child was in every way well shaped, except as regarded the head. "Dr. Kellie told me," to quote the note which Dr. Macaulay made at the time, "that the head was the smallest he had ever seen, and remarked, that though it was an axiom in midwifery, that when the placenta was implanted over the os uteri, hemorrhage *must* continue till the uterus was emptied, *yet here it stopped as soon as the placenta came down.*"[1]

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[1] Table, No. 14.

[2] Ibid. No. 27.

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[1] Table, No. 10.

SECTION III.—TABLE OF 141 CASES OF EXPULSION AND EXTRACTION OF THE PLACENTA BEFORE THE CHILD: ARRANGEMENT AND DIVISIONS OF THE TABLE.

I have been able to find upon record fifty-six cases of Placental Presentation in which the placental mass was expelled before the child, as in the preceding seven or eight instances which I have brought forward in the last section. Through the kindness of my professional friends, I have collated the notes of seventy-four additional unpublished instances in which the same accident happened. As the entire detail of more instances than those I have already stated, would at the present stage of our inquiries only swell out our pages, without any corresponding advantage, I have deemed it better to throw the principal facts connected with all the cases which I have collated, into a tabular form, in order to present thus in a more concise manner, their general features and individual peculiarities. It is only necessary to premise, in regard to the following table, that under the heads referring to the degree of hemorrhage before and after the separation of the placenta, and the time or interval between the expulsion of the placenta and expulsion of the child, I have as nearly as possible adhered to the identical words used by the reporters themselves, in each case. The table commences with those instances in which the interval between the birth of the placenta and the birth of the child was longest, and progressively proceeds to those in which this interval became shorter and shorter, till at last we come to a set of cases in which the placenta and infant were expelled simultaneously.

For the purpose of assisting in some subsequent deductions, the table is split up into the four following divisions:—

GENERAL TABULAR VIEW OF ONE HUNDRED AND FORTY- OF THE PLACENTA PRECEDED

FIRST

CASES IN WHICH A CONSIDERABLE INTERVAL (FROM 10 HOURS TO 10 MINUTES)

	By whom Observed or Reported.	No. of the Pregnancy.	Period of Delivery.	Degree of hemorrhage *before* the entire separation of the placenta.	Degree of hemorrhage *after* the entire separation of the placenta.	Mode of Delivery of the Child.
1	Dr. Collins, Dublin,	. . . .	9th mon.	. . . .	. . . .	. . . .
2	J. Y. Simpson,	2d,	8th "	Excessive,	Inconsiderable,	Turning,
3	Mr. Cripps, Liverpool,	3d,	9th "	A good deal.	None,	Turning,
4	Dr. Merriman, London,	. . . .	. . . .	. . . .	. . . .	By natural pains,
5	Mr. Hewitt, Earlston,	. . . .	. . . .	Little or none,	. . . .	By natural pains,
6	Dr. J. Ramsbotham,	. . . .	. . . .	. . . .	Little,	Turning,
7	Dr. Newman, Glasgow,	. . . .	9th mon.,	Great,	None,	By natural pains,
8	Baudelocque,	. . . .	. . . .	. . . .	. . . .	. . . .
9	Walter,	. . . .	. . . .	Almost none not 2 oz. in all	Almost none,	Turning,
10	Dr. Macaulay, Edinburgh,	Not 1st,	Full time.	Great,	None,	By natural pains,
11	Velpeau,	. . . .	. . . .	None,	None,	. . . .
12	Mr. Perfect,	. . . .	. . . .	Slight,	Slight,	Turning,
13	Mr. Small, Wemyss,	7th,	7th mon.,	Very great,	Very trifling,	By natural pains,
14	Mr. Chapman,	4th,	8th "	Slight,	Very slight,	By natural pains,
15	Dr. Radford, Manchester,	9th,	9th "	Very great,	Quite arrested,	Long Forceps,
16	Mr. Sidebottom, ditto,	7th,	9th "	Profuse,	Ceased,	By natural pains,
17	J. Y. Simpson,	3d or 4th,	5th "	Great,	None,	Decapitation,
18	Dr. Radford, Manchester,	7th,	9th "	Very consider'ble	Quite arrested,	By natural pains,
19	Dr. Ingleby, Birmingham,	Several,	9th "	Not very great,	None,	Turning,
20	Mr. Bailey. Thetford,	. . . .	7th "	Profuse,	. . . .	Turning,
21	J. Y. Simpson,	. . . .	7th "	Not great,	None,	Turning,
22	J. Y. Simpson,	6th,	7th & 8th,	Very great,	None,	By natural pains,
23	Dr. Todd, Colinsburgh,	Several,	8th "	Not alarming,	. . . .	. . . .
24	Dr. Frazer, Aberdeen,	1st,	9th "	Moderate,	Scarcely any,	Turning,
25	Dr. Radford, Manchester,	3d,	9th "	Very profuse,	Quite arrested,	By natural pains,
26	Dr. Gardiner, Glasgow,	1st	9th "	Very great,	None,	By natural pains,
27	Dr. F. Ramsbotham,	15th,	6th "	Alarming,	Not the least,	Turning,
28	Professor Glendrin,	2d,	8½ mon's,	None during la-	None,	By natural pains,
29	Mr. Wood, Manchester,	8th,	9th "	Very great, [bor.	Quite stopped,	By natural pains,
30	Dr. Campbell, Edinburgh,	1st,	8th "	Little,	. . . .	. . . .
31	Dr. Gardiner, Dundee,	1st,	7th "	Moderate,	Very slight,	By natural pains,
32	Mr. Hay, Glasgow,	1st,	9th "	Excessive,	Very little,	Naturally,
33	Dr. Ingleby, Birmingham,	9th,	5th "	Great,	None,	Turning,
34	Dr. Irvine, Pitlochrie,	10th	9th "	None,	None,	Extraction.
35	Dr. Young, Glasgow,	1st,	6th & 7th,	Very great,	None,	By natural pains,
36	Dr. John Ramsbotham,	. . . .	. . . .	Violent,	Soon ceased,	By natural pains,
37	Dr. Todd, Colinsburgh,	Several,	. . . .	Not great,	. . . .	. . . .
38	Dr. Radford, Manchester,	5th,	8th mon.,	Very great,	Quite arrested,	By natural pains,
39	Dr. Wharrie, Hamilton,	9th,	8th to 9th,	Considerable,	None,	By natural pains,
40	Mr. Dorrington, Manches'r,	7th,	9th "	Great,	None,	By natural pains,
41	Dr. Forbes, Kennoway,	4th,	9th "	Very great,	None of any consequence,	Forceps,
42	Dr. Millar, Kilmarnock,	1st,	7th "	Great, 6lbs. in 2 days,	Slight oozing,	Turning,

REMARKS.

4. See details under Section VI.

5. Mr. H. was sent for on account of the expulsion of the placenta, nothing unusual having occurred beforehand. He was some miles distant, but arrived just before the child was born. There was little or no hemorrhage.

8. "A midwife had extracted the placenta some hours before, and had been unable to turn the child, whose arm presented with the head. The uterus was strongly contracted on the child, but discharged a few drops of blood."

9. See details under Section VI.

10. See Case under Section II.

11. See Section V.

14. "In this case, very little more blood was lost than women usually do, when the placenta is expelled in the usual manner."

15. "In this case the expulsatory efforts were energetic to the time of accomplishing the separation and expulsion of the placenta, when they ceased. No hemorrhage occurring afterwards, it was deemed advisable to wait the 4 hours."

18. "The hemorrhage being arrested, there was no need to interfere, further than to adopt those measures which are necessary to support the vital powers."

19. "The placenta had been expelled nearly through the os externum. A large quantity—nine-tenths, I should say—had been cut off with a pair of scissors. I saw the case very soon afterwards; passed my hand, and delivered the child, and then removed the small bit of placenta and membranes."

ONE CASES IN WHICH THE EXPULSION OR EXTRACTION THE BIRTH OF THE CHILD.

DIVISION.

ELAPSED BETWEEN THE EXPULSION OF THE PLACENTA AND THE BIRTH OF THE CHILD.

Time between Birth of Placenta and Birth of Child.	Presentation of the Child.	Results.		Where Reported, or by whom Communicated.
		To Mother.	To Child	
Evening before,	Foot,	Recovered,	Dead,	Practical Observations, p. 192.
Several hours,	. . . . . .	do.	do.	See Case of Mrs. H. in Section II.
10 hours,	Arm,	do.	do.	Communicated by Mr. Cripps.
Many hours,	. . . . . .	Died,	. . .	Synopsis, p. 126.
A considerable time,	. . . . . .	Recovered,	. . .	Communicated by Dr. Tait, Edinburgh.
A considerable time,	Head,	do.	. . .	Practical Observations, vol. ii. p. 232.
Several hours,	Head,	do.	Dead,	Communicated by Dr. Smith, Glasgow.
Some hours,	Arm & head	do.	. . .	Baudelocque, vol. ii., p. 37
Probably some hours,	Crossbirth,	Died,	Dead,	De Morbis Peritonei, p. 83.
About 8 hours,	Arm & head	Recovered,	do.	Communicated by Dr. Macaulay.
Above 6 hours,	. . . . . .	do.	do.	Traité des Accouchemens, i., p. 356.
5 hours,	Abdomen,	do.	Alive,	Cases in Midwifery, vol. ii., p. 288.
4 to 5 hours,	Head,	do.	Dead,	Communicated by Dr. Skae, Leven.
4 hours,	Head & arm	do.	. . .	Annals of Medicine, vol. iv., p. 308.
4 hours.	Head,	do.	Dead,	Communicated by Dr. Radford,
4 hours,	Head,	do.	do.	Communicated by Dr. Radford.
3 or 4 hours,	Shoulder,	do.	do.	Seen with Dr Graham Weir.
3 hours,	Head,	do.	do.	Communicated by Dr. Radford.
About 3 hours,	Head,	do.	do.	Communicated by Dr. Ingleby.
Above 2 hours,	. . . . . .	do.	do.	Prov. Trans., vol. vii., p. 338.
Above 2 hours,	Arm,	do.	do.	Seen with Dr. Lewins of Leith.
Nearly 2 hours,	Head,	do.	do.	Seen with Mr. Hill, Portobello.
Less than 2 hours,	. . . . . .	do.	do.	Communicated by Dr. Todd.
1½ hours,	Arm,	do.	do.	Communicated by Dr. Fraser.
1½ hours.	Head,	do.	do.	Communicated by Dr. Radford.
About 1½ hours,	Head,	do.	do.	Communicated by Dr. Smith, Glasgow.
An hour or more,	Shoulder,	do.	Putrid,	Communicated by Dr. Ramsbotham.
1 hour,	. . . . . .	do.	do.	Médecine Pratique, tom. ii., p. 224.
1 hour,	Head,	do.	Dead,	Communicated by Dr. Radford.
. . . .	Breech,	do.	do.	System of Midwifery, p. 360.
About 1 hour,	Breech,	do.	do.	Communicated by Dr. Gardiner.
About 1 hour.	Head.	Died,	do.	Communicated by Dr. Smith, Glasgow.
About 1 hour,	Head,	Recovered,	do.	Communicated by Dr. Ingleby.
About 1 hour,	Feet,	do.	Putrid,	Communicated by Dr. Irvine.
Above ½ hour,	Head,	do.	Dead,	Communicated by Dr. Smith, Glasgow,
Above ½ hour at least,	. . . . . .	do.	. . .	Pract. Obs., Case 154, vol. ii., p. 229.
At least ½ hour,	. . . . . .	do.	Dead,	Communicated by Dr. Todd.
½ hour,	Head,	do.	do.	Communicated by Dr. Radford.
½ hour,	Head,	do.	do.	Communicated by Dr. Thompson, Hamilton.
½ hour,	Head,	do.	do.	Communicated by Dr. Radford.
Within ½ hour,	Head,	do.	do.	Communicated by Dr. Skae, Leven.
About ½ hour,	Shoulder,	do.	do.	Communicated by Dr. Paxton.

REMARKS.

20. "I found the vagina completely filled with the placenta, and the os uteri firmly contracting upon the funis."

26. "The hemorrhage was very excessive, so long as the placenta was only partially separated, but was immediately suppressed by completely detaching it."

27. Dr. R. saw her "two hours after the first attack of flooding. The placenta was then wholly in the vagina, but there was not the least hemorrhage; the membranes were ruptured; the cervix unexpanded."

29. "Flooding 9 hours before placenta was expelled, but was immediately suppressed on its expulsion."

30. Dr. C. kindly informs me that the woman recovered perfectly.

31. Note by Dr. G. "There was no flooding previous to the commencement of labor pains, and it was moderate throughout."

32. See case given in full in Section VI.

34. "Absolutely no more hemorrhage than in a common natural labor."

37. Dr. T. found the placenta lying in the vagina; the pains were strong and effective; and the infant was expelled in half an hour.

39. Dr. Wharrie found the placenta partly in the vagina. and partly in the os uteri. He extracted it by a kind of twisting motion, and the hemorrhage immediately ceased.

42. "In this case, the pains entirely left her after the expulsion of the placenta; a full dose of ergot was given, the os uteri being fully dilated, and was followed by one or two smart pains, by which the child was expelled."

(FIRST DIVISIO

	By whom Observed or Reported.	No. of the Pregnancy.	Period of Delivery.	Degree of hemorrhage *before* the entire separation of the placenta.	Degree of hemorrhage *after* the entire separation of the placenta.	Mode of Delivery of the Child.
43	Dr. Millar, Kilmarnock,	10th.	9th mon.	Great, 8 lbs. in 3 [d'ys.	About a pound,	Gentle traction,
44	Mr. Malcolm, Dundee,	Lge. fam.	. . . .	Very little,	. . . .	Turning,
45	Lamotte,	. . . .	. . . .	Great,	. . . .	Turning,
46	Dr. F. Ramsbotham,	9th,	9th mon.	Very great,	None,	By natural pains,
47	Mr. Johnstone, Brompton,	5th,	9th "	Very great,	None,	By natural pains,

SECOND

CASES IN WHICH A SHORTER INTERVAL (LESS THAN 10 MINUTES) ELAPSED

48	Dr. Dewar, Dunfermline,	. . . .	9th mon.	Profuse,	None,	By natural pains,
49	Dr. Fraser, Aberdeen,	6th.	8th "	Very moderate,	Great,	Turning,
50	Mr. Nimmo, Dundee,	Lge. fam.	. . . .	Considerable,	. . . .	By natural pains,
51	Dr. John Ramsbotham,	. . . .	. . . .	Severe,	Stopped,	By natural pains,
52	Dr. John Ramsbotham,	3d,	9th mon.	. . . .	None,	By natural pains,
53	Dr. F. Ramsbotham,	12th,	9th "	Exhausting,	None,	Turning,
54	Dr. Smith, Lasswade,	3d,	9th "	Excessive,	. . . .	By natural pains,
55	Dr. Maxwell Adams,	. . . .	7th "	Very profuse,	. . . .	. . . .
56	Dr. Barlow,	2d,	9th "	Profuse,	Profuse,	Turning,
57	Mr. Crawford, Glasgow,	5th,	9th "	Not great,	None,	Turning,
58	Mr. Crawford, "	4th,	9th "	Exhausting,	None,	Turning,
59	Dr. M'Donald, "	7th,	9th "	Excessive,	"A good deal,"	Turning,
60	Dr. F. Ramsbotham,	2d,	6½ mons.	Exhausting,	None,	Evisceration,
61	Dr. F. Ramsbotham,	3d,	8th "	Most violent,	None,	By natural pains,
62	Mr. Tindal, Glasgow,	10th,	9th "	Fearful,	None,	Turning,
63	Mr. Denny,	. . . .	. . . .	. . . .	. . . .	By natural pains,
64	Dr. Conquest, London,	Lge. fam.	6½ mons.	Active,	. . . .	. . . .
65	Mr. Elkington, Birmingh'm	. . . .	7th, 8th "	A good deal,	. . . .	By natural pains.
66	Mr. Rose, Swaffham,	7th,	9th "	Not alarming,	None,	By natural pains.
67	Reviewer,	. . . .	. . . .	. . . .	. . . .	By natural pains,
68	Dr. Menzies, Glasgow,	3d,	8th, 9th "	Considerable,	None,	By natural pains,
69	Mr. James,	5th,	8th "	Very great,	Slight,	By natural pains,
70	Dr. Francis,	. . . .	. . . .	. . . .	. . . .	By natural pains,
71	Dr. Wilson, Whitburn,	4th,	. . . .	Slight,	. . . .	By natural pains,

THIRD

CASES IN WHICH THE PLACENTA WAS EXPELLED IMMEDIATELY

72	Mr. Greenhow, Newcastle,	6th,	7th mon.	Considerable,	. . . .	Extracted,
73	Mr. Campbell, Glasgow,	1st,	7th, 8th "	Excessive,	. . . .	Turning,
74	Mr. Flemming, "	5th,	. . .	Considerable,	. . . .	Turning,
75	Mr. Hardcastle, Newcastle,	Several,	7th, 8th "	Very great,	. . . .	By natural pains,
76	Mr. Lowe, Manchester,	3d,	8th "	Very profuse,	. . . .	By natural pains,
77	Mr. Sidebottom, "	3d,	9th "	Very copious,	. . . .	By natural pains,
78	Dr. Smellie,	Several,	9th "	Slight,	. . . .	By natural pains,
79	Dr. Stewart, Kelso,	6th,	8th "	Severe,	. . . .	By natural pains,
80	Mr. Tulloch, Newcastle,	Several,	7th "	. . . .	. . . .	. . . .
81	Mr. Wood, Manchester,	6th,	8th "	Very profuse,	. . . .	By turning,
82	Mr. Wood, "	5th,	9th "	Very consider'ble	. . . .	By natural pains,
83	Dr. Young, Edinburgh,	. . . .	9th "	Very great,	. . . .	By turning,
84	Dr. Currie, Lanark,	4th,	. . . .	Violent,	. . . .	By natural pains,
85	Dr. Carruthers, Dundee,	3d or 4th,	9th mon.	Considerable,	. . . .	By natural pains,
86	Dr. F. Ramsbotham,	4th,	8th "	Violent,	. . . .	By natural pains,

REMARKS.

43. "The placenta was lying partly in the vagina, and partly in the uterus. After its extraction, the feet presented, which were laid hold of, and, at each pain, firm but gentle traction employed, till the child was delivered."

45. "I found the placenta occupying wholly the vagina, and pushing almost out of it. I immediately pulled it away, whereupon, the membranes being torn, the waters came in great plenty, and I brought away a dead child by the feet." The flooding had been excessive.

46. Dr. R. found the membranes pressing on the perineum, and the whole of the placenta almost in the vagina. It passed outside immediately on rupturing the membranes.

48. See Case under Section II.

49. See Case under Section VI.

52. Violent hemorrhage came on two days before delivery. It ceased entirely, however, and did not return.

53. Copious hemorrhage came on 3½ hours before delivery, on the membranes spontaneously rupturing. Dr. R. found great part of the placenta in the vagina; there was no pain nor hemorrhage; and he would not have turned, had the shoulder not been the presenting part. The placenta came away as the shoulders were passing the brim, before the head was extracted.

56. The placenta was expelled while she was on her feet. "She attempted to walk up stairs, and before she could reach the bed, a violent pain seized her, which instantly expelled the placenta, and disparted

—continued.)

Time between the Birth of Placenta and Birth of the Child.	Presentation of the Child.	Results.		Where Reported, or by whom Communicated.
		To Mother.	To Child	
20 minutes,	Feet,	Recovered,	Dead,	Communicated by Dr. Paxton.
Upwards of 20 min.,	Arm,	do.	do.	Communicated by Dr. Keiller.
Not ¼ of an hour,	. . . . .	do.	do.	Traité des Accouchemens, p. 407.
10 minutes,	Head,	do.	do.	Communicated by Dr. Ramsbotham.
About 10 minutes,	Head,	do.	do.	Communicated by Dr. Elliott, Carlisle.

DIVISION.

Between the Separation of the Placenta and the Birth of the Child.

Less than 10 min.,	. . . . .	Recovered,	Alive,	Communicated by Dr. Dewar.
5 or 10 minutes,	. . . . .	Died,	do.	Communicated by Dr. Frazer.
A short time,	Breech,	Recovered,	Dead,	Communicated by Dr. Keiller.
A short time,	. . . . .	do.	. . .	Pract. Obs., Case 155, vol. ii., p. 231.
A short time,	Breech,	do.	Putrid,	Pract. Obs., Case 156, vol. ii., p. 233.
A short time,	Shoulder,	do.	Dead,	Communicated by Dr. F. Ramsbotham.
A few pains,	Head,	do.	Alive,	Communicated by Dr. Smith.
A few minutes,	Foot,	do.	Putrid,	Monthly Journal, vol. iv., p. 936.
A few minutes,	Shoulder,	do.	Alive,	Essays on Midwifery, &c., p. 273.
A few minutes,	Head,	do.	do.	Communicated by Dr. Smith, Glasgow.
A few minutes,	Head,	do.	do.	Communicated by Dr. Smith, Glasgow.
A few minutes,	Head,	do.	Dead,	Communicated by Dr. Smith, Glasgow.
A few minutes,	Shoulder,	Died,	do.	Communicated by Dr. F. Ramsbotham.
A few minutes,	Head,	Recovered,	do.	Communicated by Dr. F. Ramsbotham.
A few minutes,	Head,	Died,	do.	Communicated by Dr. Smith, Glasgow.
Next pain,	. . . . .	Recovered,	Alive,	Lancet, 1831–32, vol. i., p. 110.
Next pain,	. . . . .	do.	Dead,	Communicated by Dr. Conquest.
Soon,	. . . . .	do.	do.	Communicated by Dr. Ingleby.
Quickly,	Head,	do.	Alive,	Communicated by Mr. Rose.
. . . .	. . . . .	do.	. . .	Med. Chir. Review, vol. iii., p. 317.
5 minutes,	Head,	do.	Alive,	Communicated by Dr. Smith, Glasgow.
4 minutes,	Head,	do.	Dead,	London Med. Repository, vol. vi., p. 412.
3 minutes,	. . . . .	do.	Alive,	Francis's Edition of Denman, p. 485.
Less than 2 minutes,	Head,	do.	do.	Communicated by Dr. Wilson.

DIVISION.

Before the Child, or both were expelled together.

Almost immediately,	Breech,	Recovered,	Dead,	Communicated by Dr. Dawson, Newcastle.
Together,	Head,	do.	Alive,	Communicated by Mr. Campbell.
Turned immediately,	Head,	do.	do.	Communicated by Dr. Currie, Lanark.
Immediately,	Head,	do.	Dead,	Communicated by Dr. Dawson, Newcastle.
Immediately,	Head,	do.	do.	Communicated by Dr. Radford.
Immediately,	Head,	do.	do.	Communicated by Dr. Radford.
Immediately,	. . . . .	do.	Alive,	Midwifery, vol. ii., p. 310.
Immediately,	Head,	do.	Dead,	Communicated by Dr. Stewart.
Immediately,	. . . . .	do.	do.	Communicated by Dr. Dawson, Newcastle.
Immediately,	Head,	Died,	do.	Communicated by Dr. Radford.
Immediately,	Head,	Recovered,	do.	Communicated by Dr. Radford.
Immediately,	. . . . .	do.	. . .	Communicated by Dr. Young.
Almost at same time,	Head,	do.	Alive,	Communicated by Dr. Currie.
Almost at same time,	Head,	do.	do.	Communicated by Dr. Carruthers.
Not many moments,	Head,	do.	Dead,	Communicated by Dr. Ramsbotham.

REMARKS.

the funis about six inches from the child's navel. A great effusion of blood followed, and the woman fainted ere she could be laid down on the bed."

58, 59, 73. "In these three cases the placenta was detached in introducing the hand to turn, and lay in the vagina till the feet were brought down." Dr. Smith's note.

60. See case at length in Section VI. 62. See case at length in Section VI.

64. Active hemorrhage had taken place before Dr. C.'s arrival. "On examination, the pelvis was filled with coagula, and something like placenta. Another pain expelled an uninterrupted ovum, which was instantly ruptured, but the child was dead."

68. The placenta was born along with the head; but Dr. M. was sensible of its being wholly detached, and lying in the vagina for fully five minutes.

70. Though not actually stated, it is clear from the context that the woman here, and in Case 143, recovered.

71. When Mr. W. saw the woman there was a little hemorrhage, but not so much as to cause any alarm, either for the mother or child. The pains were strong and downbearing, and in about twenty minutes the placenta was expelled. The next pain expelled the child.

72. The placenta was detached to some extent when Mr. G. was called in. He immediately separated the remainder, and removed it.

78, 91, 92. The fact is not stated, but it is manifest from the context that the mothers recovered.

(THIRD DIVISIO

	By whom Observed or Reported.	No. of the Pregnancy.	Period of Delivery.	Degree of hemorrhage *before* the entire separation of the placenta.	Degree of hemorrhage *after* the entire separation of the placenta.	Mode of Delivery of the Child.
87	Dr. Brownlee, Shotts,	6th or 7th	. . . .	. . . .	. . . .	By natural pains,
88	Dr. Conquest, London,	. . . .	9th mon.	Violent,	. . . .	By natural pains,
89	Dr. Dawson, Bathgate,	7th,	8th "	Profuse,	. . . .	By natural pains,
90	Professor Murphy, Lond.,	Not 1st,	9th "	Moderate,	. . . .	By natural pains,
91	Dr. Smellie,	2d,	8th "	. . . .	. . . .	By natural pains,
92	Dr. Smellie,	. . . .	8th "	Profuse,	. . . .	By natural pains,
93	Anonymous,	. . . .	. . . .	. . . .	. . . .	By natural pains,
94	Gendrin,	. . . .	. . . .	Very great,	. . . .	By natural pains,
95	Mr. Easton, Glasgow,	. . . .	9th mon.	Considerable,	. . . .	By natural pains,
96	Mr. Easton, "	. . . .	9th "	Considerable,	. . . .	By natural pains,
97	Dr. F. Ramsbotham,	Several,	8th & 9th,	Exhausting,	. . . .	Turning.
98	Mr. Rose, Swaffham,	4th,	9th "	Considerable,	. . . .	By natural pains,
99	Mr. Rae, Edinburgh,	4th,	9th "	Considerable,	. . . .	By natural pains.
100	Schweighauser, Strasburg,	. . . .	. . . .	. . . .	. . . .	. . . .
101	Dr. Robert Lee, London,	. . . .	7th mon.	Considerable,	. . . .	. . . .

FOURTH

CASES IN WHICH THE EXACT PERIOD BETWEEN THE SEPARATION

102	Mr. Bailey, Thetford,	4th,	9th mon.	Profuse,	Profuse,	Turning.
103	Mr. Bull,	6th,	. . . .	Most alarming,	. . . .	Turning,
104	Cauviere,	. . . .	. . . .	. . . .	None,	Forceps,
105	Dr. Clarke,	. . . .	. . . .	. . . .	. . . .	. . . .
106	Dr. Hamilton, Edinburgh,	. . . .		. . . .	None,	. . . .
107	Dr. Hamilton, "	. . . .		. . . .	None,	. . . .
108	Labayle, Montpellier,	. . . .		. . . .	. . . .	. . . .
109	Romaine, Bagneres,	. . . .		. . . .	. . . .	. . . .
110	Lamotte,	. . . .	. . . .	Severe,	Considerable,	Turning,
111	Dr. Robert Lee, London,	. . . .	7th mon.	Profuse,	. . . .	. . . .
112	Leroux,	. . . .	7th "	Great,	Much diminished	Decapitation,
113	Dr. Löwenhart,	. . . .	. . . .	. . . .	. . . .	Turning,
114	Dr. Maunsell, Dublin,	. . . .	. . . .	Profuse,	. . . .	By natural pains.
115	Mr. Milligen,	. . . .	. . . .	. . . .	. . . .	By natural pains,
116	Sir F. Ould,	. . . .	. . . .	Profuse,	. . . .	Turning,
117	Pardigon,	. . . .	7th mon.	Almost none,	None,	Turning,
118	Dr. F. Ramsbotham,	. . . .	. . . .	Copious,	Little,	Turning,
119	Dr. J. Ramsbotham,	. . . .	. . . .	. . . .	Little or none,	. . . .
120	Dr. J. Ramsbotham,	. . . .	. . . .	. . . .	Entirely ceased,	. . . .
121	Mr. Elkington, Birmingh.	8th,	. . . .	. . . .	. . . .	. . . .
122	Dr. Cahill, Berwick,	. . . .	6th & 7th,	. . . .	. . . .	. . . .
123	Mr. Hardcastle, Newcastle,	. . . .	. . . .	Very great,	Not much,	By natural pains,
124	Dr. Moody, St. Andrews,	. . . .	. . . .	. . . .	. . . .	Turning,
125	Dr. Mather, Brechin,	. . . .	. . . .	Not great,	. . . .	By natural pains,
126	Dr. Nimmo, senior,	. . . .	. . . .	Severe,	. . . .	. . . .
127	Mr. Rose, Swaffham,	7th,	9th mon.	Moderate,	None,	Naturally,
128	Dr. Wilson, Glasgow,	Lge. fam.	. . . .	Profuse,	Profuse,	Turning,
129	Dr. Wilson, "	. . . .	. . . .	Profuse,	Profuse,	Turning,
130	F. Osiander, Gottingen,	. . . .	7th mon.	. . . .	. . . .	Naturally,
131	F. Osiander, "	. . . .	4th "	. . . .	. . . .	Naturally,
132	F. Osiander, "	. . . .	7th "	. . . .	. . . .	Turning,
133	F. Osiander, "	9th,	9th "	Violent,	None,	Naturally,
134	Dr. Trefurt, Gottingen,	. . . .	3d "	Very great,	. . . .	Turning,
135	Kory,	. . . .	. . . .	. . . .	. . . .	. . . .
136	Loss, Dorchester,	. . . .	Lge. fam.	. . . .	. . . .	. . . .
137	Giffard,	7th,	. . . .	Violent,	. . . .	Turning,
138	Mercier,	. . . .	. . . .	None,	Almost none,	Turning,
139	Amand,	. . . .	. . . .	Profuse,	. . . .	Turning,
140	Dr. Tennant, Falkirk,	2d,	9th mon.	Severe,	. . . .	Forceps,
141	Dr. Morrison, Dalkeith,	Several,	. . . .	Severe,	None,	By natural pains,

REMARKS.

88. On endeavoring to turn, "the os became so much irritated by the attempt to introduce the hand, that the organ forcibly contracted, expelling the hand, placenta, and child, and an almost incredible quantity of blood."

94. The hemorrhage ceased on the waters being allowed to escape by a female catheter, passed through the placenta four hours before the birth of the infant, which was expelled with the placenta before it, covering the head. Slight hemorrhage followed the delivery.

95. In this case the placenta, though born along with the head of the child, was detached from the uterus about an hour. There was no hemorrhage from the time it was separated.

96. The placenta was born with the body of the child, but it had been detached for some hours.

97. The placenta passed, during the extraction of the child, before the breech.

99. I saw the woman a fortnight after delivery carrying her child, and well. 101. See Sect. V.

103. The arm was found to present after the expulsion of the placenta, and turning was then had recourse to. There was no word of hemorrhage after the placenta was expelled.

104. "Professor Cauviere has told me, that in one case where he introduced the forceps, for inertia of the

—*continued.*)

Time between the Birth of Placenta and Birth of the Child.	Presentation of the Child.	Results.		Where Reported, or by whom Communicated.
		To Mother.	To Child	
One pain,	Head,	Recovered,	Alive,	Communicated by Dr. Wilson, Whitburn.
One pain,	. . . . . .	do.	do.	Communicated by Dr. Conquest.
One pain,	Arm,	do.	Dead,	Communicated by Dr. Dawson.
One pain,	. . . . . .	do.	do.	Communicated by Dr. Murphy.
One pain,	. . . . . .	do.	do.	Cases in Midwifery, vol. ii., p. 311.
One pain,	. . . . . .	do.	. . .	Cases in Midwifery, vol ii., p. 318.
Same pain,	. . . . . .	do.	. . .	Medico-Chirurg. Review, vol. iii.
Together,	. . . . . .	do.	Alive,	Médecine Pratique, tom. ii., p. 349.
Together,	Head,	do.	Dead,	Communicated by Dr. Smith, Glasgow.
Together,	Head,	do.	do.	Communicated by Dr. Smith, Glasgow.
Together,	Head,	do.	Alive,	Communicated by Dr. Ramsbotham.
Together,	Head,	do.	do.	Communicated by Mr. Rose.
Together,	Head,	do.	do.	Communicated by Dr. Campbell, Edinburgh.
Together,	. . . . . .	. . . . . .	. . .	Pratique des Accouchemens, p. 224.
. . . .	. . . . . .	Recovered,	. . .	Clinical Midwifery, p. 148, case 269.

DIVISION.

OF PLACENTA AND THE BIRTH OF CHILD NOT KNOWN.

. . . .	Head,	Recovered,	Alive,	London Med. Repos., vol. xvi., p. 451.
. . . .	Arm,	do.	Dead,	Medical Gazette, vol. xix., p. 622.
. . . .	. . . . . .	. . . . . .	. . .	Pardigon de l'Insertion du Placenta.
. . . .	. . . . . .	Died,	. . .	Collins' Midwifery, p. 91.
. . . .	. . . . . .	Recovered,	. . .	Notes of Lectures and Obs., p. 818.
. . . .	. . . . . .	do.	. . .	Notes of Lectures and Pract. Obs., p. 818.
. . . .	. . . . . .	do.	. . .	Essai sur l'Hém. Uter., Montpellier, 1827.
. . . .	. . . . . .	do.	. . .	
. . . .	. . . . . .	do.	Alive,	Traité des Accouchemens, p. 405.
. . . .	. . . . . .	do.	Dead,	Clinical Midwifery, p. 144, case 268.
. . . .	Arm,	do.	do.	Leroux sur les Pertes du Sang, p. 262.
. . . .	Arm,	do.	do.	Neue Zeitschrift für Geburtsk, bd. vii., h. 3. See Kleinert's Rep., 1842, vi., p. 58.
. . . .	Head,	do.	. . .	Dublin Journal, vol. v., p. 373.
. . . .	. . . . . .	do.	Alive,	Lancet, 1831–32, vol. i., p. 282.
. . . .	Head,	do.	do.	Ould's Midwifery, p. 77.
. . . .	Shoulder,	do.	. . .	De l'Insertion du Placente à l'Orifice Uter.
. . . .	Head,	do.	. . .	Pract. Obs., vol. ii., p. 232.
. . . .	. . . . . .	do.	. . .	Pract. Obs., vol. ii., p. 235.
. . . .	. . . . . .	do.	. . .	Pract. Obs., vol. ii., p. 235.
. . . .	. . . . . .	do.	Dead,	Communicated by Dr. Ingleby.
. . . .	. . . . . .	do.	do.	Communicated by Dr. Cahill.
. . . .	Head,	do.	do.	Communicated by Dr. Dawson.
. . . .	. . . . . .	do.	Breath'd	Communicated by Dr. Smith, St. Andrews.
. . . .	. . . . . .	do.	Alive,	Communicated by Dr. Binning, Arbroath.
. . . .	. . . . . .	do.	do.	Communicated by Dr. Keiller, Dundee.
. . . .	Head,	do.	do.	Communicated by Mr. Rose.
. . . .	. . . . . .	do.	Dead,	Communicated by Dr. Wilson.
. . . .	. . . . . .	do.	do.	Communicated by Dr. Wilson.
. . . .	Head,	do.	do.	Kleinert's Repertorium, 1832. April Number. t. 24.
. . . .	Shoulder,	do.	do.	
. . . .	Head,	do.	do.	
. . . .	Head,	do.	Putrid,	
. . . .	Shoulder,	do.	. . .	Hannoversche Annalen, Sept., 1841. Vid. Neue Zeitschrift für Geburtskunde, 1843, p. 121.
. . . .	. . . . . .	do.	. . .	
. . . .	. . . . . .	do.	Alive,	Observationes Medicinales, Lond., 1672, p. 380.
. . . .	Arm,	Died.	. . .	Cases in Midwifery, p. 516.
. . . .	. . . . . .	do.	Dead,	Journal Gén. de Médecine, tom. 45, p. 305.
. . . .	Head & Arm.	Recovered,	do.	Observations sur la Pratique, &c., p. 336.
. . . .	Head,	do.	do.	Communicated by Dr. Tennant.
. . . .	Head,	do.	do.	Communicated by Dr. Morrison.

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uterus, when the head was in the pelvis, he was quite astonished to see the placenta pass out of the vagina, before the child, without the slightest hemorrhage."—*Purdigon's Essay.*

108, 109. "It may happen," says Labayle, "that the placenta, though attached by its centre to the os, is pushed out before the head of the child, and the labor terminates in the most happy manner. I have myself witnessed such a fact; and M. Romaine, Professor of Midwifery at Bagnares, has communicated to me also an observation of this kind."

111. Dr. Lee found the placenta protruding through the orifice of the vagina. He immediately extracted it and a dead child followed.

101. In this case the placenta was protruding at the os uteri; on drawing it forward gently, the whole ovum escaped without rupture of the membranes.

113. A midwife had separated the placenta, which presented, and had drawn it out of the external parts.

119, 120. I am kindly informed by Dr. Ramsbotham that he knows "one of the mothers recovered, and he believes the other also."

128, 129. See Section V.

137. See case in Section VI.

138. See case in Section VI.

For details of other cases see future Sections.

1st Division.—Cases in which a considerable interval—varying from ten minutes to ten hours or upwards—elapsed between the expulsion of the placenta and the birth of the child, including the forty-seven instances standing at the head of the table.

2d Division.—Cases, comprehending those from No. 48 to No. 71, in which the interval was shorter.

3d Division.—Cases, running from No. 72 to No. 101, in which the child was born immediately after the extrusion of the placenta, or expelled with it.

4th Division.—Cases, from No. 102 onwards, in which the period intervening between the expulsion of the placenta and child is not specified by the reporters, though the context shows that in many of this class the interval was evidently considerable.

SECTION IV.—GENERAL DEDUCTIONS REGARDING THE PRECEDING 141 CASES.—1. NUMBER OF THE PREGNANCY. 2. PERIOD OF DELIVERY. 3. MODES OF PRESENTATION OF THE CHILD. 4. MODES OF DELIVERY. 5. NUMBER OF CHILDREN LOST AND SAVED.

We shall now attempt to state in a generalized form some of the more important points deducible from the consideration and examination of the preceding table. Before doing so, however, I would take this opportunity of remarking, that the total separation and expulsion of the placenta before the infant, does not seem to be so very rare and uncommon a circumstance as medical men generally believe, and as authors allege it to be. Dr. Collins states,[1] that it is "extremely rare to meet with a total separation of the placenta in unavoidable hemorrhage." In reporting Mr. Denny's case,[2] Mr. Gower observes, "it is perhaps a solitary instance in the annals of obstetric practice. The placenta was brought into the world before the child. The uterus closed upon the body of the fœtus so as to prevent hemorrhage, and after another pain the child was born alive. It was a quick labor, and no ill effects followed from an accident, from which disastrous consequences might have been reasonably apprehended to both mother and child. Perhaps," adds Mr. Gower, "there is no other such case on record, and it merits notice, as an example of the competency of nature to provide for extraordinary emergencies."[3]

The number of cases included within the preceding table shows the entire separation of the placenta in placental presentations to be by no means so rare as these and other authors seem to suppose. I have no doubt that the records of medicine contain more cases than I have had leisure or opportunity of searching out; and I feel assured that a more extensive and industrious inquiry of private practitioners than I have been able to institute, might have brought to light a considerable number of additional instances.

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[1] Treatise on Midwifery. p. 90.

[2] Table, No. 63.

[3] Lancet for 1831-32, vol. i. p. 119.

1. *Number of the Pregnancy in the cases included in the Table.*—In 81 cases the number of the pregnancy is stated, or facts mentioned, so as to enable us to infer whether the patient had previously borne a family or not.

In 12	cases the mother had	"several" children previously.	
4	" "	"a large family."	
1	case it was the	15th	pregnancy.
1	" "	12th	"
3	cases it "	10th	"
5	" "	9th	"
2	" "	8th	"
8	" "	7th	"
6	" "	6th	"
6	" "	5th	"
9	" "	4th	"
10	" "	3d	"
5	" "	2d	"
8	" "	1st	"
Total, 81			

2. *Periods of utero-gestation at which the patients were delivered.*—In 89 cases out of the 141, the requisite information is supplied on this point. The result is as follows:

From the 6th month,	3	were delivered.
From the 6th to the 7th month,	5	" "
From the 7th to the 8th month,	19	" "
From the 8th to the 9th month,	19	" "
From the 9th to full time,	43	" "
Total,	89	cases.

The preceding data are so far corroborative of the well-known fact, that in placental presentations the labor is very frequently premature. In 28 of his 42 cases of placenta prævia, Dr. Rigby mentions the date at which labor came on. In 13 of the 28 the labor was more or less premature; in 15 the women are said to have reached the full term of pregnancy. Dr. Lee has reported 36 cases of unavoidable hemorrhage;[1] in 3 instances he does not state the date of the labor; in 2 only had the women reached their full time; and in the remaining 31 patients, the labor was premature. Out of 16 cases of the same complication reported by Madame Lachapelle,[2] 1 patient was near the seventh month of pregnancy; 6 were delivered during the seventh month; 5 during the eighth month; 1 at the beginning of the ninth month; 1 towards the middle of it, and 2 during the course of it. Levret discusses at some length the question, "why some of the women who have the placenta implanted upon the cervix uteri arrive at the full time, and why the greater part (*la plupart*) of those who are in the same condition do not reach that period."[3]

3. *Modes of Presentation of the Child.*—This is specified in 90 cases.

In 4	cases the	feet presented.
In 6	"	breech presented.
In 21	"	trunk or upper extremity presented.
In 59	"	head presented.

In 4 of the head cases (Table, Nos. 8, 14, 10, and 139), an arm presented along with the head.

In the above, as in all other statistical returns, referring to the presentation of the child in cases of placenta prævia, the number of preternatural presentations, and particularly of cross-births, is remarkable.

4. *Modes of Delivery of the Child.*—The means by which the children were ultimately delivered have varied greatly according to the peculiarities arising from the presentation, and the supposed necessity or non-necessity of direct instrumental or other interference.

In 1	case the child	was delivered	by the	Long forceps.
In 3	"	"	"	Short forceps.
In 1	"	"	"	Evisceration.
In 2	"	"	"	Decapitation.
In 3	"	"	"	Simple traction.
In 40	"	"	"	Turning.
In 66	"	"	"	Natural pains.
Total, 116				

In the remaining 25 cases, the manner of delivery is not specified.

5. *Number of Children Lost and Saved.*—In 113 instances in the Table, the result as regards the life or death of the child is stated.

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[1] Clinical Midwifery, p. 142, &c.

[2] Pratique des Accouchemens, tom. ii. p. 415.

[3] L'Art des Accouchemens, Paris, 1771, p. 367.

In 1 (No. 10) of the 113 cases, it was malformed (anencephalous) and incapable of sustaining extra-uterine life, and in 6 others it was putrid, or had died before labor commenced. The following statement shows the result as respects the remaining 106 cases:

In 73 cases the infant was born *dead*.
In 33 " the infant was born *alive*.

According to these data, nearly 1 out of every 3 children survived;—or 31 per cent. of the children were saved, and 69 per cent. of them were lost. I shall again have occasion to recur to this topic in the sequel of the memoir.

SECTION V.—DEGREE OF HEMORRHAGE BEFORE THE SEPARATION OF THE PLACENTA, ITS ABSENCE AND EXCEPTION TO THE RULE, DEGREE OF HEMORRHAGE AFTER THE COMPLETE SEPARATION OF THE PLACENTA, ITS PRESENCE THE EXCEPTION TO THE RULE: PROPORTION OF CASES: NO RELATION BETWEEN THE EXTENT OF THE HEMORRHAGE AND THE DURATION OF INTERVAL BETWEEN THE DETACHMENT OF THE PLACENTA AND THE BIRTH OF THE CHILD.

Out of the 141 cases included in the preceding Table (Sect. III.), we have returns in 111 instances regarding the extent of the hemorrhage that was present, previously to the detachment and expulsion of the placenta. The preceding flooding is reported as

Great	in 72 cases.
Considerable	in 24 "
Slight	in 8 "
Little or none	in 7 "
Total,	111

The seven cases in which there occurred little or no hemorrhage during and anterior to the disjunction of the placenta, are those entered in the Table as Nos. 5, 11, 14, 28, 34, 117, and 138. Mercier has devoted a special essay[1] to the consideration of such exceptional instances to the general rule of flooding occurring as an "unavoidable" symptom in placental presentations. "The hemorrhage," observes Caseaux,[2] "which they have generally considered as *inevitable* in these cases (placental presentations) may, however, not show itself even during the progress of labor, and the dilatation of the cervix uteri may be effected without there escaping one drop of blood."

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[1] "Les Accouchemens ou le Placenta se trouve opposé, sur le col de la matrice, sont-ils constamment accompagnés de l'hemorrhagie?"—Journal de Médecine, vol. xlv. p. 305.

[2] Traité de l'Art des Accouchemens, 1841, p. 559.

Caseaux afterwards adverts to the opinions which Walter, Moreau, and others have offered in explanation of this exception.[1] The most rational idea seems to be, that in such cases the child has been dead for some time, and the utero-placental circulation in consequence arrested previously to the supervention of parturition.

But in relation to the objects of our essay, it is a much more interesting and important subject for us to inquire into the degree of hemorrhage *after*—than the degree of hemorrhage *before*, the complete separation of the placenta.

"The great and excessive losses of blood," states Mauriceau, in one of his aphorisms,[2] "which happen sometimes to the pregnant woman, proceed almost always from the detachment, in *whole* or in part, of the after-birth from the uterus; and these kinds of losses of blood never cease entirely till the female is delivered."

In criticising this aphorism, Levret observes—"The first part of this statement is, in general, but too true, but the second part is not so constant as Mauriceau gives it. For the daily practice of accoucheurs shows, that there are occasionally women attacked with great hemorrhage, in consequence of partial separation of the placenta, who nevertheless arrive at the natural period of delivery; thus the word *never* is too positive, as it does not allow of any exception, and it can only apply to those cases in which the separation of the placenta is *complete*, and not to those where it is only partially detached."[3]

Levret elsewhere[4] remarks in his essay on placenta prævia—"Daily practice teaches us that the placenta is never detached spontaneously, without the contraction of the part where it was affixed, and without the detachment of this vascular mass, whether *complete* or partial—being followed by discharge of blood."

The allegations made by Mauriceau and Levret, regarding the continuance of hemorrhage after *total* separation of the placenta, and I might quote similar averments, if necessary, from later authors, are perhaps such as the mind might be inclined to draw, from reasoning upon the subject of complete detachment of the mass. But if we turn from theory to fact—and from preconceived opinions to careful observations, we shall find the above statements perfectly and directly contradicted by the results of practical experience. For I believe that the data which I have collected for the present paper are amply sufficient to establish, as a great physiological and practical fact—that when the placenta, in cases of unavoidable hemorrhage, is once *completely* detached from its connections with the interior of the uterus, the accompanying flooding in general entirely ceases, or becomes quite moderate and inconsiderable in quantity. The cases adduced in the Table (Sect. III.), afford the strongest possible evidence in favor of the truth of this important principle. A slight analysis of them, in reference to this point, will sufficiently demonstrate our proposition.

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[1] See also Velpeau's Traité Complet des Accouchemens, vol. i. p. 356, and vol. ii. p. 81.

[2] Traité des Maladies des Femmes Grosses, &c., tom. i. p. 534, aphor. 44.

[3] L'Art des Accouchemens, &c., p. 395.

[4] Ibid. p. 347.

From the nature of the *Third* Division of the Table of Cases, including, as it does, those instances in which the expulsion of the placenta was immediately, or almost immediately, followed by the birth of the infant, we can, from this section of our data, expect few or no decided returns in reference to the degree of hemorrhage existing after the total detachment of the placental mass. In the two or three cases, however, of this division, in which the complete detachment of the organ occurred some time before its complete expulsion, the attendant hemorrhage was observed immediately to cease. Thus, in reference to two instances (Nos. 95 and 96), which occurred in the practice of Mr. Easton of Glasgow, it is stated in the notes of them with which I have been favored, that though in both the placenta was only expelled immediately before the child, yet it had been previously separated—in one above an hour—and in the other for several hours, and in neither of the mothers did any hemorrhage occur after the placenta was wholly detached from the uterine surface. In both instances the placentæ were originally affixed close to the os uteri—but not over it—and were detached early in the labor.

In the 111 remaining instances, the facts in regard to the existence or non-existence of hemorrhage during the interval between the detachment or expulsion of the placenta and the birth of the child stand as follows: In 39 out of the 111 cases, the absence or presence of hemorrhage after the expulsion of the placenta is not stated or alluded to by the reporters; but it is evident from the other circumstances which they describe, that in most of these cases there could have been no serious, if indeed any, extent of flooding, because the woman was allowed to remain undelivered, in many of them, for a considerable time after the placenta was separated—a state of matters which would not have been permitted if there had been any degree of discharge calling for the immediate delivery of the patient. Three out of these 39 mothers died—one from puerperal fever (Table, No. 4); a second (No. 137), apparently from post-partum hemorrhage;—the cause of death in the other case (No. 104) is not stated.

In 70 of the 111 cases, the existence and degree of hemorrhage, after the complete separation of the placenta, is distinctly stated, and may be tabulated as follows:

In 44	cases	the hemorrhage	was	completely arrested.
10	"	"	"	very slight, or almost none.
7	"	"	"	inconsiderable.
1	"	"		ceased.
1	"	"	"	much diminished.
1	"	"	"	considerable.
1	"	"	"	"a good deal."
5	"	"	"	profuse.
Total, 70				

It thus appears, that after the complete detachment of the placenta, the hemorrhage was totally arrested in a large majority of the cases; that it was not alarming in its extent in a great proportion of the remaining instances; and that in 5 only out of the 70—or rather in 5 only out of the 111 labors—did it continue so profuse under the circumstances, as to be considered alarming by the attendant, or in such excess as to require special notice in their reports.

Hence in 1 only out of every 22 labors does there appear to have been a continuance of hemorrhage to a great or profuse degree after the placenta was detached. One of the five mothers died (see Dr. Fraser's case in Section VI.) The other four all recovered.

But it may be proper to consider more at length the five cases in which the hemorrhage is stated to have gone on to a profuse extent, after the separation of the placenta, in order to judge better of the circumstances which may lead to its continuance in other instances.

First of all, however, it seems necessary to remark, in regard to the alleged continuance of the hemorrhage after the entire separation of the placenta, that the observation itself—simple and easy as it may appear—is one which is most undoubtedly liable to several sources of fallacy. Some of the authors who have described cases of expulsion of the placenta before the child, and not a few of the medical gentlemen who have communicated to me instances of the kind, have expressed the surprise which they felt at the flooding suddenly ceasing, upon the separation of the placental mass, in contradistinction to what their preconceived opinions had led them to expect. Any degree of incaution in the observation of the case might thus easily lead the medical attendant to suppose, that the blood effused externally, or lying in the vagina, was the result of the *continuance* of the hemorrhage subsequently to the total disjunction of the placenta, whilst in reality it might have been the result of the degree of flooding existing antecedently to that event, that is, whilst the placenta was still only partially detached. The blood *already* discharged might, in other words, be readily mistaken for blood in the act of *being* discharged. I am the more inclined to insist upon this source of error, in consequence of the strong fact, that out of all the First Division of cases in our Table—forty-seven in number—where there was a *long* interval between the expulsion of the placenta and the birth of the child, and, consequently, ample time allowed to confirm or correct any observation upon the degree of existing hemorrhage, in not one single instance is the flooding after the complete placental detachment alleged to have been profuse, or even considerable in its extent. Again, if there had been going on any internal accumulation of blood in the uterine cavity, or rather between the membranes and the uterus, during the period of the *partial* separation of the placenta, and before its complete detachment, the escape of this blood after the expulsion of the placenta might lead to the same error. Another occasional source of fallacy may consist in this, that the membranes may become ruptured by the same pain which expels the placenta through the os uteri or vagina, or they may burst during a subsequent uterine contraction, and the sudden gush of escaping liquor amnii, when mixed with the effused blood, might be readily mistaken for a pure hemorrhagic discharge.

Of the five cases in which the hemorrhage is alleged to have continued to a considerable or great degree after the detachment of the placenta, one affords an illustration of this last remark. I quote it from Lamotte.

CASE IX.—*Hemorrhage, with the placenta expelled from the vagina; excessive discharge; turning; infant and mother recovered.*—Lamotte was summoned to a woman who had been in labor from the previous day, and who had been losing blood for about two hours. "I went immediately," to adopt his own narrative, "though it was a good league (*grande lieue*) out of town. As I entered the court, several women came out with frightful shrieks, indicating to me, better than they could tell me, the extreme danger of my poor patient. I instantly descended from my horse, and hurried to where she was. I found that the after-birth had just been pushed out of the vagina by the last pain, and the discharge of blood had come in such abundance, as to have imparted that terrible fright to the bystanders, that had made them utter this piercing cry. I hastened to pull away the after-birth, glided my hand into the uterus, seized the feet of the infant, drew them into the passage, and accomplished the delivery in an instant. The infant was sufficiently alive to be baptized, but died soon after. The mother recovered in a sufficiently brief period, notwithstanding the fearful loss of blood." In some remarks which Lamotte offers upon this case, he observes, that he judged the membranes to have been entire from the surprising evacuation that followed the placenta when he drew it out, and which could not have been all blood, as it came away with much greater violence than it did previously, and the woman could not have borne the loss of such a quantity of blood without sinking. "But I am persuaded," he adds, "that the waters escaping from the membranes in which they were contained, became mixed with the blood effused from the vessels, the midwife having informed me, that the waters were ready to burst, when the accident (the expulsion of the placenta) happened, and they flowed out from my tearing the bag, in separating the placenta."[1]

In the above case of Lamotte's, the evidence of a continuance of true hemorrhage after the detachment of the placenta is by no means decisive; but we have placed it in that category in order to avoid the fear of error. The continuance of hemorrhage under the same circumstances is probably better marked in the four following cases. For the first two I am indebted to Dr. Wilson of Glasgow, in whose practice they occurred. I shall give them in his own words.

Case X.—*Expulsion of placenta; hemorrhage; turning.*—"*May* 7, 1821.—Mrs. G., the mother of a large family, was seized, near the termination of pregnancy, with profuse flooding. Dr. M. was sent for. He found the placenta presenting; it very soon came away, the discharge continuing. I was called in, and such was the profusion of the discharge and state of exhaustion, that turning was instantly resorted to. The child was dead—there was no discharge after delivery. The recovery was tedious, but at length complete."[2]

Case XI.—*Placenta lying with its* fœtal *surface over the os uteri; hemorrhage; turning.*—"*April* 17, 1833. This evening I was sent for by Dr. Cunningham to see Mrs. ——, Portugal Street, who was flooding, and had been for several hours. The placenta was found lying loose over the os uteri, with the *fœtal surface downward;* the finger at once touched the origin of the umbilical cord. The placenta was turned aside, the feet laid hold of, and a dead child extracted. She made a good recovery."[3]

This last case is, so far as I know, unique, in the circumstance of the placenta being found quite inverted over the os uteri, or with its fœtal, instead of its maternal surface, lying in contact with that part. It may probably so far be regarded as a proof that in this instance there was a cause for the hemorrhage continuing, in a most unusual and extreme degree of atony or relaxation of the uterus—a state which would seem necessary in order to admit of the possibility of the inversion of the placental mass. In the two following cases of Mr. Barlow and Mr. Bailey, we have the hemorrhage persisting under different conditions, viz., the patient being in the upright posture at the moment of the separation and expulsion of the placenta; and besides, having in the first of them that disposition of the uterus, whatever its special nature may be, which gives rise to post-partum hemorrhage.

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[1] Table, No. 110.
[2] Table, No. 128.
[3] Table, No. 129.

CASE XII.—*Expulsion of the placenta preceding the delivery of the child; hemorrhage both after the expulsion and delivery.*—A woman in the last month of her second pregnancy suffered from uterine pains and a slight discharge of blood at intervals. The hemorrhage ceased when the horizontal position, &c., were adopted. Next morning Mr. Barlow was summoned to see her, and found her sitting on a chair in a state of great alarm, a profuse discharge of blood succeeding every pain. "On requesting her," he continues, "to be conveyed to bed, she attempted to walk up stairs, and before she could reach the bed, a violent pain seized her, which instantly expelled the placenta, and disparted the funis about six inches from the child's navel. A great effusion of blood followed, and the woman fainted ere she could be laid down on the bed." Mr. Barlow passed up his hand into the uterus, found the os uteri in a lax and dilated state, with the shoulder presenting, laid hold of the feet, and accomplished the delivery of the child, by turning, in a few minutes. "The child appeared feeble, but soon recovered on being placed in a warm bath. A considerable hemorrhage," he adds, "followed the birth; on perceiving which, I returned my hand into the uterus, and by keeping it moving therein for a time, its contractions were renewed, and the hand was then withdrawn, and the flooding abated, and though the woman appeared much reduced through the loss of blood, she soon recovered."[1]

CASE XIII.—*Unavoidable hemorrhage supervening during exertion; sudden expulsion of the placenta; mother and child saved.*—The case occurred to Mr. Bailey of Thetford. A woman, aged 32, three weeks before the time of her expected fourth confinement, when exerting herself by washing, &c., was seized with a sudden and violent flooding, accompanied by an extreme degree of bearing down, which, to make use of her own expression, felt "as if the head of the child was in the birth." In the act of stepping upon the bed, she was taken with a pain, during which the placenta was forcibly expelled, and was suspended between the thighs by the funis. At this moment a deluge of blood followed, and she sank down senseless upon the bed, to all appearance dead, the pulse being imperceptible, and the skin covered with a cold clammy sweat. The os uteri was found to be completely dilated, the passages were well relaxed, and the head presented in the first position. Turning was adopted and easily accomplished. During the operation, "the hemorrhage was alarming, and large coagula were present in the uterus, which were expelled as soon as the child was born. When the uterus was excited to contraction, the hemorrhage ceased. The child at first appeared to be still-born, but was restored by the proper means. Both the mother and the infant did well."[1]

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[1] Table, No. 56.

In relation to the last two cases of alleged hemorrhage after the placenta was totally separated, it deserves to be specially held in view that, as already alluded to, in both cases the patients at the time at which the placenta was detached, were in the upright position—a circumstance which is well known to be a very certain cause of post-partum hemorrhage when there is any tendency to that condition; in both patients the cervix uteri was very relaxed, the introduction of the hand in the operation of turning being performed with great ease; in both, the *complete* separation of the placenta must have occurred a very short time before delivery, as each of the children was born alive; and in the last patient (Mr. Bailey's), the discharge of blood which took place after the expulsion of the placenta, must have been to some extent the result of a *previous* internal accumulation occurring during the partial separation of the placental mass, as the blood itself had had time to coagulate. This internal hemorrhage and accumulation of blood probably occurred also in the remaining case upon our list of hemorrhage after the complete separation. For the details of it, see Dr. Fraser's case in the next section and the remarks upon it.

That the extent of the hemorrhage has no direct relation to the extent of the interval between the expulsion of the placenta and the delivery of the child is amply attested by the following facts: All the reputed instances of hemorrhage after the complete detachment of the placenta, have occurred in cases where the interval between the birth of the placenta and that of the child, was short and uncertain; or, in other words, among the patients included in the Second and Fourth Divisions of the General Table. Among the cases belonging to the First Division of the Table, in which the interval between the detachment of the placenta and the delivery of the child was longer, and varied from ten minutes to ten hours, and where, consequently, there was more time to observe any degree of flooding that might exist, *in not a single instance was the hemorrhage observed to be great, or even considerable in extent.* On the contrary, in one only of the forty-seven cases belonging to this division, was it in any unusual degree;[1] in nine, it is reported as "almost none," "trifling," or "slight," or "very slight;" and in twenty-three cases it was totally and completely arrested. In nine, the degree of it, if any, is not stated.

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[1] Table, No. 102.

I shall have occasion to revert to the practical bearing and importance of these facts in a future section of the essay.

SECTION VI.—COMPARATIVE MORTALITY IN PLACENTA PRÆVIA, FROM TURNING, ETC., AND FROM EXPULSION OR EXTRACTION OF THE PLACENTA: TEN FATAL CASES AFTER SPONTANEOUS EXPULSION: DETAILS OF EACH CASE; SEVEN OF THEM INDEPENDENT OF THE SEPARATION OF THE PLACENTA: NATURE OF THE THREE REMAINING CASES.

In common cases of placental presentation we have already found, from ample statistical data, that the average mortality to the mother is about 1 in 3 (see Section I.) Among the 141 cases of expulsion and extraction of the placenta, which we have collated into the Table (Sect. III.), 10 mothers died, or the average mortality to the mother was 1 in 14. The difference between the two sets of cases, namely, 1st. Those terminated according to the present recognized rules of midwifery; and 2d. Those terminated by the spontaneous expulsion or extraction of the placenta—is sufficiently striking when thus simply stated. The contrast may be more easily appreciated if we tabulate the results in the following manner:

Mode of Management.		Number of Cases.	Number of Maternal Deaths.	Proportion of Maternal Deaths.
Cases treated by extracting the child before the placenta, rupture of the membranes, &c.[1]	. . . .	654[2]	180	1 in $3\frac{6}{10}$
Cases in which the placenta was expelled or removed before the child,	. . . .	141	10	1 in 14

The evidence in favor of the safety of the termination of such cases by the expulsion or extraction of the placenta before the child, will become still more striking if we turn our attention specially to the ten fatal cases themselves; for we shall find that the fatal result in few, if any, of these cases, can be directly traced and ascribed to the circumstance of the placenta being completely separated, or to any possible consequence arising from that separation. An examination of these ten fatal cases in detail will sufficiently prove this remark.

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[1] Case 43. The patient lost 8 lbs. of blood in three days, and "about a pound" after the expulsion of the placenta.

[2] We have here substituted the corrected statistics, as given in the previous table, p. 601.—(ED.)

Four of the ten mothers died several days subsequently to delivery. I shall first describe these four cases, so far as I have notes of them, to show more clearly the immediate cause of death in each.

CASE XIV.—*Placenta expelled an hour before the child; patient died on 10th day, after having been up, and exposed to excitement and injury, on the 9th.*—The case occurred in the practice of Mr. Hay of Glasgow. It was a first pregnancy, and the patient had arrived at the full period. Before the separation of the placenta, the hemorrhage was excessive, and she was quite sunk and exhausted. Very little blood was lost after the placenta had come away, though the infant was not born for an hour. It was expelled by the natural pains, and was still-born. The following is Mr. Hay's note on the case: "This patient seemed to sink from excitement. She and her husband quarrelled on the 9th day after the birth of the child, and on the 10th she died." Dr. Smith of Glasgow,[1] who has reported this, with various other cases to me, states more explicitly, that "she left her bed and fought with her husband till perfectly exhausted, from which state she never recovered."[2]

The fatal result in this case does not require a word of comment; the fact of the woman being able to leave her bed, and to act in the way described, is sufficient proof that she was in a fair way of recovery, and that she would in all likelihood have done well, had it not been for her own indiscretion. In the instance which I have next to quote, the fatal event is also ascribed by the reporter to imprudence on the part of the patient, and, at all events, the degree of hemorrhage was such as in no way to endanger her life.

CASE XV.—*Spontaneous expulsion of the placenta; little or no hemorrhage either before or after its separation; death on the 7th day from "purpura alba."*—"About 16 years ago, I was called," says Walter,[3] "to the assistance of the wife of the former Castellano of the Royal Academy of Treptow. On arriving, I found her in bed;

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[1] I am happy in having this opportunity of offering my best thanks to Dr. Smith for the very great zeal and kindness with which he has assisted me in Glasgow, in the collection of cases for the present memoir.

[2] Table, No. 32.

[3] De Morbis Peritonæi et Apoplexiâ, Berlin, 1785, p. 33.

labor had commenced at 4 A.M., seven hours before; the placenta was already separated, and had fallen to the ground; it was still attached to the infant by the cord. I was astonished at this very rare phenomenon, which at that time I could not explain, as I did not then understand the structure of the uterus as I now do. As it was a cross-presentation, I had recourse to turning, and within a few moments I delivered the woman of a dead child. I can affirm most positively," the author adds, "that before my arrival, and during the labor, the woman did not lose above two ounces of blood. She did well till the third day, but an improper and contentious mode of living (*inordinata atque contentiosa vivendi ratio*) was the cause of her being seized with '*purpura alba*,'[1] of which she died on the 7th day after delivery."[2]

In two of the fatal cases the mothers died from puerperal fever or peritonitis. These two instances have been recorded by Dr. Merriman and M. Mercier, and I shall detail them, as nearly as is consistent with brevity, in the authors' own words.

CASE XVI.—*Placenta expelled long before the child, &c.*—"I was once," Dr. Merriman states,[3] "consulted by a very careful and judicious practitioner, respecting a woman, who, when I first saw her, was rapidly sinking under puerperal fever. In this case the placenta was expelled many hours before the child was born, and no extraordinary means were used to expedite the delivery of the child; a physician accoucheur, who was consulted upon the occasion, having deemed it more prudent to leave the case to nature. The fatal event, however," Dr. Merriman unadvisedly adds, "would lead one to doubt whether it was wise, under such circumstances, to decline the interference of art."[4]

CASE XVII.—*No hemorrhage with the first part of the labor; vomiting; fever; placenta spontaneously expelled; child delivered with forceps; mother died of peritonitis nine days after delivery.*—For upwards of two days before Mercier saw the patient, she had been attended by a midwife, and latterly by a medical man, who, being called in during the course of the second day, and finding the woman feverish, had bled her largely. The bleeding had lessened the pains, which did not return till that evening. The patient had not suffered from any discharge of blood from the uterus, but she felt extremely uneasy; was not able to rest in bed; and rejected by vomiting everything that was given to her. About two o'clock on the morning of the third day, while she was walking about with a person supporting her, a strong pain expelled the placenta, which fell to the ground, followed immediately by the escape of the waters. The embarrassment of the midwife was extreme. She divided, however, the cord, and waited the arrival of M. Mercier, whom she had immediately summoned. The pains again ceased, and the woman, having been put to bed, got a little sleep. "The placenta," says M. Mercier, "was shown me, of a small size, and covered with dust. The cord was implanted in its middle, and about half a yard of it was attached. Only a few spoonfuls of blood had been lost in addition to the small quantity that had escaped from the cord when it was divided." In consequence of it being impossible to excite the uterus to sufficient action, it became necessary to terminate the labor by the forceps. This was accomplished easily. The child did not appear to be at the full time. Its extraction was followed with a very moderate effusion of blood, which scarcely penetrated a cloth folded four times. "This small quantity," observes Mercier, "joined to what had accompanied the falling of the placenta, did not exceed the loss of blood in ordinary labors."[1] An hour after delivery there was sanguinolent oozing, which soon ceased. Subsequently, however, the woman was attacked with peritonitis, and died of this affection nine days after delivery.[2]

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[1] Or "miliaria," a disease which, under the old "heating" method of treating puerperal women, was formerly extremely fatal. The Stockholm Academy proposed in 1769 as a prize question: "How the different kinds of miliary fever should be prevented and cured, as well in lying-in women as in others." The successful author, Schultz, showed strongly the necessity of adopting a cooling regimen. Dr. Whyte's excellent Essay on Miliary Fever (Treatise on Lying-in Women, pp. 25–55), did much to banish the disease from English practice.

[2] Table, No. 9.

[3] Synopsis of Difficult Parturition, p. 126.

[4] Table, No. 4.

Besides the four instances of death which we have just described, at periods more or less distant from delivery, two others of the ten fatal cases occurred within a very short time after the birth of the infant, and a third (Dr. Ramsbotham's) appears also to come under this head. Yet, as will appear from the details which we shall now give, the death in these cases was not apparently in consequence of any hemorrhage or other cause arising from the complete separation of the placenta, the hemorrhage in all of them having ceased when this separation took place. All the three mothers were delivered by operative means.

CASE XVIII.—*Severe hemorrhage and presentation of the arm; child eviscerated to permit delivery; placenta detached during the operation; no hemorrhage from its detachment; mother died.*—The case occurred in the practice of Dr. Ramsbotham, to whose kindness I am indebted for the following details. On December 24, 1839, Dr. Ramsbotham was called to see Mrs. E., who was gone 6½ months in her second pregnancy. She was in a state of exhaustion from severe hemorrhage, having lost about two quarts of blood. "The previous attendant," to give Dr. Ramsbotham's own words, "ruptured the membranes at 7 or 8 A. M., after which there was no further hemorrhage. The arm now came down, which he took off. I delivered her with great difficulty, owing to the undeveloped state of the cervix, at 4½ P. M. I could not get the hand into the uterus, but managed to perforate the chest by a blunt hook, and to extract many of the viscera. In trying to perforate the chest, I hooked down the placenta, a part of which was hanging loose in the vagina; *still there was no flooding*."[1]

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[1] Journal Ancien de Médecine, tom. xlv.

[2] Table, No. 138.

CASE XIX.—*Profuse and exhausting hemorrhage, terminated by expulsion of the placenta; turning; immediate death.*—The woman was a patient of Mr. Wood's of Manchester. She had borne five children previously, and had reached the eighth month of her sixth pregnancy. The hemorrhage was very profuse previous to the expulsion of the placenta, and had caused great exhaustion; after this it completely ceased. Turning was immediately had recourse to, and a dead infant was extracted by the feet. She died immediately after her delivery.[2]

CASE XX.—*Head presentation and unavoidable hemorrhage, placenta completely detached in the operation of turning; previous exhaustion; death.*—The woman (a patient of Mr. Tindal's of Glasgow) was at the end of her tenth pregnancy. The head of the child presented. There was fearful hemorrhage before the placenta was expelled, but none after. Delivery was effected by turning, a few minutes after the escape of the placenta. The mother died in half an hour. "In this case," Dr. Smith observes, in the note which I have received with it, "the patient was exhausted previous to turning, and that operation was adopted to have delivery effected before she died. The placenta was accidentally separated during the course of the operation, and Mr. Tindal was perfectly certain that the hemorrhage ceased from that moment."[3]

In the three remaining cases of maternal deaths, the fatal occurrence took place during labor, or immediately subsequent to delivery. Of two of the three cases I have only very imperfect notes, which I give, such as they are, before offering any comment on them.

CASE XXI.—"*Flooding with one arm and part of the placenta*

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[1] Table, No. 60. [2] Table, No. 81. [3] Table, No. 62.

slipped down below the os uteri internum;" turning; post-partum hemorrhage; death.—October the 17th, 1731, about ten o'clock in the morning, Mr. Giffard was sent for to the wife of a printer, near White Fryars; "she had been seized," to use his own words, "about an hour before with a violent flooding, and when I came, I found she had lost a large quantity of blood; and I was told she was in about the seventh month of her reckoning. Upon touching, I found one arm of the child slipped out beyond the os internum, as also a large part of the placenta; wherefore, I gave it as my opinion, that she ought to be immediately delivered—letting her husband and others know the great danger she was in. As it was entirely left to my conduct, I immediately passed up my hand, well greased, into the vagina, and so on by the side of the shoulder into the uterus, where I met with the remaining part of the placenta, wholly separated from the uterus. I now passed my hand between the placenta and the body of the child, and soon met with one foot, which I drew out beyond the labia pudendi, and then taking hold of it with a soft cloth, with a little difficulty, I brought out the hip and the body almost to the shoulders, when, finding it stopped at the head, I passed in my hand, and brought down one arm, the other not being slipped up again from its first falling down. I then endeavored to draw out the head, but it would not readily follow; whereupon I passed up one finger into the child's mouth, and strove, by pressing upon the lower jaw, to bring the face forwards, whilst at the same time I pulled above at the shoulders; but as it was closely locked between the bones that form the lower part of the pelvis, I had no small trouble in bringing it out; however, at last I finished the delivery by bringing away the placenta, which, being before loosened from every part of the uterus, readily followed. I was then in hopes we had surmounted our greatest difficulties, and that the flooding would have stopped; but, to my great surprise, she continued still draining. I therefore again gently passed up my hand, believing that either some part of the placenta was torn off and left, or else that some coagulated blood kept the womb distended; but I could not meet with any part of the placenta, or any clots of blood. I then ordered cloths dipped in vinegar to be applied close to the parts, and what else I thought necessary, yet, notwithstanding all my endeavors to save her, *amisit cum sanguine vitam.*"[1]

CASE XXII.—*Unavoidable hemorrhage, with apparent hydrothorax and cardiac disease; death speedily after the expulsion of the placenta; living child subsequently extracted.*—The case occurred to Dr. Fraser of Aberdeen. It was the patient's sixth pregnancy. Labor came on at the eighth month. Very moderate hemorrhage had been going on for two hours, when the placenta became very extensively detached by one uterine contraction, and the mass of it was found lying in the vagina. "The accompanying hemorrhage," Dr. Fraser states, "was great, and, without convulsions, she expired in two minutes." A few minutes afterwards, Dr. Fraser passed his hand into the uterus, and extracted the child alive. "A post-mortem examination," Dr. Fraser adds, "was not allowed, but from a combination of marked symptoms, I have a strong conviction that she labored under hydrothorax, depending on a diseased state of the heart."[1]

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[1] Table, No. 137.

CASE XXIII.—*Fatal detachment of the placenta.*—In speaking of the complete separation of the placenta in unavoidable hemorrhage, Dr. Collins states, "Dr. Clarke informed me, that he had met with one case of total separation; the patient was dying before he reached the house."[2] By a private note from Dr. Collins, I am informed that he knows no more of the case than what is stated in the above sentence, and that in consequence of Dr. Clarke's death, it is now impossible to obtain more details.[3]

GENERAL REMARKS ON THE TEN FATAL CASES.

In all the first seven of the preceding fatal cases, the separation of the placenta, or the degree of hemorrhage after its detachment, had evidently little or no connection with the death of the mothers. In the first case, No. XIV. (Mr. Hay's), the blood lost during the hour that elapsed between the expulsion of the placenta and birth of the child, is averred to have been "very little;" in the second case (Walter's), not more than two ounces of blood escaped in all during the whole labor; in the third case (Dr. Merriman's), the hemorrhage, after the expulsion of the placenta, was, in all probability, inconsiderable, or altogether arrested, as it was not deemed necessary to expedite delivery, though the placenta was thrown off several hours before the infant was born; in the fourth case, the narrator (Mercier) distinctly attests, that the whole loss of blood was not greater than with an ordinary labor; in the fifth case, there was, to use Dr. Ramsbotham's own expression, "no flooding" after the placenta was detached, and there had been none for some time previously; in the sixth case (Mr. Wood's), the hemorrhage completely ceased after the total separation of the placenta; and in the seventh case (Mr. Tindal's) the same fact was observed. In these two last cases, though both patients sunk principally from the effects of hemorrhage, yet in both of them that hemorrhage had occurred antecedently to the detachment of the placenta; the mischief, in so far as the flooding was concerned, was done before that detachment took place; in neither of them did the peculiarity of the complete separation of the placenta occur until the case was already so far hopeless, from the antecedent discharge, and, indeed, so far from being injurious, the separation of the placental mass would, on the very contrary, by its immediately arresting flooding, seem to have been salutary, though unfortunately in each too late to save.

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[1] Table, No. 49. [2] Practical Treatise on Midwifery, p. 91. [3] Table, No. 105.

On the other hand, there occurred, in the course of these seven fatal cases, circumstances and complications amply adequate to account for the deaths of the mothers, quite independently of the separation of the placenta, or of any flooding or other possible accident connected with that separation. In Mr. Hay's case, the patient's death was evidently the result of the strong excitement and injuries to which she was subjected on the ninth day after delivery. Walter, as we have seen, attributes the attack of the disease ("purpura alba," or miliary fever), of which his patient died, to her own indiscretion. In Dr. Merriman's and Mercier's cases, puerperal fever and peritonitis were the causes of the fatal issue—a disease that too often occurs, independently of any morbid complication whatever, during labor. Mercier's patient had, though there was no accompanying flooding, become so exhausted, and the expulsive powers so inefficient, by the time he saw her, that instrumental delivery was deemed necessary. In Dr. Ramsbotham's case (an arm presentation), the child was delivered by evisceration of the chest and abdomen, an operation in itself sufficiently dangerous, and never employed except when even turning is impossible; and, in the present instance, it had its difficulties much enhanced by the rigid state of the os uteri. Lastly, in Mr. Tindal's and Mr. Wood's cases, extraction of the infants by version was had recourse to, at a time when the mothers were already greatly exhausted, and little able to withstand the additional shock of such an operation. Thus, in two of the fatal cases (Mr. Hay's and Dr. Merriman's), the delivery was effected by the natural pains; and the cause of death in each was apparently independent of any circumstances connected with the detachment of the placenta. In five of the cases (Walter's, Mercier's, Dr. Ramsbotham's, Mr. Tindal's, and Mr. Wood's), the delivery was accomplished by such operative means as are in themselves always more or less perilous to the life of the mother, particularly when, as in some of them, she had already become prostrated and exhausted by the time they were adopted.

For the above reasons we are, I believe, quite entitled to reject, in regard to the first seven fatal cases, the idea of the death of the mothers being caused by the total separation of the placenta, or by its mediate or immediate consequences.

If this be granted—and we subtract on this ground the first seven fatal cases—we have only, out of 141 deliveries, three maternal deaths left, which can be at all ascribable, directly or indirectly, to the complete detachment of the placenta, and its results. This would give a mortality of only one in about every forty-seven mothers from this complication during labor, in placental presentations; a proportion which, it must be confessed, is surprisingly small.

But it seems, indeed, even more than doubtful, whether all the three remaining fatal cases (Mr. Giffard's, Dr. Fraser's, and Dr. Clarke's), should be allowed to have been instances in which the death of the mothers was attributable simply to complete separation of the placenta, and its effects.

Mr. Giffard's patient died, if we may judge from his own account, of post-partum hemorrhage—a complication which is known to be a special source of danger to the mother after placental presentations, under all modes of management.[1] The hemorrhage was here probably the result of the injury and laceration of the vascular and imperfectly dilated neck of the uterus, in consequence of the force employed in the operation of the extraction of the shoulders and head of the infant. This view would seem to be so far corroborated by the fact, that the post-partum discharge was not connected with the presence of any clots of blood in the uterus, and hence, was not the effect of atony of the body or fundus of the organ. At all events, the fatal hemorrhage was not, in Mr. Giffard's patient, in any apparent way, dependent upon the *previous* complete detachment of the placenta during the labor; and hence, we might probably be entitled to remove this case also, like the preceding seven, from the list of those in which the death of the mother could be attributed to the contingent separation and expulsion of the placenta.

In Dr. Fraser's case, the chest affection may have had a principal share in the sudden demise of the patient—the presence of heart disease (supposing such existed) predisposing, as is well known, the subjects of it to be greatly, and, in some instances, fatally, affected by any rapid losses of blood, and occasionally leading, as I have known in two instances, to sudden death, from the shock of the delivery, when the labor was in other respects quite natural. I would add, that the details which I have obtained through Dr. Fraser's kindness are not by any means perfectly decisive, as to the whole placenta being completely detached in this instance. The same remark may apply to the other remaining case of Dr. Clarke; if we may judge from the little information that we do possess in reference to it, and contrasting it with the results ascertained in other well-observed instances. The account of Dr. Clarke's patient is so brief and defective, as to furnish us with no data whatever as to the extent and nature of the accompanying hemorrhage, the existence or non-existence of any other complication, the delivery or not of the child, or the immediate cause of the fatal event to the mother.

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[1] See on this topic Dr. Hamilton's Practical Observations, second edition, p. 329. In speaking of placenta prævia, he states, in reference to flooding from the ruptured vessels of the neck of the uterus (the body and fundus of the organ being contracted), that for many years past he has been led to "dread this danger in every case where he has been obliged to force delivery in consequence of uterine hemorrhage."

SUMMARY OF RESULTS.

Our inquiry, so far as we have hitherto proceeded, seems legitimately to admit of the following deductions.

1. The complete separation and expulsion of the placenta before the child, in cases of unavoidable hemorrhage, is not so rare an occurrence as accoucheurs appear generally to believe.

2. It is not by any means so serious and dangerous a complication as might *à priori* be supposed.

3. In nineteen out of twenty cases in which it has happened, the attendant hemorrhage has either been at once altogether arrested, or it has become so much diminished as not to be afterwards alarming.

4. The presence or absence of flooding after the complete separation of the placenta, does not seem in any degree to be regulated by the duration of time intervening between the detachment of the placenta and the birth of the child.

5. In ten out of one hundred and forty-one cases, or in one out of fourteen, the mother died after the complete expulsion or extraction of the placenta before the child.

6. In seven or eight out of these ten casualties, the death of the mother seems to have had no connection with the complete detachment of the placenta, or with results arising directly from it, and if we do admit the three remaining cases, which are doubtful, as leading by this occurrence to a fatal termination, they would still only constitute a mortality from this complication, of three in one hundred and forty-one, or of about 1 in 47 cases.

7. On the other hand, under the present established rules of practice, one hundred and eighty mothers died in six hundred and fifty-four placental presentations, or nearly one in three.[1]

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[1] See corrected table, p. 601.—(Ed.)

II.[1]

SECTION VII.—MECHANISM BY WHICH HEMORRHAGE IS PREVENTED AFTER THE COMPLETE DETACHMENT OF THE PLACENTA; SOURCE OF THE DISCHARGE IN PARTIAL DETACHMENTS OF THE PLACENTA; MEANS BY WHICH THE HEMORRHAGE IS ARRESTED IN PLACENTAL PRESENTATIONS WHEN THE DETACHMENT IS PARTIAL: AND WHEN IT IS COMPLETE; ANALOGOUS CASES IN TWIN LABORS; ANALOGOUS PHENOMENA IN THE THIRD STAGE OF COMMON LABOR; MODES BY WHICH HEMORRHAGE IS PREVENTED FROM THE VASCULAR ORIFICES LEFT EXPOSED ON THE INTERIOR OF THE UTERUS.

In his *Outlines of Midwifery*, Professor A. Hamilton, after laying down the necessity of turning in placental presentations, observes, "In some instances, before the *orificium uteri* can be sufficiently opened to admit the hand of the operator to pass, the whole cake will actually be disengaged and protruded; but," he adds, "the separation and expulsion of the placenta previous to the birth of the child is, for the most part, fatal to the mother."[2]

"If the placenta," says Petit, when speaking of unavoidable hemorrhage, "is entirely separated, the death of both the mother and child is certain."[3]

"When the placental mass is thus expelled before the child, the hemorrhage," observes Carus, "must necessarily be so considerable, that both child and mother usually become a prey to death (*eine Beute des Todes.*)"[4]

These three quotations express what seems to be the general opinion of medical men in regard to the complication which we are considering. I have already, however, taken occasion to show that the very reverse of the above statements is more consonant with fact and experience, and that the complete disjunction and expulsion of the placenta before the child is an accident neither very fatal to the mother, nor very frequently followed by any great or perilous degree of hemorrhage. Let us now inquire if we can explain the mechanism by which it happens, that generally, after the complete separation of the placenta from the uterus has occurred, not only does no considerable degree of hemorrhage supervene, but the preceding and sometimes violent discharge is at once arrested, and thus the life of the mother preserved against the impending danger of frightful and fatal flooding. The investigation will, I believe, lead us to entertain more decided views, with regard to the propriety of the practice to which our present Essay points.

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[1] This part and the following were printed and privately distributed several years since, but have not hitherto been published.—(ED.)

[2] Outlines of Midwifery, p. 44.

[3] Traité des Maladies des Femmes, &c., tom. ii. p. 22.

[4] Lehrbuch der Gynæcologie, tom. ii. p. 442.

The explanation that would occur to most minds, on first thinking of the probable mode by which nature could arrest and prevent hemorrhage after the total separation of the placenta before the child, is that offered by Drs. Ramsbotham and Campbell. "I think," says Dr. Ramsbotham, "it may be satisfactorily explained how the woman's life is preserved. The head of the child is pushed down upon the os uteri, which suddenly gives way. Under its relaxation, the placenta is loosed from its previous attachment, and falls down before the head, which now comes into immediate contact with the bleeding vessels, and by mechanical compression closes their mouths; from this moment, therefore, the loss of blood is suspended, and the head is afterwards expelled by uterine action."[1] Dr. Ramsbotham here imagines that the head of the infant acts in such cases, as a compress or plug upon the open uterine orifices left by the separation of the placenta from the interior of the cervix.

"It may," remarks Dr. Campbell, "be presumed, that in these cases the fatal event must have been prevented by the quick descent, and consequent pressure, of the body of the child, upon the point whence the placenta had been detached."[2]

That the above opinion[3] of Drs. Ramsbotham and Campbell does not afford the correct explanation of the cessation of hemorrhage after the complete detachment of the placenta, appears to me to be evident from the following facts: 1st. That in some of those few instances in which the hemorrhage continued after the total separation of the placenta, the infant's head did present, but did not produce the result here attributed to it (see for instance Mr. Bailey's case, Sect. V.); and, 2d. On the other hand, in a considerable number of the instances that we have collected, and in which the placenta was entirely detached and expelled before the child, the hemorrhage totally ceased, although the portion of the child that presented against the cervix uteri was not the head, nor indeed any such part as could produce the required degree of pressure upon the open uterine orifices.

In two of the instances which I have myself seen, and already detailed in a previous section (Sect. II.), the shoulder and neck presented in one, and the arm of the child in another—parts which could not be applied as plugs in the way supposed by Dr. Ramsbotham and Dr. Campbell.

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[1] Observations in Midwifery, part ii. pp. 191, 192. [2] System of Midwifery, p. 369.

[3] Since the above was written, I find that Dr. Radford of Manchester has published the same opinion regarding the suppression of the hemorrhage as Drs. Ramsbotham and Campbell entertain. "On a complete separation of the placenta," he remarks, "the hemorrhage is immediately and completely suppressed, provided the uterus is in a condition to so far contract, as to force down the head, with the placenta, upon the uterine openings."—Provincial Medical and Surgical Journal for January 22, 1845.

In a considerable proportion of cases collated into our General Table, the child presented preternaturally with the foot, feet, arm, or shoulder. These parts are all of such a form and size, that they could not be applied as compresses, upon the part of the uterus which was exposed by the previous detachment of the placenta, and yet the hemorrhage appears to have been as *constantly* and as *completely* arrested in those instances when once the placenta was perfectly separated, as it was in cases in which the head and breech of the child came afterwards to press upon the cervix uteri. The following tabular view will demonstrate this important point, by showing the number of instances in which a lower or upper extremity came down after the expulsion of the placenta, and the degree of the hemorrhage that was observed to follow in these cases.

Degree of Hemorrhage after separation of Placenta in	Footling Presentations.	Arm or Shoulder Presentations.	Total.
Great,	—	1*	1
Considerable,	1†	—	1
Slight,	—	2	2
Little or none,	2	8	10
Not stated,	2	7	9
	5	18	23

These data prove, that "great" or "considerable" hemorrhage, after the expulsion or detachment of the placenta, is not more liable to occur, when the retained fœtus afterwards presents by an upper or lower extremity, than when it comes down with the head or breech upon the exposed surface of the cervix uteri. The proportion of cases in which the hemorrhage continues is not greater under the one set of presentations than it is under the others, as may be seen by comparing the present table with that previously given at p. 624. And these observations seem to me to afford amply sufficient grounds for rejecting the idea, that the prevention of the hemorrhage after the complete separation of the placenta is to be explained, by the presenting part of the infant coming down, and acting as a compress upon the exposed orifices of the uterine vessels. It may be an auxiliary, but it is not a primary or essential cause of the suppression of the hemorrhage. For the mechanism of the arrestment of the flooding, in cases in which the placenta happens to be completely detached, and the child left in utero, is, I believe, of a totally different kind. But, in order to understand it, let me first premise, that obstetric pathologists are in all probability incorrect in the rationale which they currently give of the immediate source of sanguineous discharge, in instances of flooding—whether accidental or unavoidable—from partial separation of the placenta.

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* See the details of this case already given and commented upon at p. 627.

† "About a pound." Case 43. See p. 626.

Anatomical source of the Hemorrhage, in Detachment of the Placenta.

From the time of Guillemeau downwards, accoucheurs seem to have generally believed, that in cases of unavoidable and accidental hemorrhage, arising from the greater or less separation of the placenta, the blood that is effused escapes from the vascular maternal orifices that are left uncovered, and exposed upon the internal surface of the uterus, when the placenta is detached from that surface. Thus Guillemeau, in writing on the subject, states:—"The surest and most proper mode of assisting a woman when the after-birth presents at the passages, is to deliver immediately (*delivrer soudainement*). This is the more necessary, from there being usually a constant flow of blood, owing to the *mouths of the veins that are situated in the walls of the uterus* (those, namely, to which the placenta was united) *being open*, and as the uterus contracts in order to expel the infant, it squeezes out the blood that is contained in these vessels, and which is attracted to them by the heat and pain."[1]

Mauriceau entertained precisely similar views. "When flooding," he observes, "happens to a woman truly conceived, at whatsoever time it be, it proceeds likewise from the opening of the vessels of the fund of the womb, caused by some blow, slip, or other hurt, and chiefly because the secundine in such cases, and sometimes in others, is separated in part, if not totally, from the inside of the bottom of the womb, to which it ought to adhere, that it might receive the mother's blood appointed for the infant's nurture, by which separation, it *leaves open all the orifices of the vessels where it was joined, and so follows a great flux of blood*, which never ceaseth (if so caused) till the woman be brought to bed: For the secundine being once loosened, although but part of it, never joins again to the womb to close those vessels, which can never shut till the womb hath voided all that it contained: For then, compressing and closing itself, and, as it were, entering within itself (as it happens presently after delivery), the orifices of the vessels are closed and stopped up by this contraction, whereby also this flooding ceaseth, which always continues as long as the womb is distended by the child, or anything else it contains, for the reason aforesaid."[2]

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[1] Guillemeau, Les Oeuvres de Chirurgie, p. 320.

[2] Mauriceau, Diseases of Women with Child and in Child-bed, p. 87 of Chamberlen's translation.

In another part of his work Mauriceau again states—"But the coming first of the burden is yet much more dangerous; for, besides that the children are ordinarily still-born, if they be not assisted in the very instant, the mother likewise is often in very great peril of her life, because of her great floodings which usually happen when it is loosened from the womb before its due time, *because it leaves all the orifices of the vessels open to which it did cleave*, whence flows incessantly blood, until the child be born."[1]

The opinions expressed in the above quotations from Guillemeau and Mauriceau have been generally adopted by obstetrical authorities up to the present time. Thus, the writer of the last work published upon Midwifery in Great Britain (Dr. Lee of London) states, when discussing the pathology of accidental and unavoidable hemorrhage—"It is from the great semilunar, valvular-like, venous openings in the lining membrane of the uterus, which we have seen in various preparations, and from the arteries which are laid open by the separation of the placenta, that the blood *alone* flows in uterine hemorrhage."[2]

When the placenta is partially separated from the uterus, there are two surfaces left exposed by that separation, namely, a portion of the internal surface of the uterus, and a portion of the external surface of the placenta. According to the usual explanation, such as I have above shown it to be, the hemorrhage is supposed to proceed from the first of these exposed surfaces, namely, that of the uterus. On the contrary, I am assuredly of opinion that it chiefly, and, in most instances, entirely, proceeds from the other surface, namely, that of the placenta. I feel quite convinced that the pathological opinion on this point advocated by the late Professor Hamilton is the correct explanation. After citing the opinions of Drs. Ramsbotham, Davis, Dewees, and Ingleby, in reference to the origin of the hemorrhage from the exposed uterine surface in unavoidable and accidental floodings, Dr. Hamilton observes:—"Many other authorities may be quoted to prove the common opinion upon this subject; and yet the author, from the earliest period of his professional life, has been anxious to show, that the hemorrhage in these cases proceeds from the separated portion of the placenta more than from the ruptured uterine vessels."[3]

To understand the true source of the flooding in unavoidable and accidental hemorrhage the cause of the continuance when the separation of the placenta is partial, and the mechanism of its arrestment when that separation is complete, we must take into consideration the following different points:—*First*, The maternal portion of the placenta is of a cavernous structure; that is to say it consists of a series of maternal vascular cells, or dilatations, or, perhaps, more properly speaking, of one large maternal vascular bag, into which the blood of the mother is conveyed by the utero-placental arteries, and from which it is removed by the utero-placental veins. *Secondly*, The vascular maternal cells, or immensely dilated capillaries which contain the blood of the mother in the placenta, communicate so freely with each other throughout all the different portions of the organ, that the blood which has access into one part, may in this way be rapidly diffused into the other portions of the placental mass. And, *Thirdly*, The deciduous or uterine surface of the placenta has no vital, muscular or contractile power by which it can constrict the orifices of the vascular tubes which pass from the uterus into it, when these tubes are ruptured in consequence of a greater or less detachment of the organ from the interior of the uterus.

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[1] Mauriceau, Diseases of Women with Child and in Child-bed, pp. 218, 219 of Chamberlen's translation.

[2] Theory and Practice of Midwifery, 1844, p. 361.

[3] Practical Observations, Second Edition, p. 312.

Cause of the Continuance of the Hemorrhage when the Detachment of the Placenta is partial. Explanation of its occasional cessation.

From the consideration of these premises, it will readily appear, that when a small portion of the placenta is detached, it occasionally occurs, as I have already shown in the early part of the present Essay, that the consequent hemorrhage is sometimes so great as to be dangerous, or even fatal, in its extent. Its amount, under this and other circumstances, may be also regulated and increased by the occurrence, in consequence of uterine contraction,[1] or otherwise of any *laceration* in the detached portion of the placenta itself; for when the substance of the organ is torn, its vascular maternal cells will be more freely opened into and exposed, and a more profuse discharge be allowed to issue from them. We may further easily conceive why the discharge should sometimes be actually more abundant when the detachment of the placenta is slight, than when it is greater in degree. For the quantity of blood passed into the maternal vascular structure of the organ, and consequently the quantity liable to escape from its unattached surface, will, in some respects, depend upon the extent of vascular placental connection which continues between it and the uterus. In other words, the intensity of the resulting hemorrhage will be regulated as much, or more, by the extent of placental surface which *still* remains in attachment to the mother, as by the extent of surface which is *already* detached; for the freedom with which the blood is supplied to the placenta will affect the violence of the flooding, equally, or more so, than the freedom with which that blood is allowed to escape from the open orifices of its ruptured vessels.

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[1] See illustrative cases in Smellie's Collection, vol. iii. p. 411 (the placenta "split in the middle"); and Ibid. vol. ii. p. 217 ("the placenta adhered to the os internum, near its middle, or thickest part, in which part I perceived a *laceration* upwards of an inch long, and penetrating almost through the substance of the placenta"). In both these cases the mothers died of the excessive flooding before delivery, and the condition of the placenta was ascertained on post-mortem examination.

Probably, in most cases, the hemorrhage will reach its maximum when the quantity of blood which enters the placental cells by the adherent portion is equal to what can reach, and escape from, the open orifices of the separated portion. Any additional separation after this will tend rather to diminish the flooding, as less blood will be carried into the placenta, from the number of its channels of supply being diminished. Most accoucheurs seem to believe that the greater the degree of detachment, the greater will be the hemorrhage, and hence, we are earnestly cautioned,[1] in the operation of turning in placenta prævia, not to separate more of the placenta from the cervix uteri than is absolutely necessary to permit of the passage of the hand. Theory as well as experience would seem to throw the greatest doubts upon the soundness and propriety of this rule. If at all true up to a certain extent of separation, it certainly does not hold good in regard to the detachment when carried to some degree further.

There is an additional anatomical reason why the accompanying hemorrhage should be excessive, in some cases where a very small portion only of the edge of the placenta has been separated. I have already quoted from Dr. Hamilton a case of this kind, which proved fatal, and where the "area of the separated placenta was less than a square inch."[2] The largest of the maternal vessels belonging to the placenta is that which Meckel, Jacquemier, and other authors, have described under the name of the circular sinus of the organ. It courses round the circumference of the placenta, in some parts being of a great size, and at other points more or less contracted, or even absent. I have usually found this maternal placentral vessel of great dimensions in several different parts of its course. In the cases in which excessive hemorrhages occur, when a small portion only of the edge of the organ is detached, I believe the danger and fatality of the result are to be ascribed, to the fact that some portion of the course of this large circular sinus has been opened, and thus a rapid and free tide of maternal blood allowed to escape from the disrupted part of this uncontracting tube.

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[1] As by Rigby, System of Midwifery, p. 262, "The less we separate the placenta, the less will be the hemorrhage;" Ramsbotham, Obstetric Medicine, p. 393, "The profuseness of the discharge will be principally regulated by the degree of separation." See also Levret, Accouchemens Laborieux, p. 68; Hatin, Cours d'Accouchemens, p. 178; Lachapelle, Pratique des Accouchemens, tom. ii. p. 440, &c. &c.

[2] Practical Observations on Midwifery, 2d edition, p. 314.

In all instances, then, of hemorrhage from partial separation of the placenta, I hold that the blood issues principally, if not entirely, from the uncontracted and uncontractible maternal orifices that belong to the external surface of the separated portion of the organ, and that the maternal blood is supplied more or less freely to these orifices, in consequence of the free communication existing among the different maternal cells, and from these cells being kept filled with blood through the utero-placental vessels of that portion of the placental mass, which continues to remain fixed and attached to the uterus.

Further investigations will probably show, that the greatest quantity of blood that escapes, flows from the exposed orifices which lie nearest to, or are actually involved in, the line of separation between the placenta and uterus. Along this line of separation, and in its immediate neighborhood, these orifices—consisting mainly of apertures of the large and torn *decidual* veins—will be for a time kept more stretched and patulous than in the other portions of the detached surface, and more especially will this hold true if there exist any tendency in the uterus to detach itself more and more from the placental surface along this line of junction by constant contractions. Under such circumstances the placental orifices alluded to, and those in their immediate vicinity, will afford, not only the freest, but also the nearest, channel for the discharge of the mother's blood flowing into its maternal cells.

In cases of partial detachment of the placenta from the interior of the uterus, the attendant degree of hemorrhage is also, no doubt, regulated by another important circumstance—namely, by the condition of the blood itself in the separated portion of the organ. If the blood in the maternal cells of that portion continues still to remain fluid, it will be ready to escape from every ruptured orifice upon the detached placental surface. Hence, when a considerable portion of the placenta is at once and suddenly separated, the discharge is sometimes excessive, until the blood in the tissue of the detached part becomes more or less coagulated. Gradually, by its coagulation and infiltration into the structure of the separated portion of the organ, it obstructs the maternal cells of that part, and consequently more or less completely arrests the discharge.

It is in fact to this infiltration and coagulation of blood in the detached portion of the organ, that we are, as Gendrin[1] has well shown, to look for an explanation of the occasional temporary cessations of those floodings which are so frequently observed during the latter periods of utero-gestation, in cases of placenta prævia. It is well known that when the placenta presents, hemorrhage is liable to occur at intervals, for days and weeks, or even for months, previously to the completion of the full term of pregnancy. Each of these hemorrhages depends upon a partial detachment of the expanding surface of the *cervix uteri*, from the expanding surface of the placenta. For the expansion of the cervix uteri, during the last periods of pregnancy, is known to produce its detachment from the placenta, when the placenta is implanted upon it, exactly in the same way as the contractions, or rather retractions, of the same part during labor, lead to a similar result.

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[1] Médecine Pratique, tom. ii. p. 216.

Each partial detachment occurring during pregnancy gives rise to the exposure of a greater or less number of vascular placental orifices, and consequently to a greater or less degree of hemorrhage from these orifices. Each of these hemorrhages generally ceases after a time, and the mechanism of their cessation is not so much to be found in any changes in the corresponding part of the uterus, as in the changes I have adverted to as occurring in the separated portion of the placenta. The blood becomes infiltrated and coagulated in the substance, and occasionally also upon the surface, of this separated portion; its vascular maternal cells are thus rendered impermeable; and the temporary arrestment of the flooding is consequently affected. The blood diffused and infiltrated into and upon the detached portion of the placental structure, undergoes a series of changes which I have elsewhere attempted to trace minutely;[1] and, after a time, the separated and ecchymosed tissue of the placenta itself becomes yellowish and atrophied, partly from the alterations which occur in the blood infiltrated through it, and partly from the obliteration of its vessels, and the consequent degeneration and desiccation of its tissues. In cases of placenta prævia, in which there has been a repeated recurrence of hemorrhage, and as frequent an arrest of it, we can occasionally trace in the placenta, after its expulsion, different parts of it, showing a series and gradation of pathological changes arising from successive partial detachments, and successive apoplectic infiltrations and obliterations of its substance from coagulated blood of different ages lodged in its structures. These alterations are confined to the detached portion, and the part always presenting the most recent stage of the pathological changes in question, is that lying nearest the line of junction between the separated and affixed divisions of the organ. The part showing the most advanced stage of the changes will be found situated furthest from this point; or is, in other terms, the part which was first and earliest detached. In cases of direct and central implantation of the placenta over the os, the centre of the organ, having in general become first detached, will be found to present the oldest morbid alterations; and the newer forms and phases of it may be sometimes traced in successive departments or layers, from this to the circumference of the detached portion—always supposing there has previously occurred a succession of detachments and attacks of hemorrhage. If the edge only of the placenta has presented—and several successive hemorrhages have in the same way taken place previously to labor—the same series of morbid changes will be found in the organ, running, not, as in the above instance, from the centre towards the circumference, but from the presenting or earliest separated point of the edge, more or less towards the centre of the mass.

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[1] See Pathological Observations on the Diseases of the Placenta, in the Edinburgh Medical and Surgical Journal, April, 1836, p. 275, and in vol. ii. of this work.

As affording some proof of the correctness of the view which I have already ventured to give of the immediate source of the discharge of unavoidable hemorrhage, I would beg to dwell for a moment on one other point:—Almost all obstetric authors mention, as a mark of diagnosis during labor, between unavoidable and accidental hemorrhages, that in the first or unavoidable species, the flooding is greatest during the pains, and least during the intervals; whilst in the last, or accidental form, the discharge is least during the pains, and greatest during the intervals. In placental presentations, "the character of the hemorrhage," says Dr. Rigby,[1] "is also different from that of common hemorrhage, inasmuch as it increases during a pain, and diminishes or ceases during the intervals, whereas in hemorrhage as in ordinary circumstances it is the reverse." I am not aware that any solution has been hitherto attempted of this peculiarity in unavoidable hemorrhage. And, whilst it seems very explicable upon the idea generally received that the discharge comes from the exposed surface of the uterus, it is a condition which we might have *à priori* anticipated from the opposite opinion, that the effusion flows from the detached surface of the placenta. For, if in placenta prævia the hemorrhage proceeded from the vascular orifices laid open on the interior of the uterus, it ought to be diminished and not increased in quantity during the pains, as these orifices will necessarily be temporarily diminished under the contraction of the uterine fibres. If we adopt, however, the other view, that the discharge proceeds from the open vascular orifices existing on the outer or maternal surface of the detached portion of placenta, we can easily understand how its amount should be temporarily augmented by each labor pain. For each uterine contraction, in pushing down the presenting part of the child against the compressible placental mass, will squeeze out from its maternal cells, as from a sponge, a portion of the fluid blood contained in them; and hence, during the pressure, an increased flow of this blood will issue from the vascular orifices opening upon its detached surface. During the intervals between the pains, a reaccumulation of maternal blood will take place in the interior of the placenta; but the quantity actually escaping will be comparatively less, till again it is forced out in accumulated amount by the compression by which it is subjected by a returning pain.

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[1] System of Midwifery, p. 255.

Cause of the Cessation of the Hemorrhage when the detachment of the Placenta is complete.

All the preceding remarks apply to the mechanism of unavoidable hemorrhage, and its arrestment, when the placenta is partially separated. Their application to the rationale of the complete arrestment of the hemorrhage, in those instances in which the placenta is *completely* detached, is still more obvious and simple. For, if the explanation which I have above given of the source of the hemorrhage in partial detachment of the placenta, be true—namely, that it proceeds principally, if not entirely, from the maternal vascular cells belonging to the separated portion of the organ being still, more or less freely, supplied through the utero-placental vessels of the adhering portion—we can further easily understand how it occurs, that the attendant hemorrhage is immediately moderated, or entirely arrested, when the placenta is once thoroughly and perfectly separated from the interior of the uterus, as in the class of cases which form the subject of the present memoir. If the flooding proceeds, as I have endeavored to show, from the detached and exposed surface of the placenta, and not from the detached and exposed surface of the uterus, the placenta must cease to yield any new or additional quantity of maternal blood, as soon as its own vascular connections with the mother are destroyed; or, in other words, the immediate source of supply of the hemorrhage is cut off, and its continuance consequently prevented, as soon as the placenta is entirely separated from the interior of the uterus.

Besides the preceding *anatomical* considerations, we must take some important *physiological* points into consideration, in investigating the mechanism of the complete cessation of unavoidable hemorrhage upon the complete separation of the placenta. The uterus, during pregnancy, has, like other organs under high vital periodic activity, both its arteries and veins, but especially the latter, enormously enlarged. The immediate and final object of this great enlargement is to supply the necessary materials for nourishment and respiration to the included fœtus. The medium through which these materials are supplied to the fœtus is the placenta; and the maternal cells of that organ form the more immediate locality in which they are transmitted from the maternal to the fœtal system. We have already stated, that these maternal placental cells are merely dilated capillaries, inasmuch as they constitute the only vascular connection between the terminations of the utero-placental arteries and the origins of the utero-placental veins. The capillaries, especially of those parts and organs which are liable to periodic excesses of action, are allowed to have a power of producing, under these excesses of action, an increased determination of blood to themselves and their corresponding arteries and veins, and that quite independently of any change whatever in the central organ of the circulation.[1] This seems to hold true to a great and remarkable degree with regard to the maternal form of capillary circulation carried on in the placenta, in consequence of the great and remarkable functions which this temporary portion of capillary circulation is destined to perform. And if the organic relations of the placenta to the uterus are disturbed, the resulting deviations are equally striking. If the placenta is only partially detached, the "attractive" power of the maternal circulation in the organ is, with the other moving powers of the blood, generally sufficient to keep the cells of the detached, as well as those of the adherent portion, filled with blood; and at the same time the circulation in the uterine arteries and veins in the immediate neighborhood continues to be more or less vigorously maintained, in consequence of their contiguity to the free tide of communication that is carried on between the uterine vessels, and the part of the placenta that still remains affixed. Hence, probably, even the orifices of the congested and enlarged uterine veins opening on the interior of the uterus, and lying near the existing line of junction between the uterus and the placenta, may occasionally allow of some discharge, in addition to the freer form of flooding that we have seen to take place from the more patulous apertures left on the exposed surface of the placenta itself. But separate entirely and completely the placenta from the uterus, and then you at once alter the course—as you have at once removed the great physiological aim and object—of the utero-placental circulation. The blood in the uterine vessels being now no longer attracted by the maternal capillary system of the placenta, and so far the *distributive* influence of that system being completely and suddenly abrogated, there is not only a less absolute tide of blood determined towards the uterus, but that which is contained in its vessels seeks the freest and most patent course backward to the heart through the higher and larger communicating branches between the uterine arteries and veins. The placental capillary influence no longer *turns aside* from these channels, the direct and onward course of the circulating current into its own set of special uterine vessels. These special vessels, now that their function is arrested, are comparatively empty of blood, and their collapsing or collapsed sides may serve to keep the general vascular current in its proper canals; for the hemorrhage, if any, is from venous orifices, and hence easily repressed by slight impediments: and we shall afterwards see that it is not a direct discharge, but arises from retrogression or regurgitation of the blood in the venous tubes.

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[1] "The capillaries possess a distributive power over the blood, so as at least to regulate the local circulation, independently of the central organ, in obedience to the *necessities* of each part."—See note in Mr. Palmer's admirable edition of the Works of John Hunter, vol iii. p. 332.

I might add, if it were necessary, some analogous instances in illustration of these opinions. The case of a limb suddenly and completely avulsed by machinery, or otherwise, might be shown to be similar in most points. Every one knows how slight the hemorrhage generally is, which is met with in connection with this severe accident. But another illustration may be regarded as more apposite. The fœtus has a circulation of blood to and from the placenta through its umbilical vessels, in the same way as the mother has a circulation to and from the placenta through its utero-placental vessels. We have found as a general law, that when the utero-placental vessels are entirely and totally divided by the complete detachment of the placenta, the mother has little or no tendency to lose blood from their exposed extremities. When the umbilical vessels are divided, the fœtus has as little tendency to lose blood from their divided extremities, if once the vicarious *function* of the pulmonary respiration is freely established.[1] In the one instance as in the other, the circulation through the divided vessels is stopped, and their tendency to bleed is arrested as soon as the *physiological* conditions which called these vessels into existence and action, is completely arrested or superseded, and that though some *anatomical* conditions of its vessels, apparently favorable to hemorrhage—particularly the presence of a large venous tube or tubes unprovided with valves, and admitting of regurgitation—may be still found persisting.

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[1] In reference to this point it may be necessary to state two facts. *First.* It is generally acknowledged by physiologists, as the result of various observations, that immediately after the child is born, the umbilical vessels cease to pulsate and carry blood, if the pulmonary respiration becomes active; and if the pulmonary respiration is by any cause interrupted or arrested before the cord is divided, the circulation through the umbilical vessels again becomes more or less active. *Secondly.* There seems to be very little danger of hemorrhage from the umbilical vessels after the division of the cord, if previously to that division, the pulmonary respiration be allowed to become completely established. Hence various practitioners have gone so far as to aver, that it is unnecessary, as a general rule, to place a ligature upon the fœtal extremity of the cut umbilical cord, the tendency to bleed directly through its arteries, or indirectly by regurgitation through its veins, being so slight as not to require it, if the cord be not cut till the child has cried loudly, and the lungs are in full and free action. See, in support of this opinion and practice, Dehmel in Haller's Dissertationes Anatomicæ, tom. v. p. 607; Koltschmid, De intermissâ Funiculi Umbilicalis Deligatione non absolutè lethali, Jena, 1751; Schweikhard, De non necessariâ Funiculi Umbilicalis Deligatione, Argent, 1769; Carboue, Journal General de Médecine, tom. iii. p. 334; Van der Eem, De Artis Obstetricæ Hodiernorum prae Veterum Præstantiâ, in Schlegel's Sylloge Operum Minorum in Arte Obstetricâ, tom. i. p. 94; Ziermann, Die naturgemasse Geburt des Menschen, &c., Berlin, 1817. C. Martin, in a Thesis published some years ago at Munich, De Ligaturâ Funiculi Umbilicalis, maintains (p. 11) that the practice of tying the fœtal extremity of the cord is not only useless, but hurtful, and denounces its adoption as a reprehensible crime propagated down to our times, *facinus damnandum ad nostra usque tempora propagatum.* "Whatever," observes Velpeau, "may be the explanation, it always happens, that if it is left to itself and without ligature, the cord would very rarely expose the fœtus to any hemorrhage, or to any accident, even if it were clean cut, and not bruised or torn."—Traité des Accouchemens, tom. ii. p. 566.

Absence of Hemorrhage in Twin Labors, with one or both Placentæ entirely detached before the birth of the Second Child.

There is another series of cases which, if my present space permitted, might be adduced at length, both in corroboration of the fact, that the complete separation of the placenta is not followed by hemorrhage, and in evidence of the special explanation of the cessation of the flooding which I have above offered. I here advert to cases of twins, in which it occasionally occurs, that, after the birth of the first child, and before the birth of the second, one or both of the remaining placentæ are expelled, and yet no hemorrhage follows.[1] Such cases may be arranged under three divisions, namely—

1. *Twins, in which, after the birth of the first child, its own placenta is expelled or removed, the other infant and placenta remaining without flooding, for a greater or less length of time in utero.*—"When a woman," says Mauriceau, "has a plurality of children, we must not deliver the placenta till after the birth of the last infant; because there would be produced a great discharge of blood (*une grande perte de sang*) if we thus detached the placenta prematurely."[2] Dr. Denman adverts to this subject in language implying similar theoretical doubts upon the point, and yet, at the same time, affording practical confutation of it. "When," he remarks, in his observations upon twins, "the placenta are separate, that of the first child should not be extracted before the birth of the second child, as a discharge of blood must *necessarily* follow, and perhaps a hemorrhage; though sometimes one placenta has been discharged before the birth of the second child, without any material loss of blood. In some cases of hemorrhage, when there was only one child, the placenta has been expelled before the child, without any detriment, though not without much apprehension of danger."[3] "I have seen," Dr. Collins

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[1] See Proceedings of Edinburgh Obstetric Society, December 22, 1847.—Edinburgh Monthly Journal, March, 1848, p. 692.

[2] Aphorismes touchant la Grossesse, &c., No. 214.

[3] Introduction to the Practice of Midwifery, p. 541.

observes, "several instances where the placenta of the first child came away without interference before the birth of the second, and yet there was no hemorrhage of any consequence."[1] Several cases of a similar kind have been reported to me by different professional friends.[2] They all tend to give a direct contradiction to the observation of Caseaux—the author of one of the latest and best French works on midwifery—that the complete detachment of the afterbirth of the first child "would expose the woman to a fatal hemorrhage (*exposerait la femme à une hémorrhagie mortelle*)."[3] In his work, Peu even ventured to suggest it as a proper mode of practice in some cases, to remove the first placenta, when ascertained to be quite isolated, before the birth of the second child. "When there is," he observes, "more than one child, the method ordinarily followed is to receive the one that presents first, the cord of which should serve as a guide for the others. The fingers are slipped along it to the mass of the placenta, to discover if it is absolutely separated from the other placentæ. In this case, which is sufficiently rare, one may draw it out immediately."[4]

It may be objected to these twin cases, as corroborative of the fact for which I adduce them, that the distended bag of the membranes of the remaining child might prevent any hemorrhage by acting as a sufficient compress upon the whole uterine surface laid bare by the detachment of the placenta of the infant already expelled. But this objection will not hold good with respect to twin cases included under the two divisions which I have next to speak of.

2. *Twins, in which after the birth of the first infant, the placenta of the second child was expelled, one infant and placenta remaining still in utero.*—A case of this kind has been related to me by my friend Dr. Andrews, Lecturer on Midwifery at the Westminster School. One of his pupils was in attendance. A placenta was expelled after the birth of the first child. Little or no hemorrhage occurred till the

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[1] Practical Treatise on Midwifery, p. 312. Dr. Collins adverts to two hospital cases. The two following similar cases occurred to him in private practice:

1. "C. S. was delivered of twins, June 21, 1823. The placenta of the first child was expelled immediately. The feet of the second were then found in the vagina, and brought down. *There was not the slightest hemorrhage.* The first was born alive, the second putrid."

2. "M. C. was delivered of twins, May 6, 1823. The placenta of the first child was expelled before the birth of the second, *without hemorrhage.* Both children were born alive at the full period." See Ibid. pp. 312, 313.

[2] Thus, "I have seen," Dr. Dawson of Newcastle writes me, "two cases of twins, where the placenta of the first came away before the birth of the second child. There was *no hemorrhage* in either case. Mr. Sang," Dr. Dawson adds, "has met with two cases of this description, and also *without hemorrhage.*" A case of the same kind, and with the same result, occurred some time ago to Dr. Dickson, one of my own pupils. Dr. Fenton, Dr. Campbell, and others, have related to me similar instances.

[3] Traité de l'Art des Accouchemens, Paris, 1841, p. 785.

[4] Pratique des Accouchemens, p. 208.

second infant was born, some time afterwards. It was found that the previously expelled placenta belonged to it, and the still retained placenta belonged to the child that had been first born. Here, in a case of twins, we had a child and a placenta remaining in utero, but the remaining placenta was not the placenta of that child. And the placenta which was previously detached and separated had necessarily torn asunder the bag of membranes, leaving the portion of uterine surface to which it was itself affixed exposed, without that exposure leading to any marked degree of hemorrhage.

3. *Twins, in which between the birth of the first and second child, the placentæ belonging to both were expelled.*—Dr. Dewar of Dunfermline has reported to me two cases of this kind. In one "the placentæ were firmly united, and were discharged after the birth of the first child. There was *no* unnatural discharge of blood. The mother did well." This case occurred in the practice of Dr. Brown. The other occurred under Dr. Dewar's own observation. "The mother had borne several children. The first child, which was at the full time, presented by the feet, and immediately after its birth the two placentæ, connected by a membranous but not vascular band, were expelled. Labor followed briskly, and in little more than five minutes the second child, which presented naturally, was born. *The hemorrhage,*" Dr. Dewar adds, "*was very slight*, and not greater than what occurs after some ordinary labors. The mother did well."

It may be considered that, in these two cases, the interval between the expulsion of the placenta and the birth of the child was too brief to allow accurate observations to be made upon the degree of existing hemorrhage. This objection is completely answered by a third case of the same kind mentioned to me by my friend Dr. Protheroe Smith of London. "I was," he writes to me, "called to a patient some time since who had given birth to one child, immediately after which a large double placenta followed *without hemorrhage*, leaving a second child in utero, which was expelled, dead of course, *three or four hours afterwards, without further discharge of blood.*" Dr. Tyler published, two years ago, a similar case.[1] After the birth of the first child, a double placenta was discharged. Two hours afterwards, the shoulder of the second child was found presenting, and the uterus in a state of "hour-glass contraction." Many unsuccessful efforts to turn the fœtus were made. At last its thorax was eviscerated, and the breech brought down. The mother made a good recovery. "Here," observes Dr. Tyler, "we have a case not merely of simple placenta prævia, but a double surface exposed—

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[1] American Journal of the Medical Sciences for October, 1843; or, Provincial Medical Journal, vol. vii. p. 245.

there having been two placentæ; the fibres of the womb first in a state of rigid contraction; then the irritation consequent upon the performance of the embryotomy; and, lastly, a state of excessive relaxation, *and still not a drop of hemorrhage*, and the female entirely recovered."

Presence of Hemorrhage in Partial, and its absence in Complete, Detachment of the Placenta in the Third or Last Stage of Ordinary Labor.

I have now referred, under the present section, to two separate sets of facts, illustrative of the non-supervention of hemorrhage after the complete detachment of the placenta from the interior of the distended uterus. We have seen that both in the placental presentations and in twin labors, a large portion of the internal surface of the uterus may be left exposed by the entire separation of the placenta, while the organ continues distended by the presence of a remaining child, and yet the vessels of that exposed surface have little or no tendency to bleed, if the placental mass be only once completely detached from it. We have further seen, that if, in the same cases, the placenta be only partially separated from its uterine attachment, hemorrhage is generally present. The supervention of the hemorrhage, when the detachment of the placenta is partial, is probably not more certain than its cessation is, when that detachment is perfect and complete.

Though these facts have escaped the general notice of obstetric pathologists, they have all been long familiar with a *third* set of instances in which the same phenomena may be observed. I refer to circumstances well known in connection with the last or third[1] stage of common labor, and after the child is born. When during that period of the labor, the placenta happens to become only partially separated from the uterus—as in cases where a portion of it still remains firmly attached from morbid adhesion—hemorrhage is generally present. When, on the other hand, the placenta becomes totally separated from the interior of the uterus, either naturally or artificially, all hemorrhage generally ceases. In other words, we have the same consequences resulting from partial, as contrasted with complete, detachment of the placenta, in the last stage of labor, and when the uterus is comparatively small and contracted, as we observe under the same conditions in the earlier stages of parturition, and when the uterine cavity is still filled and distended by the presence of an infant. Under these, at first apparently different circumstances, the general fact seems to be the same, namely, that hemorrhage is almost sure to occur if the placenta be only partially separated; it is almost as sure to cease if the placenta be once fully and entirely detached.

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[1] In correspondence with the language of Denman, Hamilton, Ramsbotham, &c., I here use the words "third stage," as denoting the period intervening between the birth of the child and the expulsion of the placenta.

Means by which Hemorrhage is prevented from the Venous Orifices left exposed on the Interior of the Uterus.

I have no intention at present to push the present investigation further, and enter upon a full inquiry into this *other* remaining point, namely, by what special mechanism nature prevents the occurrence of hemorrhage from those patulous vascular orifices that are left on the interior of the uterus when the placenta is, as in the instances we are considering, completely detached from its internal surface. The simple empirical fact of its non-occurrence is sufficient for *all the practical purposes* of the present essay. And I believe that the subject, if fully discussed, would form an anatomical and pathological problem that would require for its complete solution a much more laborious and lengthened series of researches than I can pretend at this time to afford to it. I have, however, but little doubt, that the means by which in placental presentations, hemorrhage is prevented from the uterine vessels themselves after the detachment of the placenta, will be found in all its essential points to be the same by which flooding is prevented after the removal of the placenta in the ultimate stage of common labors. The non-occurrence of hemorrhage from the uterine vessels in this last state, or after the complete detachment of the placenta in ordinary parturition, is probably not explicable, as is generally imagined, upon the sole circumstance of the simple and absolute contraction that occurs in the uterine fibres after delivery. We know, from the observations of Gooch,[1] Velpeau,[2] Rigby,[3] and others, that post-partum hemorrhage sometimes supervenes when the uterus appears contracted and reduced to its usual size after delivery. On the other hand, the facts that I have adduced in the preceding pages show, that there is little or no tendency to hemorrhage after the perfect expulsion of the placenta, when this simple and absolute contraction of the uterine fibres is so far prevented by the presence in utero of a full-grown fœtus. When the child is born in ordinary labor, and the placenta happens to be retained from *want* of uterine contraction, hemorrhage does not necessarily supervene. It is well known that Ruysch, William Hunter, and others, adopted, for a time, the practice of leaving the placenta in utero for hours and days till nature herself threw it off, and that they were at last forced to abandon this line of treatment—not because uterine hemorrhage was liable to supervene, but because the dead and retained organ was found to become putrid, and to give rise to symptoms of severe irritation and fever. Again, after the complete evacuation of the uterus in common parturition, and the total removal of the placenta, hemorrhage does not necessarily supervene, though the uterine fibres are not in a state of firm contraction. We often find them alternately relaxing and contracting when after-pains supervene, and yet the general relaxation that is observable between the pains may not give rise to the slightest degree of flooding. Every practitioner has had occasion to watch, with more or less anxiety, the uterus remaining of considerable size and softness for some time after delivery, and consequently with its fibres not firmly contracted, but without after-pains, and without hemorrhage supervening. In the same way I shall afterwards have occasion to show that in some cases of placental presentation after the placenta had become expelled, and while the child remained in utero, the labor-pains have ceased; and still, notwithstanding this cessation, not only has no hemorrhage followed, but, on the contrary, the flooding that previously existed has immediately ceased.

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[1] Medico-Chirurgical Transactions, vol. xii. p. 157. "The observing practitioner must," Dr. Gooch observes, "have been frequently struck by the little proportion that existed between the want of contraction and the degree of hemorrhage; having found the uterus bulky without any hemorrhage, and a profuse hemorrhage without greater bulk of uterus. Nay, further, I have witnessed a profuse hemorrhage, though the uterus had contracted in the degree which commonly indicates security; and I have ventured to do what is seldom justifiable, separate the placenta before the uterus had contracted, without more hemorrhage than after a common labor. What is this circumstance that has so great an influence that its presence can cause a moderately contracted uterus to bleed profusely, and its absence can cause an uncontracted uterus to bleed scarcely at all?"—P. 152.

[2] Traité de l'Art des Accouchemens, tom. ii. p. 539.

[3] London Medical Gazette, vol. xiv. p. 332, and System of Midwifery, p. 218.

Such instances prove that strong uterine contractions in the special complication which forms the subject of our observations, are not probably so essential a part in the mechanism of the prevention of hemorrhage from the open orifices of the uterine veins as we might *à priori* suppose. When hemorrhage does continue after the expulsion or extraction of the placenta in placental presentations, the determining cause of the hemorrhage is probably the same as gives rise to this accident in the last stage of labor, and after the complete evacuation of the uterus. In two cases, which have been already quoted at length, and where, in placental presentations, the hemorrhage remained after the complete expulsion of the placenta, the child being retained in utero, the flooding still continued also for a time after the infant was extracted, and the uterus was completely emptied. The same circumstances—whatever they may be—in all likelihood led to its recurrence under both of these conditions.

No doubt the occurrence, after delivery, of great and decided atony in the whole muscular system of the uterus, does assuredly give rise to post-partum hemorrhage;[1] and the same antecedent condition may be the cause of the continuation of the flooding in some circumstances where this happens after the expulsion of the placenta in placenta prævia. But if I may judge from my own observations, I would venture to remark, that the morbid condition which is most frequently and earliest seen in connection with post-partum hemorrhage, is a state of irregularity and want of equability in the contractile action of different parts of the uterus—and, it may be, in different planes of the uterine fibres—as marked by one or more points in the organ feeling hard and contracted, at the same time that other portions of the parietes are soft and relaxed—and by the contracting and relaxing fibres slowly but frequently changing their relative situations.[2]

Upon the same principle, I believe that in attempting to prevent or remove the morbid condition leading to post-partum hemorrhage, when it is functional in its nature, and not connected with any organic or traumatic causes, we ought to endeavor to produce not merely a certain degree and amount of uterine contraction, the great and primary practical point to which we always justly look, but also a certain equability and uniformity of contraction. The same may perhaps be found true, both pathologically and practically, in regard to the state of the uterus, after expulsion or extraction of the placenta before the child, in placental presentations. At the same time, I would repeat, that this part of the subject, like the whole question of the manner and means by which hemorrhage is prevented from the exposed uterine veins, after every case of ordinary labor, stands, in my opinion, in need of new, careful, and extended investigations. I have, however, at present, no desire to encounter so wide and complicated an inquiry; and shall content myself with stating in reference to the subject the few following suggestions:

First. Uterine hemorrhage, after the separation of the placenta, in any of the stages of the labor, is *not Arterial* in its character. The utero-placental arteries are numerous, but so long and slender[3] as to become readily closed; 1. By the tonicity of their coats; 2. By contraction of the uterine fibres upon the course of these vessels themselves as they pass through and amid the uterine structure; and, 3. And principally, by the changes in their tissues produced by the mechanical rupture of their coats—torn arteries being little, if at all, liable to bleed—and the placenta being separated by a true process of avulsion.

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[1] For similar remarks by Dr. Simpson on post-partum hemorrhage, see Northern Journal of Medicine, Jan. 1846, p. 1.—(Ed.)

[2] "I have rarely introduced the hand into the uterus in a case of flooding without meeting with it (hour-glass or irregular contraction), whether the placenta had or had not been expelled."—Dr. Burns's Principles of Midwifery, 9th edit. p. 543.

[3] I speak of these utero-placental arteries as they are seen in the beautiful injected preparations of them left by William Hunter and the second Munro, and as I have myself observed them in recent specimens.

Secondly. Hemorrhage, therefore, under the conditions supposed, is *Venous* in its source and nature. Further, it is especially important to mark that it is a *venous hemorrhage by retrogression.* The *forward* course of the uterine and utero-placental venous circulation is from the dilated maternal capillaries or cells of the placenta towards the periphery of the uterus, and the ovarian and hypogastric venous trunks. In uterine hemorrhage, the blood that escapes, instead of flowing onwards, regurgitates *backward* into the uterine cavity.

Thirdly. The mechanism by which, after the separation of the placenta, this retrograde course of the venous circulation towards the cavity of the uterus so as to lead to hemorrhage, is *prevented,* is probably of a compound character, or is effected by different means. Each of these means may be more or less efficient under different circumstances and at different times.

Fourthly. The most powerful of these preventive measures consists in the uniform and regular contraction of the uterine fibres. By this contraction the canals of the supplying arteries are constricted, and the venous tubes or sinuses which more immediately yield the discharge are directly compressed. The facility of this compression of the sides of the veins and the consequent diminution of their cavities is promoted by the naturally thin, flattened forms of their canals, and by the fact that the proper contractile tissue of the uterus forms their second coat—the uterine veins consisting of the usual lining membrane of the venous system placed in direct contact with the muscular tissue of the uterus. At the same time, it is to be recollected that there seems to be often no direct relation between the degree of uterine contraction and the degree of tendency to hemorrhage, for, as we have already seen—1. No hemorrhage may occasionally be observed after delivery, though the uterus is not contracted to its usual degree; and, 2. It may be present when the uterus is apparently well contracted. But, 3. There are, according to most anatomists, few or no *contracting* fibres in the structure of the os and cervix uteri, and certainly after delivery I have generally, if not always, found it remaining open, gaping, soft, and flaccid, even when the proper cavity of the uterus above felt shut and contracted, and its parietes hard and firm. Still when the placenta is attached to the surface of this uncontracting portion of the uterus, hemorrhage is not common after its separation, unless some laceration of its vessels has occurred.

Here we have post-partum hemorrhage prevented, *without* the contractile mechanism, generally considered necessary for its avoidance, being almost in existence. And lastly, in cases of spontaneous or artificial extraction of the placenta in some placental presentations and twin labors, the placental mass may be completely separated, and the uterus still remain distended by the presence of a child in its cavity, so as to prevent much contraction of its fibres, without hemorrhage occurring. The venous trunks running to the uterus are not supplied with valves, and under the above and other circumstances, by what means in addition to, or in substitution of, the contraction of the uterine fibres, does nature prevent the retrograde flow of venous blood into the uterine cavity—or, in other words, uterine hemorrhage?

Fifthly. The structure and mutual relations of the venous sinuses of the uterus seem calculated to obstruct and prevent such a retrograde flow of blood in their tubes as to cause hemorrhage. The uterine veins are large, but of a compressed, flattened form, and arranged in several planes or floors above one another in the uterine walls. On examining these veins in several pregnant uteri, by dissecting them from the outer or peritoneal surface of the organ, downwards towards the mucous, I have found the following arrangement: Each venous tube gives off numerous communicating branches to the veins of its own plane or floor, by a set of *lateral* foramina. When, however, a venous tube of one plane comes to communicate with a venous tube lying in the plane immediately beneath it, the foramen between them is not in the *sides*, but in the *floor*, of the higher or more superficial vein, and the opening itself is of a peculiar construction. Looking down into it from above, we see the canal of the vein below partially covered by a semilunar or falciform projection formed by the lining membrane of the two venous tubes, as they meet together at a very acute angle, the lower tube always opening very obliquely into the upper.[1] In the folds of these falciform projections, the microscope shows the common contractile tissue of the uterus. Do these semilunar or falciform projections, and the oblique communications of the lower with the higher planes of veins, allow the normal flow of venous blood from the deeper to the more superficial veins of the uterus, while after the placenta is separated, they prevent that anormal or retrograde flow of it from the more superficial towards the deeper-seated venous tubes which would produce hemorrhage? Here I suppose it possible that these falciform processes may act upon the same principle as the Eustachian valve, but in a less perfect manner, while by the obliquity of the communications between the different planes of veins it may be that blood does not so readily retrograde into the deeper vessels, in the same manner as urine does not retrograde into the ureters from the bladder, in consequence of the oblique opening of the former into the latter. Do the uterine fibres seen in the venous falciform processes tend to aid this valve-like mechanism, by diminishing, under contraction, the apertures between the different planes of veins?

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[1] In the course of dissecting the veins of a pregnant uterus, in the sixth month, from the peritoneal surface downwards, Mr. Owen states that he "observed that where the veins of different planes communicated with each other, in the substance of the walls of the uterus, the central portion of the parietes of the superficial vein invariably projected in a semilunar form into the deeper-seated one; and where (as was frequently the case, and especially at the point of termination on the inner surface) two, or even three of these wide venous channels communicated with a deeper sinus at the same point, the semilunar edges decussated each other, so as to allow only a very small part of the deep-seated vein to be seen. It need scarcely be observed how admirably this structure is adapted to insure the arrest of the current of blood through these passages, upon the contraction of the muscular fibres with which they are everywhere immediately surrounded."—Works of John Hunter, vol. iv. p. 68. See also Mr. Goodsir's corroborative statement, in his admirable Anatomical and Pathological Observations, p. 61.

Sixthly. I have already shown that one cause contributing to prevent hemorrhage after the total separation of the placenta, is the abstraction from the uterine vascular system of the derivative or sugescent power of the maternal circulation in the placental cells, and the consequent tendency of the blood to flow in the more direct and freely communicating channels that exist between the uterine arteries and veins. Besides, the general and direct forward current of the blood along the course of these larger uterine veins diminishes, and, in a measure, destroys, the tendency which it might otherwise have either to flow backwards, or to escape by any existing lateral apertures of the vessels.

Seventhly. Among the other remaining means by which hemorrhage is more or less prevented after the detachment of the placenta, I may mention—1. The occasional presence of tufts of fœtal vessels left in the orifices of the uterine veins,[1] and forming not only immediate mechanical obstacles, but nuclei for the ready coagulation of the blood; 2. The formation of coagula in some of the collapsed venous tubes and orifices; and, 3. The presence for some hours, or even days, after delivery, of the collapsed decidua over the apertures seen in the veins on the interior of the uterus.

To these few and imperfect suggestions I am desirous to add one remark. Some of the natural means of arresting uterine hemorrhage that I have spoken of admit of extended anatomical examination being applied to their more perfect investigation; and several of the observations that I have ventured to offer in this section may be yet proved or disproved, by being tested by direct experiments with vascular injections thrown into the dead body.

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[1] See, on this point, the observations of Professor Reid in his excellent paper on the Anatomical Relations of the Bloodvessels of the Mother and Fœtus.—Edinburgh Medical and Surgical Journal, vol. lv. p. 8.

SECTION VIII.—INSTANCES OF PLACENTAL PRESENTATIONS, IN WHICH THE PLACENTA HAS BEEN ARTIFICIALLY REMOVED BEFORE THE CHILD; WARNINGS BY AUTHORS AGAINST ATTEMPTING THE PRACTICE; CASE OF ITS SUCCESSFUL ADOPTION; RECORDED INSTANCES OF IT UNDER SUPPOSED MISMANAGEMENT; CASES OF PORTAL; SUGGESTION OF CHAPMAN; MODERN CASES OF EXTRACTION, ETC.

We have already found that various obstetric authors (see Section I.) have strongly declared the total inefficiency of nature in cases of placenta prævia. Several of them have especially warned us against attempting to imitate her in any of her modes of management of this obstetric complication, and against taking any hints from the principles of treatment which she seems to follow under the circumstances. Dr. Ramsbotham has, in his admirable "Observations on Midwifery," reported several cases of placental presentations in which the placenta was expelled before the child. In commenting upon these cases, he remarks, "Although it satisfactorily appears that a spontaneous detachment of the placenta is not necessarily followed by fatal consequences, that fact can furnish no precedent in practice for the artificial separation and removal of it. I think," he adds, "that few practitioners, aware of the probable consequences, would have the temerity to make the experiment."[1] "It would," he again observes in another place,[2] "be the extreme of hardihood in any practitioner to attempt the artificial separation of this fœtal appendage in imitation of its natural expulsion." In a similar strain Dr. Burns, the distinguished author of the well-known "Principles of Midwifery," observes, "There are, doubtless, examples where the patient has, by labor, been safely, and without assistance, delivered of the child, when part of the placenta has presented. Nay, there have been instances where the placenta has been expelled first, and the child after it. These examples are to be met with in collections of cases by practical writers, and some solitary instances are likewise to be found in different journals. It would be much to be lamented if these should ever appear without having, at the same time, a most solemn warning sent along with them, to the accoucheur, to pay no attention to them in his practice. I am convinced that they may do inexpressible mischief, by affording argument for delay, and excusing the practitioner, to himself, for procrastination. . . . . . These instances are not to be converted into general rules, nor allowed to furnish any pretext for procrastination. They happen very seldom, and never ought to be related to a young man without an express intimation, that he is not to neglect delivery, when required, upon any pretence whatsoever. There is scarcely any malady so very dreadful as not to afford some examples of a cure effected by the powers of nature alone. But ought we, thence, to tamper with the safety of those whose lives are committed to our charge?"[1]

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[1] Practical Observations on Midwifery, part ii. p. 193.

[2] Ibid. p. 286.

Notwithstanding the strong and decided opinions expressed by the above authors and others, who might be cited for the purpose, I became so convinced—from two cases which I happened to see, and from the study of others that I found upon record—that the extraction of the placenta before the child was the proper plan of treatment in some cases of placenta prævia, that during the course of 1841, I laid before the Obstetric Society of Edinburgh the deductions derivable from thirty-nine cases that I had then collated, and proposed that the artificial separation of the placenta ought to be our line of practice in some instances of unavoidable hemorrhage. I was deterred at the time from publishing my investigations, under the fear that it would be considered rash in a young member of the profession, propounding from the data which I had, so great a revolution in the usual practice of placental presentations. In the course of the spring of 1844, I laid before the same Society an additional number of cases, with the view of supporting the same opinion. The data which I then had to found upon were not so extensive as those I have now brought forward, but they were to my mind so distinct and decided, that notwithstanding the earnest counsels of Drs. Ramsbotham, Burns, and others, I predetermined to have recourse to the artificial extraction of the placenta itself, in any case that I should happen to meet with in which the hemorrhage was very great, and where the rupture of the membranes was insufficient, or the operation of turning dangerous or impossible.

It was not till last autumn that I met with an instance in point. And I hope I shall be excused for stating at length a case, which was to me so interesting in its nature and in its results.

CASE XXIV.—*Great hemorrhage; rupture of the membranes insufficient; the os uteri not so dilated as to allow of turning; the placenta artificially extracted about two hours before the birth of the child.*—I was called to see the lady who was the subject of this observation, at about five in the afternoon of the 1st of October, 1844. The gentleman who was in attendance upon her, Mr. Hill of Portobello, informed me on arriving, that she was between the seventh and eighth month of pregnancy; that she had almost daily suffered from considerable discharges of blood, without pain, for about a fortnight previously; and that she had been flooding with slight uterine contractions, for about ten or twelve hours before my visit. I found her a weakly person, with bleached features, and much sunk and prostrated by the preceding hemorrhage. The pulse was very small, soft, and compressible. The abdomen seemed much distended with liquor amnii. On examination, I found the vagina filled with coagulated blood. It was exceedingly difficult for me at first to reach the os uteri, partly in consequence of the rigidity of the parts, and partly from the very high situation of the os uteri, itself. On touching the os uteri I found it still very slightly dilated, and on passing the finger through it, it came in contact with the anterior edge of the placenta; the presentation being one in which the placenta was attached to the posterior lip of the cervix uteri, and so as to project over the os itself. Mr. Hill and I agreed together to allow the escape of the liquor amnii, provided I could reach and rupture the membranes. I was enabled to do so with some little additional difficulty; and immediately upon perforating the membranes anteriorly, an immense gush of liquor amnii took place, and the abdomen became comparatively smaller. I had hopes that I had done enough to arrest, in all probability, the hemorrhage under which the patient was evidently sinking. She got at the same time a large dose of the ergot of rye, and we waited with some impatience for the result. Stronger uterine contractions came on, and shortly afterwards I re-examined, in order to know their effect on the os uteri. I was distressed to find the vagina again filled with clotted blood, showing too manifestly that the rupture of the membranes, and the supervention of more powerful pains had been anything but sufficient to arrest the progress of the flooding. A small portion of the anterior part of the placenta was by this time threatening to protrude through the os uteri. I passed my finger by the side of it posteriorly, endeavored to detach as freely as possible the organ, and then seizing the protruded part between two fingers, I gradually and steadily pulled the whole mass downwards into the vagina, and through the vulva. After accomplishing this, I confess that for a few minutes I felt a degree of timidity at what most of my professional brethren would have at once denounced as a most improper line of proceeding on my part, and one in direct opposition to all the most approved and established rules in obstetric medicine. The result, however, was such as to answer my best expectations. All fears were dissipated, by ascertaining in a few minutes that there were no new clots, nor any new discharge of blood accumulating in the vagina, and that the head of the infant was presenting—a circumstance which could not be previously ascertained. The cervix, however, was still so undilated as to hold the head from impinging directly on the interior of the os uteri—a band of contracted fibres high up in the cervical canal acting as a shelf on which it rested.[1] The patient got an additional dose of ergot, and I cut through the umbilical cord, and separated the placenta, for the purpose of ascertaining, by ocular inspection of the mass, if the whole of the organ had been extracted. It was for this purpose placed upon a plate nearly two hours before the labor was completed. The infant came down slowly, but without any additional hemorrhage. The mother made a perfect recovery. Her pulse, during convalescence, never, I believe, rose above 80.

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[1] Principles of Midwifery, p. 367.

In the preceding instance, I adopted the practice which I have detailed as a matter of principle and choice, and as a result of observation and reasoning upon the cases which I had previously seen and collected. In a number of other instances upon record, the same practice has been adopted through reputed ignorance, on the part of the attendant, of the established rules of midwifery in this variety of obstetric complication. The details, however, of some of these cases may both demonstrate the truth of the principle and practice which it is my wish to establish, and the practicability and propriety of the treatment itself. The first case I shall quote is from Dr. Collins' Treatise. He gives it as "an instance eminently calculated," to quote his own expression, "to show the marvellous escapes occasionally witnessed when the gross ignorance of the attendant blinds him as to the danger of his patient."

CASE XXV.—*The placenta removed many hours before the birth of the child.*—"G. J., at her full time was admitted in a state of extreme debility, her pulse so weak and frequent as not to be counted. The foot was found in the vagina, so putrid, that the skin peeled off on the slightest touch. The discharge was fœtid. Stimulants and cordials were freely given, and the child brought away without difficulty. The uterus remaining enlarged and relaxed, the hand was passed to remove the placenta, when there was *none* to be discovered, nor was there any hemorrhage. The membranes had ruptured and been discharged a fortnight previous to admission, from which time, until the evening before she was brought to the hospital, she had had more or less hemorrhage. It was now ascertained that the placenta had been expelled the evening before her admission, and separated by the midwife in attendance. She had been twice visited by a medical practitioner, who bled her and gave her purgatives. She left the hospital well on the 13th day."[1]

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[1] See an analogous and very interesting case of placental presentation, with the os uteri largely dilated, and stricture in the upper part of the cervix, recorded by Dr. James Reid, in the London Medical Gazette, vol. xvi. p. 145. Dr. Reid's case was further remarkable, as being one of those instances in which the placenta presented *without* any attendant hemorrhage.

Dr. Ramsbotham describes graphically, in words that demonstrate his own surprise, the following similar case:

CASE XXVI.—*Placenta extracted before the child; the flooding arrested; safety of the mother.*—"Late in the evening," observes Dr. Ramsbotham, "of Thursday, May 7, 1818, during a temporary absence from home, a message was delivered at my house, desiring me to see a woman at Wapping, who was said to be very ill, but of what disease, or in what state, was not mentioned. After some conversation between my servant and the messenger, it was agreed that I should visit this woman early the following morning, a second message was sent to countermand the first, with the intimation, 'that the woman was better, and was doing well.' A few days afterwards, I accidentally met the medical gentleman, who had sent the above verbal message, and inquired the nature of the case upon which he wished to have my opinion a few nights before. To which he replied, 'It was the strangest case I ever saw; it was a placenta presentation, with a most violent flooding; but I *got it away.*' 'Got what away?' said I; 'Why, the *placenta,*' answered he. 'What! before the child?, asked I; 'Yes, before the child,' said he; 'and the flooding ceased, and the woman did well; the child soon followed the after-birth.'"[2]

I am indebted for the following most interesting case to the politeness of Dr. Cripps of Liverpool, and shall relate it in his own words. Dr. Cripps' letter to me is dated 28th of December, 1844.

CASE XXVII.—*Placenta extracted ten hours before the child; arm presentation; no intervening hemorrhage.*—"I was sent for a few days ago, about 8 P.M., to see a poor woman who supposed herself to be at the early part of the last month of pregnancy with the third child. She had had occasional flooding to no great extent for a week previously. On the morning of the day on which I saw her, a surgeon had been sent for in consequence of the occurrence of several labor pains, together with a good deal of hemorrhage. This gentleman being out of town, his assistant went; he remained with her during the day, and in the evening finding things not going on so favorably as he wished, he sent for a friend of his employer's, who, soon after his arrival, sent for me. On making an examination, I found an arm down, which was much swollen, the pains very severe. I immediately gave one drachm of laudanum, and on their subsiding, turned without much difficulty. The funis was divided, only about four or five inches remaining, and appeared as though it had been cut. On expressing my surprise at this circumstance, I was informed that it was cut 'when the after-birth was taken away, about ten in the morning.' Not believing it possible that such could be the case, there *having been no hemorrhage whatever from that hour until the period of delivery*, I searched for the other portion of the navel-string, but not finding it, and being again assured that 'the after-birth had come in the morning,' I introduced my hand into the uterus, and made a most careful examination; it was contracting satisfactorily, but was perfectly empty. I watched her strictly until her complete recovery. I had every portion of discharge saved for my inspection, and am therefore perfectly satisfied that this is a case in which the placenta presented, and was removed ten hours previously to the birth of the child, and that, in the meantime, *there was no hemorrhage whatever*."

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[1] Practical Treatise on Midwifery, p. 102.

[2] Observations on Midwifery, part ii. p. 231.

The following brief notice of an analogous case, reported by Dr. Löwenhardt, is extracted from Kleinert's Repertorium for 1842, tom. vi. p. 58.

CASE XXVIII.—*The placenta separated and pulled away before the child; transverse presentation; turning.*—"Some time ago, Dr. Löwenhardt of Prenzlaw met with a case where an ignorant midwife had separated the placenta, which presented all round, and removed it from the passages. She then discovered that the child was lying transversely, and attempting to turn it, she brought down an arm into the pelvis instead of a foot. The uterus being fully contracted, it was with difficulty that the author accomplished the turning of the child, and saved the mother."

Many years ago, Baudelocque related, in a footnote, in the second volume of his well-known System of Midwifery, an instance of the same kind.

CASE XXIX.—*Placenta extracted some hours before the child; arm and head presentation.*—"A midwife had extracted the placenta some hours before I was called, and had not been able to turn the child, whose arm was engaged below the head. The uterus, irritated by the manœuvres of the midwife, was strongly contracted on the child, and discharged but a few drops of blood. Astonished, after the extraction of the child, to see the cord was torn off near the umbilicus, and more surprised still not to find the after-birth in the uterus, I discovered that it had been extracted a long time before my arrival, and carefully concealed."[1]

In the five cases, the particulars of which have been last given, the placenta was artificially extracted a considerable time before the birth of the child, in simple ignorance, and hence in defiance, on the part of the attendant, of the established rules of management in unavoidable hemorrhage. In the first case that I have detailed in the present section, I pursued, as I have stated, exactly the same practice, as a matter of election, and from the belief, that, under the dangerous circumstances in which the patient was placed, this mode of treatment was the measure best calculated to suppress the extreme attendant hemorrhage, to gain time for the rigid cervix uteri to dilate, and to place the mother in the greatest relative degree of safety.

We have already seen (p. 604), that all practical authors in midwifery insist upon the artificial extraction of the child by turning, as the principal or only means of treatment in placental presentations, more especially when the presentation is complete. Almost all of them take occasion also, to inculcate under some form or other the supposed propriety of not detaching the placenta more than is absolutely requisite, when the operator introduces his hand into the uterus for the purpose of grasping the foot or feet of the infant. It is well known that, in order to follow out this principle to its fullest extent, various authorities advise that, either as a constant or occasional rule, the presenting part of the placenta should be perforated, rather than that more of the attachments of the placenta to the cervix should be separated during the operation;[2] and at all events, if the hand is introduced between the cervix and placenta, it is to be passed to one side only, and in such a way, as to produce as little detachment as possible between the two organs.

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[1] Baudelocque's Midwifery, Heath's Translation, vol. ii. p. 37.

[2] For the practice of perforating the placenta, "the obvious reason," says Dr. Rigby, is, "that by this means not more of the placenta may be separated than is necessary for the introduction of the hand, and consequently that as little increase of bleeding as possible may be produced by the operation; but if it be impracticable, as I have more than once found it, and it must ever be when the middle of the placenta presents to the hand, from the thickness of it near the funis, it must be carefully separated from the uterus on one side, and the hand passed, till it gets to the membranes."—Essay on Uterine Hemorrhage, p. 61. Dr. Foster inculcates the perforation of the placenta under all conditions as the best method of proceeding to turn in unavoidable hemorrhage.—Principles and Practice of Midwifery: London, 1781, pp. 231-32. In one instance, Smellie tells us, he was obliged to employ it (vol. iii. p. 163). Richter, in his Synopsis Praxis Medico-Obstetricæ, p. 176, attributes the first recommendation of this practice to Deleurye and Mohrenheim. Deleurye (Traité des Accouchemens, p. 368), argues, on the contrary, strongly and sensibly against the practice. Mohrenheim did not publish his "Abhandlung über die Entbindungskund," till 1791.

While all thus perfectly agree upon the general rule, that the child should be extracted before the placenta, at the same time a self-evident and necessary exception to this practice is mentioned by Guillemeau[1] (who was himself among the first, if not the very first, to inculcate the necessity of turning in placental presentations), and by Mauriceau,[2] Daventer,[3] Roederer,[4] Ould,[5] Pugh,[6] Wallace Johnson,[7] and other authors.[8] Occasionally, by the time that turning seems required, or before the operator is called upon to pursue this measure, the placenta has been already separated by the uterine contractions, and is so far protruding or pushed down into the vagina, as to fill up that canal, and prevent the easy introduction of the hand, or the ready extraction of the infant. Under such a complication, the authorities I have named unite in stating, that we must, as a matter of necessity, first remove the detached and obstructing placenta, in order to have the maternal passages sufficiently clear for the operation of turning.

The cases and circumstances under which this point of management has been advised are totally and essentially different, and founded upon a different principle from those in which I venture to recommend the extraction of the placenta before the child. A quotation or two from one or more of the authors to whom I have referred will point this out. I shall select for this purpose the observations of Mauriceau and Roederer, as being at once the simplest and the most explicit on the matter.

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[1] Les Œuvres de Chirurgie, p. 320, Rouen, 1649. Guillemeau's rules for the management of cases of placenta prævia are so original, explicit, and brief, that I shall quote them here in full.—"Il faut observer deux choses. La *prémière* est de considerer si ledit arrière-fais est *peu* ou *beaucoup* advancé et sorty: car estant *peu* advancé (apres avoir bien situé la mere) il sera remis et repoussé le plus diligemment que faire se pourra; et si la teste de l'enfant se presente, elle serra conduite droict au couronnement, pour aider à l'accouchement naturel. Mais s'il se trouve quelque difficulté, et que l'on apperçoive que ladite teste ne se puisse tost avancer, ou que la mere ou l'enfant, ou tous deux ensemble soient debiles; prevoyant que l'accouchement soit long, sans faute le plus expedient sera de chercher les pieds de l'enfant, comme nous avons dit, et le tirer doucement par iceux. *L'autre* point a observer, est si ledit arriere-fais est *fort sorty*, et qu'il ne se puisse remettre, tant pour sa grosseur que pour le flux de sang qui l'accompagne ordinairement; joinct aussi que l'enfant le suit de presse, et ne demande qu'à sortir et venir au monde, il faudra, tirer du tout ledit arriere-fais; lequel estant tiré et sorti sera mis à costé sans couper le boyau qui est adherant à iceluy; Car par la guide dudit boyau l'enfant se trouvera lequel s'il est viuant ou mort sera tiré par les pieds le plus dextrement que faire se pourra."—In the Provincial Medical and Surgical Journal for April 2, 1845, Mr. Blenkinsop has inadvertently published a literal translation of the above passage into old English, as a piece of original writing, by Dr. Percival Willoughby.

[2] Diseases of Women with Child; Chamberlen's Translat., p. 220.

[3] Art of Midwifery, p. 154. [4] Elemens de l'Art des Accouchemens, p. 368.

[5] Treatise on Midwifery, p. 76. [6] Ibid., p. 113. [7] System of Midwifery, p. 33.

[8] Smellie (Cases, &c. in Midwifery, vol. ii. p. 307) states a case in which he followed this practice, the head being "hindered from advancing by the placenta." At p. 315, he observes, "I have had cases where the placenta has come down into the vagina before the child's head, and was *obliged* to deliver it first." It is remarkable that later authors should not generally mention a rule so obvious as that spoken of in the text. Yet among the writers of the present century, Petit is the only one, so far as I at present recollect, who even adverts to it.—See his Traité des Maladies des Femmes, tom. ii. p. 23, 1806.

"If," observes Mauriceau—and I give as nearly as possible a literal translation of his words, "the placenta only presents at the os uteri without passing out, and the membranes are still entire, as sometimes happens, the accoucheur will push aside a little the part of the placenta that presents, till he reach the membranes, which he will immediately rupture with his fingers, to allow the waters to escape, and at the same time to turn the child, if it presents by any other part than the feet; by which he ought immediately to draw it out. For it must be observed, that the after-birth, which presents first, is now nothing more than a foreign body in the womb, when it is entirely separated from it, as it is now; and that in this case one ought, as it would seem, to draw it out before the infant. Nevertheless, as it is strongly attached to the membranes surrounding it, one would not easily accomplish this; for we could not pull away the body of the placenta, without at the same time pulling away the membranes that surround the body of the child; besides, the membranes that line the whole of the interior of the womb, from their smooth and polished surface, allow the child to be more easily turned, and prevent by their interposition the womb from being easily injured during the operation; which would not succeed so well if the placenta were just pulled away. For these reasons it is much more sure to draw away the infant first; which is, moreover, on these occasions, so feeble, that it would soon die, unless promptly assisted. *But if the surgeon find that the after-birth is almost completely escaped from the womb*, and that its membranes have been almost entirely broken up and torn, *in this case he ought to draw it out;* for, besides that it would now be useless to push it back into the womb, it would very much incommode the surgeon in his operation, and make him lose time in assisting the child."[1]

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[1] Maladies des Femmes Grosses, &c., tom. i. p. 332. Speaking of Mauriceau's opinions and practice in placental presentation, Dr. Lee observes, "The rules for the treatment of these cases are laid down with the greatest precision. When the placenta was entirely separated, then only did he consider it as a foreign body, and recommend its extraction before the child; but to this practice, he states, as an obvious objection, that the placenta is strongly attached to the membranes which surround it, and that it cannot be drawn out without the membranes enveloping the body of the child being drawn out also. Mauriceau has related seventeen cases of uterine hemorrhage in the later months of pregnancy from presentation of the placenta, and in sixteen of these, delivery was accomplished artificially by passing the hand through the opening formed by the separation of the placenta from the uterus, rupturing the membranes, and turning the child. Two women died after this operation, and one who would not consent to have it performed, died undelivered."—An Historical account of Uterine Hemorrhage in the latter months of Pregnancy, by Dr. Robert Lee, Edinburgh Medical and Surgical Journal, 1839, p. 332.

Roederer, according to the just and candid remark of Dr. Edward Rigby, "stands pre-eminent as being the first author who gives a distinct and complete description of this [the unavoidable] species of hemorrhage."[1] After stating the propriety of passing the hand between the cervix and placenta, and promptly extracting the child by turning, when the hemorrhage is great, he adds, in relation to the point I refer to, "If the placenta is entirely detached, and is arrested in the vagina, and opposes the passage of the hand, it ought to be brought out with the clots that fill the vagina. But if the hand can be introduced, the placenta should be left, that it may shut up the passage by which the blood and liquor amnii which distend the womb, might escape. In all cases when it is adherent to the womb, in whole or in part, it is most advantageous both to mother and child to leave it."[2]

The preceding extracts show that, as I have stated, the cases and circumstances under which the removal of the placenta before the child has been recommended by some of the older authorities, are entirely different from those in which I wish in these pages to insist upon its propriety. I advise its separation in cases in which it is still attached to the cervix, and often still contained within the undilated os uteri; they advised its removal only in cases in which it was already separated from the cervix, and expelled through the dilated os uteri. They began their practice of removing the placenta at the very point at which I would generally end all interference with it, viz., *after* the placental mass was completely detached. I would employ its artificial detachment as a measure of election and choice; they resorted to its removal from the passages, after it was detached by nature, merely as a measure of self-evident necessity and compulsion. I recommend its separation upon the *pathological* principle of arresting the existing hemorrhage, and so far cancelling the immediate source of danger to the mother; they recommend its abstraction on the *physical* principle of clearing the obstructed maternal passages, and gaining more free space for the operation of turning. They removed the placenta in order to be able to have recourse immediately to turning. I would remove it, in order to prevent the necessity of having recourse at all to that operation.

After having become interested in the investigation of the present subject, I made a somewhat extensive research among obstetric works and essays, with the double object of ascertaining the average mortality to the mother in placental presentations under the common modes of management, and with the view of attempting to ascertain if any author had previously practised, or proposed as a plan of treatment, the method which had suggested itself to my mind; of artificially separating, and, if necessary, of removing the placenta instead of the infant, and operating thus *not* upon the child, but upon the after-birth. The results of the first part of the inquiry I have given in a preceding page. In reference to the second point, I was long under the belief, that I was original in the idea of a practice, which, so far as I was myself concerned, was in the first instance the result of simple reasoning upon the data afforded by two cases that I had personally observed, and by the histories of a few others that I had read of, or collected.

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[1] System of Midwifery, p. 252.

[2] Elemens de l'Art des Accouchemens, p. 368, Paris, 1765.

At the time I laid a summary of the present paper before the Edinburgh Medico-Chirurgical Society in December last, I had not met with any cases or remarks which altered my opinions in this respect. I stated at the time that I was the more surprised at such a result, for, believing as many of the older authorities did, that before the placenta could be found presenting at the os, it had already become detached, and fallen down from the higher parts of the uterus, I fully expected to meet with some of them recommending its removal at an early stage of the labor, if not upon the principle of arresting the attendant hemorrhage, at least for the purpose of clearing away a supposed foreign and obstructing body. They appear to have been generally intimidated from following such a line of practice by the dread of injuring the unruptured membranes, and through them the inner surface of the uterus, as intimated in the quotation which I have already made on this point from Mauriceau. And hence also the removal of the placenta, when it was once found completely, or almost completely, protruded from the uterus, and obstructing the vagina so as to impede turning, was justified, on the similar ground that before this could happen, the membranes were necessarily torn, and consequently no evil effects would now ensue from this mode of interference.

Since these observations were communicated to the Medico-Chirurgical Society, I have fallen in with two cases recorded by one author, and a suggestion incidentally offered by another, both of which bear, as it appears to me, so importantly upon the history of the practice which forms the immediate subject of these remarks, that I shall make no apology for dwelling briefly upon them.

During my former researches among the older authors, I did not examine in detail, as I certainly ought to have done, the cases of placenta prævia detailed in the work of the celebrated Paul Portal. In drawing up the table of maternal mortality in placental presentation (see page 601), I had predetermined to take the returns of which it consists, from such authors or sources only, as mentioned upwards of at least ten cases, in order that I might arrive at more certainty and statistical truth in the calculation. I took it for granted that Portal's cases did not come within this range, in consequence of Drs. Rigby,[1] Lee,[2] and others, erroneously speaking of them as being only eight in number. On the other hand, I knew well that this accurate and original observer was acquainted with the fact, that in placental presentations, the placenta was originally fixed over the os uteri, and had not fallen down there after its attachment to the fundus. Hence, I did not expect to meet among his cases with any instances in which the placenta had been artifically extracted before the child as a foreign body, or any deviation made from the treatment usually followed. In two cases, however, which he has described, he most distinctly adopted the practice of detaching the whole placenta immediately before extracting the child. As these cases appear to me to be in various respects, not only historically, but practically, interesting, I shall take the liberty of quoting them at full length. They afford strong additional instances of the success of the practice which it is my wish to inculcate.

CASE XXX.—" *The Delivery of a Child with the after-burthen foremost.*—April the 7th, 1672.—I was called to a woman in St. Mederic's Street, being in the sixth month of her reckoning, and troubled with a violent flux of blood. I told her immediately, that without being delivered forthwith, she was in danger of her life. Dr. Cresse, a Paris physician, was of the same opinion, and ordered her immediately to be let blood; however, we staid a little to see whether nature would help itself, it happening sometimes that such fluxes cease when the child comes to the birth well turned. But the woman, growing weaker and weaker, her husband and friends asked my opinion once more, and I answering, that the delivery of her was the only way to secure her life, they desired me to delay no longer.

"Whereupon I brought two of my fingers, well greased with butter, into the inner orifice of the womb, and, finding the same opened to the bigness of a French half-crown piece, I spread my fingers in the nature of a screw, and thus extending it, brought in my hand, and felt the after-burthen foremost. I separated the same to open my way to the membranes, which being opened, the water came forth, and I drew out the after-burthen, that it might not be in the way afterwards.[1] Then, searching after the child's feet, I took hold and pulled out one, which I baptized; and, wrapping a cloth round the foot, I accomplished the delivery of a daughter of six months, after my usual method. Immediately after the woman recovered of her swooning fits, she began to breathe and speak, telling us that she felt herself something stronger, but relapsed soon after; yet, being recovered again, by the care of Dr. Cresse, she did very well afterwards. We applied a plaister against the flux, made of the flower of gips used by the plaisterer, mixed with the best vinegar, laying it to the belly above the womb, and above the hips. The next day we prescribed a clyster of barley and white bread, boiled together, and mixed with three ounces of honey of violets; but the nurse, mistaking the matter, took honey of roses instead thereof; whence, it is obvious how careful women ought to be in choosing their nurses, since, by the management of clysters only, many dangerous symptoms have ensued through the carelessness or ignorance of the nurses. Wherefore women ought to pitch upon such nurses as are used to attend in lyings-in, and such as they know, that will not be employed in the small-pox and other infectious distempers, which thus may be communicated to the woman in childbed. All nurses ought to be cautious not to give medicines to women in childbed, without the advice of a physican, surgeon, or expert midwife; and especially not to apply astringent remedies in the beginning of the flooding, which, if it succeeds ill, they are sure to lay the blame on the man-midwife, or midwife. 'Tis but lately that I knew such an ignorant and presumptuous nurse, who, in the beginning of the flooding, washed the gentlewoman under her care with red wine only, which, stopping the flooding, &c., fever ensuing, the patient was forced to be let blood several times in the arm and foot, and yet they had much ado to save her."[2]

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[1] "In Portal's cases in Midwifery there are eight in which he was under the necessity of delivering by art, in consequence of dangerous hemorrhage, and in all of them he found the placenta at the mouth of the womb."—Rigby's Essay on Uterine Hemorrhage, 6th edit. p. 22.

[2] "Portal's Treatise, 1685, contains an account of eight cases of uterine hemorrhage, in which he found the placenta not merely at the mouth of the womb, but adhering to the whole neck of the uterus. In several of these cases, he felt the placenta adhering all round to the internal orifice of the uterus. In those cases the treatment employed by Portal did not differ from that which had been employed by Paré, Guillemeau, and Mauriceau, the propriety of artificial delivery by turning being then as completely established as at the present time."—Lee's Clinical Midwifery, p. 140.

CASE XXXI.—"*The Delivery of a Woman affected with a most violent flux of blood.*—January 14, 1679.—I was called out in the morning at four o'clock, to deliver a gentlewoman in the street called Gervais Laurent, at the foot of the Bridge of Our Lady, in the parish of the Holy Cross. This gentlewoman being about eight months with child, was seized with a most violent flux of blood, which having continued for ten or twelve days, she was reduced to a miserable condition. Upon search made with my fingers (well greased), I found the whole vagina or passage filled with clods of coagulated blood, notwithstanding which the flux continued. As soon as I had brought out the clods of blood, I conveyed my fingers further into the orifice of the womb, which I found very thin and soft, and so wide, that I could put in three of my fingers foremost. I searched with one finger first, and found the after-burthen foremost, and closely joined round the inner orifice of the womb, which was the occasion of the excessive flux of blood; and, as it had reduced the woman to a very low condition, so this, joined to the other circumstances, made me fear the life both of the woman and child. I desiring, therefore, the assistance of a physician, Dr. Biendisant was sent for, who, finding the poor patient not in a condition to dispense with many remedies, prescribed her only a mixture of purslane and plantain water, three ounces each, half a drachm of the confection of hyacinths, without musk; prepared pearls, one scruple, mixed with an ounce and a half of the syrup of pomegranates. This stopped the flux for a while, yet not quite; so that her weakness, increasing every minute, it was resolved to have her delivered, notwithstanding the great danger which must needs attend it; but considering her death was infallible, unless she was delivered, I went to work; though as I laid her across the bed, there appeared in her not the least signs of sense or motion, so that everybody concluded she would die under my hands. However, I found some means to convey my hands (well greased before) into the inner orifice of the womb, where I again felt the after-burthen fastened to it, as I had told Dr. Biendisant before. I peeled it off by degrees, and brought it out; and then, turning my hand again in the womb, the first thing I met with was the navel-string, along which I guided my hand first to the child's belly, and then downwards to the thigh, and hence lower to the leg and foot, which I brought out and baptized.[1] Whilst I was pulling this foot, the other followed, and the whole body after it, as it has been observed frequently before. The child being quite alive, the parish priest of the Holy Cross (who had before administered the sacrament unto the mother) had the opportunity of baptizing it, though contrary to his and all our expectations. Immediately after the delivery, the woman recovering in some measure her senses, Dr. Biendisant prescribed the following cordial: of succory and bugloss water, three ounces each; of the confection of alkermes without musk, half a drachm; of prepared pearls, one scruple; and syrup of maidenhair, two ounces. About two hours after, she was ordered to take half a cupful of broth only, for fear of overcharging her stomach. She was much better the next day, yet not without some symptoms of a fever, against which a clyster was ordered, made of the decoction of the cooling herbs, with three ounces of honey. The second day after the flooding stopped, she recovered again by the use of the beforementioned cordial. However, she complained of a tension and pain in her belly (which was much swelled), as also in her hips, wherewith she had been affected before her delivery: she was also afflicted with a violent pain in the head (the ordinary symptom of all excessive fluxes of blood), which continued even after her lying-in, with an intermittent fever: she was also troubled with frequent bilious dejections, or a looseness, against which we prescribed her clysters of the decoction of emollient herbs, with lintseed; yet, after some time this woman recovered her health, except that three weeks after her lying-in, she lost the sight of one of her eyes by a violent defluxion, which by all the art that could be devised, was never removed thence. The cause of this disaster[1] I attribute to a sharp, viscid, and bilious humor, contained in the veins, which being put in motion by the violence of the pain this woman suffered during her labor, and the anxiety she lay under, was carried upwards and settled in her head."[2]

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[1] I shall quote the original French of this important passage: "Je glissay ma main dans l'entrée de la matrice, òu je sentis l'arrière-faix qui se presentoit. L'ayant separé, afin de me frayer le chemin, je sentis les membranes des eaux que je perçay, et les eaux s'estant écoulées, je tiray l'arrière-faix le prémier, afin qu'il ne m'incommodast point à la sortie de l'enfant."—La Pratique des Accouchemens, par Paul Portal, Paris, 1685, p. 203.

[2] Compleat Practice of Men and Women Midwives, by Paul Portal, p. 148, Case 43. London, 1763.

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[1] "Je separay tout doucement cet arriere-faix, et je tiray dehors; ensuit je glissay ma main dans la matrice. La premiere partie qui se presenta, fut l'ombilic que je suivis jusques au ventre, apres lequel je suivis la cuisse jusques a la jambe et au pied, que je tiray à l'orifice externe de la matrice sur lequel je jettay de l'eau, pour ondoyer l'enfant sous condition."—See original French edition of Portal's work, p. 293.

In addition to the two preceding cases, Portal gives a detailed account of six other placental presentations which came under his care, and mentions six others that he had seen, but of which he does not give the full particulars.[3] In one of these cases the uterus contracted so strongly, as to push the child through the placenta. The mother recovered. In all his other cases he seems to have introduced his hand, and turned the infant; but in none of them, with the exception of the two we have already quoted, does he state such particulars as to show whether, as in them, he entirely separated the placenta immediately before turning the child, or only partially separated it in that position, so as to enable his hand to pass into the uterine cavity, for the purpose of practising version of the infant. Unfortunately he in no passage states to us in the way of remark, or otherwise, what special practice, or principle of practice, he recommended or followed in placental presentations, and in only one part does he throw out any hint of the reason which induced him to separate the placenta in the two cases I have cited. The hint in question I have already had occasion to quote, as given incidentally in the details of the first case which I have already cited (see Case XXX.) "I drew out," he observes, "the after-birth first, that it might not incommode me during the passage of the infant. (*Je tiray l'arrière-faix le premier, afin qu'il ne m'incommodast point à la sortie de l'enfant*")[1] And in all probability this was his sole and only reason for the practice. For if he had separated the placenta with any idea whatever of the principle under which I have recommended it, namely, that of totally arresting the violence of the attendant hemorrhage, and averting the principal or only danger to which the patient is subject in placental presentations, he certainly would not have practised what he tells us he did—the instantaneous extraction of the infant afterwards.

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[1] This is perhaps the earliest case on record of that *Phlebitic* or *Puerperal Opthalmitis* to which the attention of the profession has been particularly called in our own day by Drs. Hall and Higgenbotham, Dr. Locock, Dr. M'Kenzie, and others. In attributing it to a "humor contained in the veins," Portal almost forestalls the pathology of the disease accredited at the present day. An instance of this destructive ophthalmia occurred two years ago, in the practice of my friend Dr. Graham Weir, after, as in Portal's case, a placental presentation. The patient also ultimately recovered. I know of a case in which both eyes were affected. Some years ago, in a case of placental presentation, in which I turned the child, I saw come on in a different part, namely, the region of the parotid gland, a secondary inflammatory deposit from "a sharp humor contained in the veins, and carried upwards and settled in the head." I have met with one other case of fatal *Puerperal Parotitis*. (For further observations on this subject, see Section on Pathology of Puerperal State, in vol. ii.)

[2] Compleat Practice of Men and Women Midwives, p. 214, Case 69.

[3] Ibid. pp. 29–105, 107, 135, 143, 166, 169, 250.

I have already (at page 608) mentioned some particulars of a very interesting case of expulsion of the placenta before the child, detailed by Mr. Chapman, surgeon at Ampthill, Bedfordshire, and reported by him in the 4th volume of Dr. Duncan's Annals of Medicine, published in the year 1800. My attention has been particularly drawn to three brief remarks that Mr. Chapman appends to his case. One of these remarks is specially deserving of note, from containing, so far as I am aware, the first explicit suggestion as to the proper principle of treatment in some placental presentations. I shall quote it in the author's own words. "From the expulsion of the placenta to the birth of the child was full four hours. She (the mother) lost little or no blood. How far does this suggest a different practice to that in general followed? I mean that of delivering the placenta previous to delivering the child, in those cases of alarming hemorrhage where the placenta is situated on the side of, or over, the os uteri."[2]

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[1] Pratique des Accouchemens, p. 209.

[2] Annals of Medicine, vol. iv. p. 308.

Up to the time at which I communicated the views contained in the present memoir to the Medico-Chirurgical Society in December last, the two cases that I have quoted from Portal, and the preceding interrogatory of Mr. Chapman, contain, so far as I have been able to learn, all that had been put upon record with reference to extraction of the placenta before the child, in placenta prævia. Since, however, the first part of the present paper was printed in the Monthly Journal of Medical Science, I have obtained such information as to convince me, that several years ago, a medical teacher and practitioner of the highest distinction in Manchester was aware that the total separation of the placenta in unavoidable hemorrhage was capable of arresting the attendant flooding, and adopted this practice as a measure of treatment in one of the classes of cases that I shall have occasion to consider under the next section, namely, where the woman is too exhausted to allow with safety of the operation of turning. The gentleman I allude to was the late Mr. Kinder Wood, who for some years was lecturer on Midwifery in the Manchester Medical School. He died in 1830. His opinions on this matter were never, so far as I have been able to ascertain, known or divulged beyond the range of his own immediate friends and pupils; and one of his own colleagues in the Manchester Lying-in Hospital, Mr. Wood, confesses he was unacquainted with his views till they were mentioned to him within the last few months.

In a short and interesting biographical account of Mr. Kinder Wood, published in the London Medical Gazette for 1830, it is stated that he left behind him a volume of midwifery essays all but ready for the press.[1] One of these papers was entitled, "An Essay on Uterine Hemorrhage, and the best mode of treatment in alarming cases of this kind." It is deeply to be regretted that this volume has never been given to the public, as the opinions of so acute and able an observer as Mr. Kinder Wood on any points connected with the obstetric profession, are almost certain to be sufficiently valuable to have entitled them to a better fate. Dr. Radford, who succeeded Mr. Kinder Wood as lecturer on midwifery in the Manchester School, purchased, as I am informed, the manuscript volume I allude to, after the author's death, and has lately published the following observations and cases of Mr. Kinder

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[1] The biographer, Dr. Bardsley, states: "Mr. Wood had been employed for some time in collecting materials for a separate volume on midwifery, embracing the consideration of some of the most important points connected with the practice of the art. The essays on 'Uterine Hemorrhage, and the best mode of treatment in alarming cases of this kind;' on 'Rupture of the Uterus during Labor;' on 'Inversion of the Uterus;' and on 'Impracticable Labor from Distortion,' are left in such a state of readiness for the press, as to require only a few verbal, and other trivial alterations, before being submitted to the public eye, should this be the intention of Mr. Wood's family."—London Medical Gazette, vol. vii. p. 624.

Wood, relative to unavoidable hemorrhage, probably from the manuscript in question. I have very great and sincere pleasure in inserting them here, at length, as a piece of posthumous justice to the memory of a man of distinguished professional attainments.[1]

"If we find so much exhaustion as to make us fear the effect of further hemorrhage, during artificial delivery, the first step after passing the hand, must be to detach the whole of the placenta; by this, hemorrhage will be completely suppressed, for the effect of passing the hand through the os uteri, and throwing off the placenta, will always be to produce so much contraction as to arrest the bleeding from the small decidual or uterine mouths. It is satisfactory to know that the child is rarely living in these cases of exhaustion, its blood being poured out through branches of the placental structure, along with that of the mother; and when brought down, its appearance, like that of the mother, is bleached and exsanguined. The time required to separate the placenta is very short, and the loss of blood during the attempt exceedingly trifling. I know from experience, that when the placenta is wholly detached, the hemorrhage will cease."

CASE XXXII.—"I was desired to see Mrs. Clayton, Lad Lane. The hemorrhage had been going on several days, under the inspection of a female midwife, without any assistance or advice. The discharge had been excessively profuse, and was coming away in gushes, with slight pain, every two or three minutes. She was extremely exhausted; of a deadly paleness, very cold, with a quick and feeble pulse; the os uteri was moderately dilated, the cervix obliterated, and the placenta presenting. I feared the effect of hemorrhage, which must occur during the act of delivery in the common manner. The patient was placed in a proper position, and a little brandy and water exhibited, and the placenta completely detached, as the hand dilated the os uteri, and before reaching the feet of the child. No hemorrhage succeeded the separation. The patient recovered favorably, but slowly. October 4, 1821."

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[1] "Since I communicated the present memoir to the Medico-Chirurgical Society, Dr. Radford has himself given and published a lecture on galvanism, in which, among other ingenious suggestions, he proposes the detachment of the placenta to be adopted in conjunction with galvanism, in cases of placenta prævia."—Provincial Medical and Surgical Journal for December 24, 1844. In this lecture he limited the practice in question to cases of "exhaustion"—as Mr. Kinder Wood had done. Latterly, however, in the same Journal (see Number for January 22, 1845), he has adopted more fully, and published, my previous ideas of applying the detachment of the placenta to other cases of placenta prævia, besides those in which there is simple exhaustion, erring, however, in this, that he conceives the practice is one which cannot be attempted until the cervix and os uteri will safely allow the introduction of the hand. Under the next section we shall find that this would exclude an important range of cases to which the practice is especially applicable. I had an opportunity of stating my views on the artificial detachment of the placenta to Dr. Radford when he made a visit to Edinburgh last year. Dr. Campbell, the well-known Lecturer on Midwifery in Edinburgh, and a friend of Dr. Radford, heard the subject discussed between us. When writing lately upon this matter, Dr. Campbell observes, "It does strike us as remarkable, that Dr. Radford, while on a visit to this city something less than a year ago, did not, when this subject was the topic of conversation, make known his knowledge of Mr. Kinder Wood's views."—The Northern Journal of Medicine for July, 1845, p. 90. See further the Provincial Medical Journal for Feb. 5th, Feb. 26th, and March 19th, 1845. See also London and Edinburgh Monthly Journal of Medical Science, Feb. 1845, p. 161.

Case XXXIII.—"I was desired to see a poor woman in Newberry Street. I found from the female midwife in attendance, that the discharge had been going on long and copiously. The poor patient was extremely exhausted. Feeling that she could not survive long if left to nature, and that she could not bear the hemorrhage consequent upon the common operation, I separated the placenta, brought down the feet of the child, and delivered. The effusion produced by separating the placenta was extremely slight, and it ceased upon effecting the complete detachment. The child was dead, and tending to putrefaction. A stimulant was given before the operation, and during its continuance, but the heart never recovered its energy nor the skin its warmth. She died in about an hour, from pure exhaustion. November 1, 1821."

Case XXXIV.—"I attended Mrs. T., Newton Heath. She was very much exhausted. It was obvious she could not bear the loss of blood consequent upon the ordinary delivery. A cordial was now administered. The hand was introduced, and the placenta, which was adherent over the os uteri, was completely separated, the membranes ruptured, and the feet seized. The child was easily delivered. She only survived the operation a very short time, although the hemorrhage ceased from the moment the placenta was detached."

Case XXXV.—"I was called to Mrs. B., January, 1822, aged 25, St. James's, who was violently flooding. I found her cold, and her pulse scarcely perceptible; os uteri partially dilated and soft; brandy and water given; the discharge still continued; she was much exhausted. The hand was introduced, the placenta detached, and the membranes ruptured. There was no further discharge; but she died in a few hours afterwards, although stimulants were freely administered."

Case XXXVI.—"On the morning of the 11th April, 1822, I was desired by a female midwife to see Mrs. Rawson, in Little Lever Street. She was in the latter end of the eighth month of pregnancy. She had sustained frequent and copious discharges the two previous months, and which were very profuse during the night. The pains were slight, but always attended with fresh discharge. The patient presented a very distressing appearance. The pulse could not be counted; the lips were white; she was very cold, and spoke in a whisper; she had frequent syncope. Warm brandy was freely administered. The patient was placed with the utmost care slowly and gently on the side, and upon making an examination, the os uteri was found low, soft, and dilated about the size of half a crown, the cervix was obliterated, the placenta was found over the os uteri, one portion was loosened. Convinced that the patient could not bear immediate delivery, and satisfied that the hemorrhage would be fatal very early if she was left to nature, I insinuated the hand through the os externum and os uteri, detaching the placenta by sweeping the fingers beneath it, as the hand was passed forwards. The soft parts yielded freely, and the operation was done quickly and easily, and with very trifling loss of blood. The presence of the hand excited uterine contraction, and seemed to rouse the languid patient. A stimulant was given during the operation. The head was found presenting. After the placenta was separated, the membranes were ruptured, and the hand slowly withdrawn. She was ordered to remain in a state of perfect and complete rest, to take light support, and a stimulant mixture every three hours. The labor pains became stronger about six hours afterwards, and in an hour expelled the placenta, and a dead child, tending to putrefaction. No hemorrhage occurred when the hand was withdrawn, after detaching the placenta. The patient regained health slowly, and had a severe attack of phlegmasia dolens.

"In some cases I have been called to attend when the ordinary method of delivery has been adopted, the patients died, and this led me to modify the practice, which I adopted in some of the above cases by detaching the placenta, rupturing the membranes, and then delivering the child; but after due consideration, I was again induced to vary my plan; and in those cases where we can have no hope of saving the patient if we proceed to delivery, however well the operation be conducted, I have no hesitation in recommending that the placenta be separated completely, and the membranes ruptured, that the hand be withdrawn immediately upon this being effected, leaving the child and placenta behind. By this practice, the patient will be placed precisely in the situation which occurs in the most favorable cases of recovery [delivery] by the natural efforts. I conceive no fact in midwifery rests upon a more solid foundation, than that this hemorrhage will cease upon separating the placenta, and by this practice the patient is placed in as favorable a situation as is possible for recovery. Time will be gained to support her by proper means, and which can be used with greater freedom, as the hemorrhage is infallibly suppressed by this operation."

On communicating, a few months ago, my ideas upon the subject of this memoir to my esteemed friend Dr. Beatty of Dublin, he favored me with the following account of a very interesting and illustrative case from the note-book of his late father. It is dated July 28, 1835 (1825 ?)

CASE XXXVII.—*Placenta probably entirely separated for about twenty-four hours before delivery; hemorrhage arrested by it.*—"The practice I adopted in this case was such as I never tried before, nor heard of, and from its success I think it necessary to note it. This lady sent for me at four o'clock on the morning of the 27th. I found that she had been flooding excessively, and, as the nurse said, she was in a sea of blood. She was within three weeks of her full time. I examined and found the placenta directly over the os uteri. I sent for Dr. Duke, and on his coming, I introduced my hand into the vagina, and, finding I could not turn without more difficulty than I thought necessary, it occurred to me to separate as much of the placenta as was within my reach. This I did to such an extent as entirely stopped the hemorrhage, except a trifling oozing. She continued all day without pain, and about three o'clock the following morning, labor set in, and she was delivered of a still-born child at half-past four o'clock, A. M. She had no further flooding, and is likely to do well."

Since the publication of the first part of the present essay, three instances have been published in the Medical Journals, in which the proposed practice was adopted—one by Mr. Wilkinson of Spalding, another by Dr. Walker of Chesterfield, and a third by Dr. Maclean of Edinburgh. I shall append an abridged detail of these cases.

CASE XXXVIII.—*Flooding suppressed by the artificial separation of the placenta; patient delivered in an hour and a half afterwards.*—Mr. Wilkinson was called, on the 7th June, 1843, at 12 P. M., to a woman between six and seven months advanced in pregnancy. "Three weeks previous to my being sent for," says he, "there had been, I was informed, very great hemorrhage, which had continued, more or less, up to the time of my seeing her. On the evening of the 7th, it had been very considerable, and previous to my seeing her, excessive. I found the os uteri dilated to the size of something less than a five-shilling piece; the placenta presenting; the hemorrhage excessive; the pains very feeble. She was greatly exhausted; the pulse scarcely perceptible; the countenance blanched, and I found that she must sink. I directed some brandy and water to be got down immediately, and also a scruple of ergot of rye. I passed first three fingers, and, with as little delay as possible, the whole hand, into the uterus. The gush of blood was at first great; the placenta, however, was quickly and completely detached, and the hemorrhage *almost at once* ceased. I waited a while with my hand in the uterus; I then brought away the placenta, and immediately reintroduced my hand, with a view of bringing on contraction. The head of the child presented; I turned; but feeling that my patient was not in a state to bear immediate delivery, I waited an hour and a half. She then having somewhat rallied, I delivered. She remained during the first two or three days in a most exhausted state, from which, however, she gradually recovered. I feel satisfied," Mr. Wilkinson adds, "that had the usual plan been adopted in this case, so great had been the hemorrhage previously to my seeing her, that she must have sunk."[1]

CASE XXXIX.—*Exhausting flooding; placenta extracted, and flooding suppressed; arm presentation; turning.*—Dr. Walker was called to see Mrs. H—w, about four hours after labor with her sixth child had begun. She appeared in a most alarming state of exhaustion, exhibiting in an extreme degree all the symptoms consequent upon great loss of blood. "On making an examination," writes Dr. Walker, "I found the vagina filled with clotted blood, the os uteri fully dilated, and a large portion of the placenta presenting, nearly closing the orifice of the uterus. With some difficulty, I passed my finger round the anterior edge of the placenta, to ascertain the presenting part of the child, and felt what I thought was either a shoulder or the nates. The attempt increased the hemorrhage slightly; and, fearful of further reducing the already too exhausted powers of my patient, I desisted. Having previously determined to adopt the plan of treatment lately brought before the profession by Drs. Radford and Simpson, I proceeded to remove the placenta. Introducing my left hand, I completely, and in one mass, separated the placenta, which was immediately expelled with my hand into the vagina; after its complete removal, the hemorrhage, which before was considerable, *entirely* ceased. At this time, the pains were feeble, and not of frequent occurrence. At my next examination, I found that a hand had followed the placenta, and now presented at the os externum. With the usual precautions, version was easily accomplished, and the woman safely delivered of a stillborn child. The uterus contracted with tolerable firmness, and no farther hemorrhage supervened. On the day following, I found my patient comfortable, though suffering slightly from the effects of the hemorrhage, and in a few days she was perfectly recovered."[1]

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[1] Provincial Med. and Surg. Journal for July, 1845, p. 471.

CASE XL.—*The placenta extracted artificially before the child; rapid recovery of the mother.*—This case I have already alluded to as having happened in the practice of Dr. Maclean. The subject of it, Mrs. Nixon, was taken in labor with her eighth child on the 14th of June, 1845, at 4 P.M. The pains were trifling till about three next morning, when they became more frequent and severe, and were accompanied with a discharge of blood. The os uteri was found at this time expanded to the size of a shilling, but rigid and undilatable, with the placenta presenting over it, and a constant oozing of blood, which at each pain became much increased. The pains and discharge nearly ceased after the administration of a slight opiate, and using the ordinary means to check the hemorrhage. At 11 o'clock the pains and flooding returned; and an hour and a half after this, "I found," says Dr. Maclean, "the os dilated to the size of a half-crown, the placenta presenting, and protruding through it about one and a half inches. The hemorrhage, which had recurred with the pains, had caused such a degree of faintness and collapse, that the fatal termination of the case appeared inevitable; and the pains, which now came on frequently, from the great weakness of the patient, had but little effect in dilating the os uteri, and advancing the labor. Having ascertained," he continues, "by the stethoscope, that the child was dead, and Mr. Woodhead being again in attendance with me, it was at once agreed upon, in consultation, the mother alone requiring our immediate attention, and the state of collapse to which she was reduced rendering the forced delivery by turning exceedingly dangerous, whilst the evacuation of the liquor amnii had entirely failed even to moderate the hemorrhage, that I should immediately endeavor to suppress the discharge by separating the whole body of the placenta from the uterine parietes, when the patient might be allowed to rally a little before removing the child, supposing the views of Dr. Simpson to be correct. Accordingly, having administered a small quantity of spirits, with a few drops of laudanum, to the patient, I immediately introduced my hand into the womb, so as to remove the placenta. This I was easily enabled to do after dilating the os uteri, by pressing down the placenta, with the fingers introduced behind it, into the palm of the hand. A few minutes were sufficient to effect this; and I was much gratified to find all hemorrhage cease, as soon as *the whole placental mass was detached.* The placenta being carried down into the vagina, a dose of ergot was administered, and, in about a quarter of an hour, the natural pains expelled the child. There was no after-hemorrhage, and only slight lochial discharge. The mother recovered without the slightest drawback, and was out of bed in a few days."

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[1] Provincial Medical and Surgical Journal for July, 1845, p. 557.

"The above case," observes Dr. Maclean, "from the alarming symptoms attending it, and the certain fatality which must have ensued, had the flooding continued for a short time longer, with the immediate suppression of all hemorrhage on the placenta being completely detached, thus allowing time for the patient to rally, would seem to prove the operation of turning in these cases to be almost needless; at the same time that it affords another instance of the accuracy of the conclusions published by Professor Simpson in the London and Edinburgh Journal for March last, where he proposes in such cases to remove artificially the placenta, and not the child."[1]

After the present section was written, my attention was accidentally called to some interesting remarks, bearing directly upon the point of which it treats, and contained in the old and rare "*Observations sur la Pratique des Accouchemens*" of Viardel, printed at Paris in 1671. I append a literal translation of the passage in Viardel's work.

Case XLI.—"All these labors where the placenta presents or escapes entirely are very dangerous, as the infant often loses its life, as happened to the wife of Monsieur le Fèvre, merchant, living in the Rue de Gèvre, in whom the after-birth presented first, and occupied all the internal orifice of the womb. Being called, therefore, to deliver her, and finding matters in this state, as I discovered from examination, I pushed back the after-birth with the extremity of my finger, in order to return it into the womb, and having passed my hand into it as far as I could, I passed it around the internal orifice in order to assure myself, and thus discovered that it was the after-birth which was entirely separated from the womb (*entirement separé de la matrice*), and that the infant presented behind it by the umbilicus. After observing all these things, and being assured that it was the placenta, I prepared to succor her as quickly as possible, in the following manner: I placed her across the bed with the thighs separated and the heels drawn up to the hips, having first made her take a couple of eggs with a little wine, to strengthen her. I introduced my hand into the womb (as I have said above), and having arrived at the internal orifice, I grasped the placenta by its middle part with my open hand, and holding it firmly, drew it out of the womb, and, the moment it was out, I put back my hand to search for the feet of the child, and having found them I drew it out, dead. * * * As soon as I completed the delivery, the loss of blood, which had persisted till then, and all the other accidents, ceased; and I believe that the infallible prognosis in such a case is, that the child must be dead, although in truth the case is not always so, for it may happen in similar labors, that the infant being strong and vigorous, may escape shipwreck if it be promptly and timeously succored; and I may add here in confirmation of this, that I have met with a case of the same kind where the infant lived for three days, although very weak and feeble."[1]

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[1] Northern Journal of Medicine for August, 1845, p. 132.

III.

SECTION IX.—CASES OF PLACENTA PRÆVIA IN WHICH IT IS PROPER, 1. TO EVACUATE THE LIQUOR AMNII; 2. TO EXTRACT THE INFANT BY TURNING; AND, 3. TO SEPARATE AND EXTRACT THE PLACENTA BEFORE THE CHILD.—SERIES OF CASES TO WHICH THIS LAST PRACTICE IS APPLICABLE, VIZ.: 1. WHEN THE OS UTERI IS RIGID AND UNDILATABLE; 2. IN FIRST LABORS; 3. IN PREMATURE LABORS; 4. IN LABORS SUPERVENING EARLIER THAN THE SEVENTH MONTH; 5. WHEN THE UTERUS IS TOO CONTRACTED TO ALLOW OF TURNING; 6. WHEN THE PELVIS OR PASSAGES ARE ORIGINALLY CONTRACTED; 7. IN CASES OF EXHAUSTION; 8. WHEN THE CHILD IS DEAD; 9. WHEN IT IS PREMATURE AND NOT VIABLE.

The various cases adduced in the last section will probably be admitted on all hands to demonstrate sufficiently the practicability of the plan of treatment in placenta prævia which it is my object to bring before the profession in the present Essay. In the next instance, it is requisite for us to consider in what special cases of placental presentation it will be proper to adopt the principle of treatment in question.

I have already stated (see page 605) that I believe it would be found the just and legitimate mode of practice in those cases of placental presentation in which either the artificial evacuation of the liquor amnii is unsuccessful, or forced delivery by turning is inapplicable and dangerous.

To understand, then, thoroughly the varieties of cases in which we should adopt the complete separation and extraction of the placenta before the child as a line of practice, it will facilitate our inquiries if, in the first instance, we consider the class of cases in which, *first*, The evacuation of the liquor amnii ought to be followed, and, *secondly*, Those in which the delivery of the child by turning constitutes the suitable mode of treatment. Having cleared the way by considering these two points, we shall be more able to judge of and appreciate the remaining varieties of placental complication in which this new means of treatment ought to be adopted.

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[1] Pratique des Accouchemens, p. 90.

1. CASES OF PLACENTA PRÆVIA, IN WHICH THE EVACUATION OF THE LIQUOR AMNII FORMS THE PROPER PRACTICE.

The artificial evacuation of the liquor amnii appears to be principally followed when the placental presentation is partial only, and consequently, when a segment of the bag of membranes, as well as a segment of the placenta itself, is placed over the os uteri. When this practice has the desired effect, it forms undoubtedly the simplest and safest of all the means of treatment. But its applicability is limited. The proportion of cases in which the placenta is observed to present partially, would seem not to be so great as of those in which it presents completely.[1] Besides, when it does present partially, the rupture of the membranes and the escape of the liquor amnii would appear to be by no means so certain a mode of arresting the hemorrhage in this form of unavoidable as it is in accidental flooding.

"The method of Puzos[2] cannot," Baudelocque avers, "have, in these cases of partial placental presentation, those advantages which have been generally found in it when the source of the hemorrhage is further off (or of the accidental form). When the placenta is attached to the neck of the uterus, if the hemorrhage is suspended for a moment, when the waters are evacuated it soon appears again, and becomes so much the more abundant as the orifice of the uterus dilates further, and as the violence of the labor increases. I have met," he adds, "with but one case where the flooding has entirely ceased, after the evacuation of the waters, out of at least five-and-twenty where the placenta was attached to the neck of the uterus."[1]

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[1] Out of 50 cases of placenta prævia reported by Dr. Francis Ramsbotham, the placenta presented completely in 32, and partially in 18 instances.—Principles of Obstetric Medicine and Surgery, Appendix, p. 721.

[2] Puzos wrote a very beautiful memoir upon the artificial evacuation of the liquor amnii, as a means of arresting and treating cases of accidental hemorrhage (Memoire sur les Pertes, &c., et sur la Methode de procéder a l'Accouchement dans les Cas de Necessité, par une voye plus douce et plus sûre que celle qu'on a coutume d'employer.—Traité des Accouchemens, p. 323; or Mem. de l'Acad. de Chirurgie, 1743, tom. ii. p. 203). Hence this practice is often spoken of, as in the text, under the name of the method of Puzos. The honor, however, of first proposing and adopting this particular line of treatment certainly belongs to Mauriceau, who described and practised it half a century before Puzos (see his Maladies des Femmes, &c., tom. ii. cases 307, 450, 459, 479, 480, &c.) He even preceded Puzos in his explanation of the principle upon which the treatment acts in suppressing the discharge. "The vessels of the uterus, which," says Mauriceau, "were open, become shut by the contraction of its proper substance, as soon as the waters of the infant, which held it extended, are evacuated from it," tom. i. p. 334.—Daventer (Art of Midwifery Improved, p. 153, English trans., London, 1716), and Dionis (General Treatise of Midwifery, p. 244 of English trans., London, 1719), both also recommended the practice previously to the time of Puzos. "But if there are floodings," says Dionis, "from the separation of some part of the after-birth, however little soever the womb is dilated, the membranes which contain the waters must be broke, that the distension may be taken off, and that the after-birth may not be farther loosened, which both prevents the increase of the flooding, and makes way for the child's advancing into the passage, and being born the sooner."

The practice of evacuating the liquor amnii in partial placental presentations has generally, however, been found much more successful than it appears to have proved in the hands of Baudelocque. No obstetrician has probably, at the present day, had more opportunities of testing its value than Dr. Francis Ramsbotham of London. He informs us, in his Principles and Practice of Obstetric Medicine and Surgery, that he had attended, up to 1834, as many as forty-four cases of partial presentation of the placenta. In forty of these cases the membranes were ruptured some time before delivery. The results of the treatment of the artificial evacuation of the liquor amnii in thirty-nine of these cases were as follows:

In thirteen cases, *or in* 32 *per cent. of the whole*, the labor was afterwards terminated by the natural powers alone.

In twenty-six cases, *or in* 65 *per cent. of the whole*, in consequence of the hemorrhage not ceasing on the evacuation of the liquor amnii, turning was subsequently adopted. One was terminated by the forceps.

Eight of the forty-four patients died, four of them apparently from the excessive loss of blood suffered before delivery was effected.

The principal arguments that have been used by obstetric authors against the artificial evacuation of the liquor amnii in placental presentations, have been, first, Its alleged inefficiency in arresting the hemorrhage; and, secondly, The difficulty which the loss of the liquor amnii entailed or produced in the operation of turning, if that operation afterwards required to be adopted in consequence of the continuance of the flooding. These objections, but particularly this last one, will probably be entirely removed, by having the other expedient in abeyance, to which our attention is directed in the present essay, namely, the complete detachment of the placenta itself. The previous evacuation of the liquor amnii, whether by puncturing the membranes or by puncturing the placenta, would not interfere with any plan that it might be afterwards considered proper to adopt for the entire detachment of the placental mass. The knowledge of this latter alternative will certainly entitle us to have recourse oftener than has hitherto been done to the milder expedient, in the first instance, of evacuating the liquor amnii. It can be practised, when necessary, at a time when the os uteri is still so small and contracted as not to admit easily of the adoption of other measures. Little or no difficulty is in general encountered in its performance. When, from the placental presentation being partial, the membranes alone require to be perforated, the nail of the fore-finger, or the end of a surgeon's probe, or of a wire or pen, will suffice for the purpose. But, when the placental presentation is more complete, the instrument that is employed requires to be passed through its substance, and more care is required lest the placental structure be too much lacerated, or the fœtus itself wounded. The hair needle, which we have already (p. 603) found spoken of in this case by Daventer, the small trocar recommended for it by Deleurye, and the perforating instruments of Roederer,[1] Fried,[2] Ritzen,[3] Kluge,[4] &c.,[5] consisting of hollow tubes or canulas, blunt or rounded at the extremity, and provided with a puncturing lancet worked by a spiral spring, would probably none of them be so safe as other means with which the surgeon is more likely to be provided. The common surgical exploring needle might be used with a blunt, instead of a sharp-pointed, wire passed through it. Gendrin used successfully, and he states, "with facility," a common catheter. Probably its blunted extremity would, more readily than we might at first conceive, pass through the membranes covering the placenta, in consequence of the resistance opposed on their fœtal side by the distension of the liquor amnii. A common quill, with a lateral aperture like that of a catheter cut near its extremity, and another at the opposite end of the barrel, to allow of a free escape to the waters, would perhaps answer the purpose perfectly, could always be easily procured, and its blunt point, like that of the catheter, would not endanger the infant. Perforating the placenta with the fore-finger, and rupturing the membranes with the nail, might in the same way be used with similar results, and even with greater certainty of effect; but from what we have said of the source of the discharge in unavoidable flooding, it would seem a leading object not to lacerate, to any great and unnecessary extent, the substance of the placenta itself, as its maternal vascular cells might thus be opened up to a more dangerous degree.[6]

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[1] System of Midwifery, Heath's Translation, vol. ii. p. 37.

[1] Elementa Art. Obstetriciæ, par. 627.

[2] In Knauer's Selectus Instrumentorum, Tab. xxv. fig. 4.

[3] Scheibler's Dissertatio de rumpendis velamentus, &c.

[4] Die Anzeigen der Mechanischen Hülfen bei Entbindungen, &c., mit Kupfern, p. 436.

[5] See Kilian's Operationslehre für Geburtshelfer, 1843, vol. i. p. 248; and Siebold's Abbildungen der Geburtshulfe, p. 162.

[6] Médecine Pratique, tom. ii. pp. 350 and 352.

If the operation, in whatever way performed, failed, by not proving successful in sufficiently arresting the hemorrhage, it might still at least gain for us some time for the greater dilatation of the os uteri, and hence for the more easy separation of the placenta subsequently, and the more safe passage of the fœtus. Long ago, Deleurye stated the objects and advantages of the practice explicitly, in the following terms: "There are, notwithstanding, cases of placental presentation in which the placenta must be pierced: as, when it becomes necessary to terminate labor before the full term, without uterine contractions, in consequence of profuse flooding, and the fear of uterine inertia after this from the feebleness of the patient; then, with a thrust of a trocar, the evacuation of the waters is to be facilitated. The uterus, which by this evacuation ceases to be passively dilated, contracts and diminishes its volume, and we facilitate the time of election for delivering the woman with more safety."[1]

2. CASES OF PLACENTA PRÆVIA, IN WHICH TURNING FORMS THE PROPER PRACTICE.

I have already shown that several of the highest obstetric authorities look upon turning, and the artificial extraction of the infant, as the only advisable mode of treatment in every case of placenta prævia, and have quoted at length the opinions of Plenck, Denman, Merriman, Conquest, &c., to this effect. (See p. 604.) "When the placenta," says Dr. Rigby, "is fixed to the os uteri, nothing but turning the child will put a stop to the flooding."[2] "It may be laid down as a rule," observes Dr. Lee, "admitting of no exception, that where hemorrhage occurs from the placenta being situated over the os uteri, artificial delivery must be performed."[3] The same author, in a later publication, candidly and correctly states, in reference to this practice—the operation of turning in unavoidable hemorrhage: "At the best, it is a dangerous operation, and you can never tell with certainty whether the patient will recover after its performance, however easily it may have been effected."[4]

The professed object of my present inquiry is to attempt to prove that the artificial removal of the placenta is a more safe and commendable operation in different forms of placental complication, than the artificial removal of the infant, is not attended with such extreme hazard to the life of the mother, and is specially applicable in those very cases in which turning is specially difficult and dangerous. There are, however, very many cases of placental presentation in which the artificial delivery, or turning, of the fœtus, will still remain as the most proper and legitimate plan of treatment. This remark applies, I believe, particularly—1. To those instances in which the child is alive, and at or near the full term of utero-gestation, when labor supervenes; and, 2. Where fortunately the mother has borne a family previously, and the os uteri, by the time the hemorrhage proceeds to a dangerous extent, is either so dilated, or dilatable, as to allow of the introduction of the hand of the operator and the extraction of the fœtus, without any fear of injury and laceration. Under such a condition, turning affords a very fair chance of life to the infant—and I presuppose the state of the os uteri to be such, that the danger of lacerating it, and thus leading either to subsequent hemorrhage or phlebitis, is not so great as to endanger the mother's recovery.

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[1] Traité des Accouchemens, p. 369.

[2] Essay on Uterine Hemorrhage, 6th edit. p. 91.

[3] Researches on the Pathology and Treatment of Diseases of Women, p. 207.

[4] Lectures on the Theory and Practice of Midwifery, p. 373.

Further, there is another set of cases of placental presentation, in which we must continue to be driven, sooner or later, to the adoption of turning. I have already shown that in a considerable portion of these presentations, the fœtus presents preternaturally, and in a number of them transversely. When the position of the fœtus is transverse, we must of course ultimately rectify it, and deliver by turning. But here we are driven to adopt the operation of turning, not in consequence of the peculiar presentation of the placenta, but in consequence of the peculiar presentation of the child. Knowing that we can at any time safely arrest the hemorrhage by the complete detachment of the placenta, we shall be further able, in such cases of cross-birth, to delay, if in other respects it is imperatively necessary, the operation of turning, and select for it that time which may be considered most compatible with the state and structure of the uterus, and the safety and life of the mother; and here also the adoption or not of the artificial detachment of the placenta will be greatly regulated by the ascertained death or life of the child.

3. CASES OF PLACENTA PRÆVIA IN WHICH ARTIFICIAL DETACHMENT OF THE PLACENTA FORMS THE PROPER TREATMENT.

When our practical acquaintance with this method of treatment becomes more extensive, and the measures for effecting it are sim-

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[1] Cases of placental presentation, thus favorable for the safe operation of turning, are much more likely to be met with in hospital than in private practice, because hospital patients are generally near the full term of delivery before they are admitted, but of 33 cases mentioned by Dr. Lee, only 2 seem to have reached the full time. On the other hand, out of 11 cases which occurred in the Dublin Hospital during Dr. Collins' mastership, all had reached the full time except 2. If turning were applied in every case, such evident differences in the conditions of the patients, must of necessity greatly modify the resulting maternal dangers and mortality.

plified and better understood, it is possible that the practice itself may come to be applied in almost all the instances of placental presentation that I have alluded to under the two preceding heads. This, however, is by no means the case during our present knowledge of the subject, and in the meantime I would wish to point out here in detail the special cases of placenta prævia in which the practice I propose seems to me to be more particularly applicable. These cases include, 1. Some complications on the part of the *mother*, especially such a degree of rigidity of the os uteri or vagina, or such obstructions of any form in the maternal passages or uterus itself, or such states of general constitutional exhaustion, as contraindicate and prevent the safe exercise of the operation of turning. And, 2. Some complications on the part of the child, particularly its death or prematurity, rendering the operation of version unnecessary, so far as any view to its safety may bear upon the question, provided the mother can be delivered by means that afford greater safety to her. I shall now proceed to consider somewhat in detail each special set or division of cases to which the present mode of practice appears to be legitimately applicable.

FIRST SERIES.

When the os and cervix uteri are too rigid or undilatable to allow of the safe exercise of the operation of turning.

Obstetric authors appear, as we have already seen, to be generally and perfectly agreed as to the propriety of turning in all cases of complete presentation of the placenta; they differ, however, from one another in regard to the time and circumstances which ought to be selected as most fit and safe for the performance of this operation in placenta prævia. Some strongly advise us to operate rather too soon than too late. Others as strenuously counsel us to beware of operating till the time that the os uteri is so dilated or dilatable as to permit of our interference, without any chance of injuring or lacerating the cervix in the passage of our hand, or the extraction of the infant. "In recommending early delivery, I think it right," says Dr. Rigby, the highest authority I can quote on such a subject, "to express a caution against the premature introduction of the hand, and the too forcible dilatation of the os uteri, before it is sufficiently relaxed by pain or discharge; for it is undoubtedly very certain that the turning may be performed too soon, as well as too late, and that the consequences of the one may be as destructive to the patient as the other. I am particularly led to observe this, as I have lately been informed, from very good authority, namely, a gentleman to whom one of the cases occurred, of three unhappy instances of an error of this sort, which happened some years ago to three surgeons of established reputation, who, from the success they had met with in delivering several who were reduced to the last extremity, were encouraged to attempt it where but very little blood had been lost, in hopes that their patients' constitutions would suffer less injury, and their recovery be more speedy; which, till the experiment was made, was a very reasonable supposition. The women died, and they seemed convinced that their deaths were owing to the violence of being delivered too soon, and not to the loss of blood, or any other cause."[1]

On the other hand, Dr. Ingleby, the author of one of the best works that has been written on uterine hemorrhage, tells us that "a very experienced accoucheur, in whose practice about twenty cases of this description have occurred, informs me that those women who were delivered at an early period of gestation, recovered, but when delivery was postponed to a late period, the result was fatal. Similar answers have been given to the same inquiry by other gentlemen."[2]

Two great sources of danger, in fact, require to be taken into consideration in relation to the operation of turning, in each individual case of placental presentation—namely, *first*, The danger of too long a continuance of the hemorrhage, and consequently the exhaustion, and even the death of the patient, if the operation be not performed sufficiently early; and, *secondly*, The danger of contusion and laceration of the cervix uteri and its included vessels, if the operator, afraid of delay, and of the effects of the hemorrhage, proceeds to deliver too soon. And it is to be especially recollected in relation to this last point, that any degree of laceration in the tissues of the cervix uteri in a case of placenta prævia is fraught with unusual and imminent danger. The part of the uterine parietes to which the placenta is affixed becomes, as is well known, most freely supplied with bloodvessels; and there, in particular, the venous sinuses of the uterus are especially large and abundant. In placental presentations, it is the cervix uteri itself that assumes this highly vascular condition; and hence any laceration or injury of it is much more liable to be followed, than under other positions of the placenta, with hemorrhage immediately after delivery, or with subsequent inflammation of its included and lacerated veins, giving rise to uterine phlebitis, a disease which, under one or other of its many forms, is found to be one of the most common causes of death after the occurrence of placental presentations.

The two preceding and opposite causes of danger and death to which the mother is submitted under the operation of version in placental presentations, appear to be very equally balanced in their consequences; that is to say, amongst the number of fatal cases which occur in practice, or are to be found upon record, nearly the same proportion appear to perish from the extent of the hemorrhage before delivery, or its subsequent effects (the results of protracting the delivery, and from post-partum flooding from the lacerated vessels of the cervix uteri, with uterine phlebitis, and all the other fatal consequences arising from injury and laceration of the vascular structures of the cervix, when the os is injured by being forced open too early).

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[1] Essay on Uterine Hemorrhage, p. 37.

[2] Practical Treatise on Uterine Hemorrhage, p. 147.

As this appears a most important practical point to establish on sufficient statistical data, I have collected into the following table the dates of the death of the mother in 78 fatal cases of placenta prævia, reported by the different authors whose works I have quoted in a former page (p. 600). The table is constructed to show, *first*, The number of mothers who died undelivered; *secondly*, The number of those that sunk within three hours after delivery, and probably from exhaustion, under the hemorrhage preceding flooding, or from its continuance from the lacerated vessels of the cervix, after the emptying of the uterus; *thirdly*, The number that died within forty-eight hours after delivery, and hence, I suppose, from exhaustion under hemorrhage and the operation of delivery; and *lastly*, The number of those that died at a later period, after the immediate danger from the hemorrhage and operation had subsided, and when the fatal result was apparently always, or almost always, the consequence of uterine phlebitis, in some of its manifold forms.

Period of Death of the Mother in Fatal Cases of Placenta Prævia.

Reporters.	No. of Fatal Cases.	Died Undelivered.	Died within 3 hours after Delivery.	Within 48 hours.	At a later period.
Mauriceau,	3	1	1	1	—
Giffard,	7	—	6	1	—
Smellie,	6	3	2	—	1
Rigby,	11	2	3	3	3
Collins,	2	—	2	—	—
Lachapelle,	9	—	3	2	4
J. Ramsbotham,	9	1	7	1	—
F. Ramsbotham,	16	—	7	1	8
Lee,	15	1	5	2	7
Total,	78	8	36	11	23

Some authors and practitioners seem to have fallen into the mistake of supposing, that in all cases of placenta prævia, the os uteri will be found dilated or dilatable by the time that the hemorrhage has taken place to such an extent as to endanger the life of the mother. It may be sufficient to quote, on such a point, the opinions of an author, who has written one of the soundest and most classical professional works that we have in the English language: "In some cases," says Dr. Denman, "in which it has been presumed to be necessary to deliver the patient on account of the hemorrhage, in placenta prævia, the parts have been in such a state, that the operation could not, it was thought, be performed with safety. Whenever the case demands the operation, on account of the danger of the hemorrhage, the state of the parts will on this account *always* allow it to be performed with safety, though not with equal facility."[1] It must, however, be confessed with regret, that the opinion here expressed by Dr. Denman rather indicates the condition which we anxiously desire than that which we always meet with in practice. And our best practical authors seem to be becoming more and more agreed, that cases of placenta prævia are constantly occurring, in which the hemorrhage may proceed to such a degree, as to urgently demand artificial interference upon our part, in order to arrest its violence or complete the delivery, without the os uteri and passages being at the same time in such a condition as to permit of the safe passage of the hand into the uterus, or of the safe extraction of the infant from it.

"If," says Peu, "a pregnant woman is seized with considerable flooding, which does not cease, the secret is to deliver her as soon as possible. * * * For this, I always suppose that there is a sufficient opening. For to force and dilate the internal orifices of the womb is just so many deaths, or rather lives thrown away and sacrificed (*c'est autant de morts, ou de vies plûtôt qu'on precipite et qu'on prodigue*). If then, the smallness of the opening, or the extreme debility of the patient, render the thing visibly impossible, it is better to leave the case to nature, than to irritate the blood so as to augment the flooding without hope of alleviating it."[2]

When treating of the subject of unavoidable hemorrhage, Professor Davis states, that he "had met with many examples even of fatal results of profuse uterine hemorrhage unaccompanied by any amount of dilatation of the orifice of the womb."[3] Dr. Hamilton, when speaking of placental presentations, tells us that in the month of September, 1816, he was called to two cases where the patient seemed to be *in articulo* from the deluge of the discharge, and, nevertheless, where the os uteri was in the state of obstinate rigidity which Dr. Davis has described. "It has been advised," Dr. Rigby states, "never to introduce the hand till nature has shown some disposition to relieve herself, by the dilatation of the os uteri to the size of a shilling, or a half-crown; and this rule is certainly founded on a rational principle; for when it is so much dilated, there is no doubt but the turning may be easily and safely effected; but from some of the annexed cases it appears, that a dilatation to this degree sometimes does *not take place at all*, and that even when the woman is dying from the great loss of blood, the uterus is very little open."[1] "There is not unfrequently," remarks Dr. Lee, "most profuse and alarming flooding from complete placental presentation, where the os uteri is so thick, rigid, and undilatable, that it is impossible to introduce the hand into the uterus without producing certain mischief."[2]

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[1] Introduction to the Practice of Midwifery, p. 530.

[2] La Pratique des Accouchemens, p. 516.

[3] Principles and Practice of Obstetric Medicine, p. 1040.

"Again, it is by no means impossible," Dr. Ramsbotham observes, "that such alarming symptoms (the patient faint, and gasping, and cold, &c.) may show themselves before the os uteri has acquired the diameter of half a crown, as to render it extremely hazardous for us to delay our means until that degree of dilatation is arrived at. The blood may be gushing forth in a copious and continued stream, or may be oozing away in a less violent though steady draining, or coagula of considerable size may be passing from the vagina every few minutes; and it must be evident to the least attentive observer, that such a state of things cannot be allowed to proceed unchecked. Two modes offer themselves for our choice: either immediate delivery, or endeavoring to restrain the flow, and delaying until the due degree of dilatation is effected. Our practice will mainly be guided by the state of the os uteri itself; if it appear soft, lax, and distensible, offering but little resistance to our fingers in the attempt at dilatation, we shall mostly be able, under the use of sufficient caution, to pass the hand entirely through it without injury, even although its disk be not exceeding the diameter of a shilling; and, indeed, I have accomplished the operation of turning on some few occasions, under these unpromising circumstances, by slowly insinuating the fingers *seriatim*. Although, then, such a proceeding be not desirable, if it can be avoided, inasmuch as every minute's delay brings with it an augmentation of danger—we are fully justified in effecting the dilatation of the os uteri thus artificially, even when, at the commencement of our efforts, it will scarcely admit the introduction of the tips of two fingers."[3]

"I know," observes Dr. Collins, "of no circumstance *so much to be dreaded*, as the forcible introduction of the hand where the parts are in a rigid or unyielding state; for although turning the child is the established and most desirable practice, yet the success of this operation will mainly depend on the judgment of the practitioner in selecting the most proper and favorable time. Cases will happen where he is obliged either to suffer his patient to sink from loss of blood, or proceed to deliver when the parts are in an undilated and rigid state, in order to afford her the only chance of life; but dire necessity should alone compel him to hazard the consequences of such violence. We are well aware the os uteri will yield at a much earlier period after a severe loss of blood than under other circumstances; we are equally well aware of the great injury the patient sustains by delaying delivery beyond the earliest moment that the mouth of the womb will by gentle efforts permit the introduction of the hand; it is against *premature measures* we wish to guard the young practitioner; as every individual of experience will acknowledge the great embarrassment he not unfrequently has labored under, in deciding on the time, beyond which, to defer affording assistance were timorously to risk his patient's safety, and previous to which, delivery would be either impracticable, or, if effected by violent means, truly dangerous; for even a slight injury to the mouth of the womb will prove more fatal than an increased loss of blood, so long as the strength can possibly bear it."[1]

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[1] Essay on Uterine Hemorrhage, p. 38. [2] Principles and Practice of Midwifery, p. 373.

[3] Obstetric Medicine and Surgery, 2d edit. p. 387.

That the flooding in placental presentations sometimes proceeds to such an excess as to demand measures either for its suppression, or for the forcible delivery of the patient, *before* the os uteri is in such a dilated or dilatable state as to permit with safety to its structures of the operation of turning, admits of being still more forcibly illustrated by an appeal to the history of individual cases of unavoidable hemorrhage. As the matter is one of most important bearing upon the argument before us, I shall offer no apology for enforcing and extending the evidence in favor of it, by adducing some individual facts and instances in illustration of my present purpose.

"In no case," observes Dr. Edward Rigby, "is it proper or safe to force delivery by artificially dilating the os uteri, when it is contracted and unyielding; but where the placenta is presenting, it is peculiarly dangerous, for even slight laceration of the os uteri will be followed by serious consequences. Where the placenta is situated in the upper part of the uterus, it is of very little consequence if the edge of the os uteri has been torn somewhat during labor; but in the present case it is very different—the os uteri now plays the part of the fundus, its vessels are immensely dilated, and larger ones are ruptured, which cannot be closed by the firmest contraction of the uterus. * * * Cases have occurred where the os uteri has been artificially dilated, where the child was turned and delivered with perfect safety, and the uterus contracted into a hard ball; in fact, everything seemed to have passed over favorably; a continued dribbling of blood has remained after labor, which resisted every attempt to check it; friction upon the abdomen, and other means for stopping hemorrhage by inducing firm contraction of the uterus, were of no use, for the uterus was already hard and well contracted; the patient has gradually become exhausted, and at last died: on examination after death, Professor Naegele has *invariably* found the os uteri more or less torn."[1]

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[1] Practical Treatise on Midwifery, p. 93.

We have sometimes proof afforded us in *post-mortem* examinations, of the fact, that unavoidable hemorrhage may proceed to a fatal extent during the first stage of labor, without the tissues of the os and cervix uteri ever dilating to such a degree as to allow of the introduction of the hand without laceration.

In his magnificent plates of the anatomy of the gravid uterus, Dr. William Hunter has given two representations of the uterus of a woman who died of unavoidable hemorrhage during the first stage of labor. She had reached the ninth month of pregnancy. The first plate (Tab. xi.) represents the external surface of the uterus, with the enormous uterine veins about the cervix much enlarged and injected. The second (Tab. xii.) shows the uterus opened, with the placenta fixed over the os uteri, and the infant presenting in the second position of the head, or with the face looking to the left sacro-iliac synchondrosis.[2] The os uteri appears very small and unopened. The preparations of the parts in this case are still contained in the Hunterian Museum, belonging to the University of Glasgow. On lately examining them, I found the os uteri apparently not opened to the size of a shilling, and the lips of the cervix, particularly on one side, so thick and turgid (they are correctly represented so in the plate), as to leave no doubt that it would have been physically impossible to distend the uterine orifice, so as to admit the hand without certain laceration. The placenta is partially detached and slightly lacerated upon its external surface opposite the os. In reference to this preparation, I may add one fact bearing upon the matter discussed in a former page, relative to the mode in which retrograde hemorrhage is prevented, from the uterine veins after the placenta is detached. The veins, as I have said, are very large, as seen in the published plates, and are injected with yellow wax. There is, however, no appearance whatever of any of this wax having reached so far back as the utero-placental orifices left by the separation of the placenta.

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[1] Rigby's System of Midwifery, p. 259.

[2] It has always appeared to me a remarkable fact, that both in this plate and in the beautiful engraving, Tab. iv., of the natural position of the fœtus in utero, Dr. Hunter, who is justly and deservedly celebrated for his accuracy and fidelity of observation, has represented the fœtus as lying in utero in a position which it very rarely occupies—so rarely indeed, that Naegele avers, he only met with it once as an original position in twelve hundred cases. Plates xv. and xxiii. represent the child in the third position; none in the work shows it in the first or most frequent position. I may add, that the "reversing" the infant is not a mistake of the engraver's, as I find it exists in the original drawings by Rymsdyk, which are still preserved in the museum. Some casts also, taken probably from the same preparations, show the fœtus placed in this unusual position.

Long ago Dr. Smellie detailed three cases of Cæsarean operation performed immediately after the mothers had died from unavoidable hemorrhage. These cases are interesting in an historical point of view, as affording some of the earliest evidence which we have on record of the occasional implantation and organic adhesion of the placenta to the interior of the cervix uteri being ascertained by actual dissection. They all occurred during the two years of 1747 and 1748.[1] One of the cases is further valuable in reference to our present inquiry, as showing a perfectly undilatable state of the os uteri, under fatal flooding from placental presentation. I shall append some details of it.

CASE XLII.—*Cæsarean operation, after death from unavoidable hemorrhage; rigid state of os uteri as found on dissection; its laceration under the attempt to open it.*—"The woman was turned of forty, of a gross habit, and had never borne a child." After a fall in the seventh month, she had repeated discharges of blood. Two or three weeks before her full time, she was taken with slight pains, upon which Dr. Smellie was called, and found the os uteri opened to about the size of a sixpence, and within it a soft substance that felt like the placenta. As she had rested but indifferently the preceding night, was faint and weak, and had some small returns of the discharge, Dr. Sands was consulted, and gave it as his opinion that it was still proper to support the patient's strength with broths and nourishing food, and more safe to wait till the slight pains should bring on the right labor, than to use any violence to deliver her immediately. "I was again," says Dr. Smellie, "called about nine o'clock the same night, when she was taken all of a sudden with frequent faintings, in one of which she expired as I entered the room. This sudden alteration prevented me from making any attempt at delivery; and, indeed, had not this event happened, I should have been afraid of her dying in the operation, because of her gross and weak habit of body. As soon as all present were satisfied that the person was dead, I opened her abdomen, and having taken out the child, examined the uterus. I found the placenta firmly adhering to its interior and posterior parts; about two fingers' breadth of its lower edge was separated from the os internum, which it covered; and this was what Dr. Sands and I had felt in the morning. Having extracted the secundines, *I tried with my hand to open the os internum from the inside of the uterus, which with great force I performed, not without tearing it about two inches on one side.*" "By this," he adds, "it appears how difficult it is to dilate this part in women going with a first child, especially when they are pretty old. Indeed, it is sometimes impossible to be done before they come to their full time, and even then not until the parts are thin, soft, and largely opened by previous labors."[1]

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[1] The first case in which there was post-mortem evidence of the organic implantation of the placenta over the os and cervix uteri, seems to have been that published by Petit, in the Memoires de l'Academie for 1723. It is well known that the general belief of accoucheurs was that the placenta was always originally affixed to the interior of the fundus or body of the uterus; and when found at the os, it was believed to have separated and fallen down into that position. So late as 1717, Dr. Simpson, Professor of Anatomy at St. Andrews, published in the Edinburgh Medical Essays, vol. iv., a paper in which he attempted to prove "that the placenta *inviolably* adheres to the cavity of the fundus (uteri); with which it is ingraft, and can never again shift its place."

If, in such cases as the preceding, it were necessary, in consequence of the extent of the hemorrhage, to proceed to delivery, and yet the os uteri were not more dilated or dilatable than it was found in Dr. Smellie's patient after death, it is evident that the forcible introduction of the hand into the os uteri, and the artificial extraction of the child through it, would necessarily give rise to lacerations of a greater or less extent in the tissues of the cervix, and such we find to be the fact in actual practice. The simple details of some cases may enforce this important point, by showing the difficulties of the practitioner, and the dangers of the patient under such a complication. For this purpose I shall quote *verbatim* the reports of two or three instances from the valuable and practical works of Dr. Collins and Dr. Lee.

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[1] Cases in Midwifery, vol. iii. Collection xxix. No. 1, Case ii. p. 412.—Dr. Rigby, in commenting upon this case of Dr. Smellie's, argues that the trial being made to open the mouth of the uterus after death, "when every strong membranous part is incapable of contraction and extension, is no proof that if the most favorable opportunity had been watched for, and a gradual and repeated endeavor to open it had been before made, it would not have succeeded."—Essay on Uterine Hemorrhage, p. 44. Dr. Rigby appears to forget that the examination of the uterus, and the laceration of the os, upon attempting to open it, occurred *immediately* after death, and as the dilatability of the part depends upon its physical rather than its vital conditions, we cannot believe that an os uteri which was thus found incapable of dilatation without tearing after death, could have been found during life, a few minutes previously, capable in any degree of dilatation, without the same laceration. Dr. Smellie has given an additional instance of Cæsarean operation, performed immediately after the death of the patient in unavoidable hemorrhage, with the view of saving the child. The dilatability of the os uteri was not in that instance in any way affected by the death of the mother; it was, as he describes, thin, soft, and open to the breadth of half-a-crown. "*I dilated it with ease*, which showed that if I had been sent for in the evening, she might have been safely delivered." On Dr. Smellie being called to this case, before he had time to put her in a position for delivery, the patient fainted away, was thrown into convulsions, and died instantly. Two or three years ago I had an opportunity of seeing the dissection of a woman who had died of accidental hemorrhage in the first stage of labor. The os uteri was dilated to nearly the size of a half-crown, but its edges were perfectly rigid, and undilatable without tearing.

CASE XLIII.—*Profuse unavoidable hemorrhage; turning required while the passages were badly prepared; laceration of the cervix uteri; death.*—"W. S. was admitted, at her full time, November 12. She had been shedding occasionally for five days before, which reduced her to a state of great debility. There was no hemorrhage on her admission, but on examination, the placenta was found at the mouth of the womb, which was not more dilated than the size of a half-crown, with its edge thick, but not *very* rigid. As the discharge had ceased, and her strength was much exhausted, she was ordered to be kept perfectly cool and quiet, to have some cold chicken broth. About an hour and a half afterwards, suddenly, the most profuse hemorrhage set in, so much so, that in two or three minutes, the blood was running in every direction over the edge of the bed; this was consequent on some slight uterine action. There being no chance of life without speedy delivery, we determined to make the attempt, though the parts were badly prepared; accordingly the hand was very slowly and cautiously introduced, and the feet brought down with little exertion; the uterus acted strongly, and felt well contracted after delivery. The placenta came away with the child. Great debility succeeded the operation, with a slight discharge of blood at intervals; and on examining an hour after, a laceration of the neck of the uterus, anteriorly and to the right side, was discovered, commencing at its junction with the vagina, and extending upwards. She died shortly afterwards. It was her fourth child—a girl (living). Dissection verified the result of the vaginal examination."[1]

As we have seen in a previous extract, p. 696, no author seems to be more thoroughly impressed than Dr. Collins, with the necessity of not hastening the delivery in placental presentations, if it is at all possible to avoid it—and the above case affords an apt illustration of Dr. Lee's remark, that "it is sometimes absolutely necessary to deliver by turning, before the hand can possibly be introduced into the uterus, without producing fatal contusion or laceration of the part."[2] The following are analogous cases from Dr. Lee's own Clinical Reports.

CASE XLIV.—*Complete placental presentation, with exhausting*

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[1] Collins' Practical Treatise on Midwifery, p. 97.

[2] Lectures on Midwifery, p. 373.

hemorrhage; os uteri so rigid as not to allow the hand to pass; feet of the child seized through the os; forcible extraction of infant; death of the mother.—On the 12th of January, 1839, Mr. Jones, of Carlisle Street, Soho Square, called me to see a lady in the eighth and a half month of pregnancy, who had been attacked with uterine hemorrhage a month before. It first took place without any accident or pain, and the quantity lost was about half a pint, and it produced little effect upon the constitution. She remained quiet for several days, and then got up, and only felt a little weak. For ten days she went about, but the hemorrhage returned on the fifteenth day after the first attack, but not to a great extent. Seven days after this, a third and more profuse hemorrhage took place; it gradually went off, but not so quickly as the other attacks. At one o'clock, 12th January, it was renewed to an alarming extent without any pain; about a quart of blood was suddenly lost, and she became extremely faint. At four A. M. the discharge still continued. When I first saw her, at seven o'clock, she felt faint; the pulse was rapid and feeble. The upper part of the vagina was filled with a large clot of blood, which adhered to the os uteri. By displacing this at the back part, I could distinctly feel the placenta adhering all round to the neck of the uterus, which was thick and rigid, and very little dilated. *The effect produced by the hemorrhage was so great, that it was evident death would soon take place if the delivery were not speedily completed; and the state of the orifice was such, that it was certain the hand could not be passed, but with the greatest difficulty.* At eight o'clock, Dr. Merriman saw her with us, and agreed that immediate delivery was necessary. I passed the right hand into the vagina, and insinuated my fingers between the uterus and placenta at the back part, and reached the membranes. But the *rigidity of the orifice was so great, that though I employed great force for a considerable time, I could not succeed* in getting the hand into the uterus. Dr. Merriman recommended rupturing the membranes, and I was proceeding to do this with the fingers, when I felt one of the feet of the child, which I grasped and brought down into the vagina enveloped in the membranes, which then gave way. Nearly half an hour elapsed before the version could be completed, and when it was effected, the neck of the uterus grasped the neck of the child so firmly, that I experienced the greatest difficulty in extracting the head, and not till I had made pressure for some time with the finger, and dilated the orifice of the uterus. A great discharge of blood instantly followed; the placenta was removed, and every means employed to stop the hemorrhage, but the breathing became hurried, the extremities cold, and she died in less than an hour after delivery. Dr. Merriman informed me, that a patient of his had actually died under similar circumstances before the head could be extracted. He considers the tampon as of little or no use in such cases."[1]

CASE XLV.—*Exhaustion; delivery of child postponed, from rigid state of os uteri; craniotomy; death of mother immediately afterwards.*—"Mrs. H. was attacked with uterine hemorrhage at the beginning of February, 1839, when seven and a half months pregnant. About twelve days after, it returned a second time, and yesterday morning a third time. About half-past twelve on the 5th of March, Dr. Davies requested me to see her with him, as the hemorrhage had returned in a dangerous form, and the orifice of the uterus was not in a condition to admit of delivery. We found the placenta adhering all round to the neck of the uterus, the orifice rigid and undilatable, and open to the extent of a crown, the head of the child presenting. By cold applied externally and internally, the hemorrhage was restrained till six o'clock in the morning, when it was renewed with violence. Dr. Davies then pressed his fingers through the placenta, tore it in two parts, and perforated the membranes. Half-past eight A. M. no hemorrhage; slight pains. Eleven A. M. no flooding; head pressing into the orifice of the uterus. *We were prevented at the time from perforating and extracting the head, by the rigid state of the os uteri.* She seemed to regain strength during the day; but at ten in the evening, without any further loss of blood, she began to breathe with great difficulty, the lips were livid, the hands and feet cold, and it was evident she would soon die undelivered, if we did not interfere. I opened the head, and extracted it with the greatest difficulty, in consequence of the firm and rigid state of the os uteri. The operation was scarcely completed before she was dead."[2]

CASE XLVI.—*Turning attempted while the os uteri was too rigid to allow the hand to pass; two days subsequently, syncope from sudden flooding; turning; death of the mother.*—"On the 26th of April, 1835, I was called to a patient of the St. Marylebone Infirmary, who was more than seven months pregnant, and had been attacked fourteen days before with alarming uterine hemorrhage. The first discharge of blood took place during the night, when she was at rest; it was not preceded by a sense of uneasiness about the uterus, and could be referred to no accident or injury of any kind.

"A considerable oozing of blood still continued, when I first saw her. The placenta presented; the orifice of the uterus was opened to the size of a crown piece, but its margin was so hard and undilatable, that I found it impossible, without employing too great force, to pass the hand into the uterus. After a cautious trial for about half an hour to get the hand insinuated through the orifice,

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[1] Clinical Midwifery, p. 155, Case 282.

[2] Ibid., p. 157, Case 283.

I was compelled to withdraw it altogether, as there was no hope of overcoming the resistance. On the 27th, the flow of blood continued, the strength remaining unimpaired, and the os uteri being not less unyielding. I resolved to wait till relaxation should take place, and moderate the discharge by the recumbent position, and the application of cold externally and internally. 28th. A large quantity of blood suddenly escaped, which produced complete syncope. The countenance was afterwards pale, the extremities cold, and the pulse rapid and feeble. The os uteri being soft and dilatable, I immediately passed up the hand, and delivered by turning. The child was born alive. The placenta was removed soon after; but though no further loss of blood was experienced, she continued gradually to sink, and died in a few days."[1]

CASE XLVII.—*Severe and exhausting unavoidable hemorrhage with the os uteri perfectly undilatable; a foot caught through the undilated os, and the child extracted; mother in great danger, but recovered.*—"On the 7th of October, 1835, I was requested by Mr. Gairdner, of Foley Place, to see a patient, residing in Frith Street, who had completed the seventh month of pregnancy, and had been attacked with uterine hemorrhage three weeks before. A slight discharge of blood had continued during the whole of this period, but it had produced little effect upon the system until a few hours before I saw her, when several pints of blood were suddenly discharged, and her whole strength seemed at once extinguished. The pulse was not perceptible; the extremities were cold, and the respiration feeble. The blood still continued to flow in great quantities, and it was evident death would soon take place if the uterus was not speedily emptied of its contents. The os uteri was not dilated to the size of a crown, and it was so rigid that I found it *absolutely impossible*, though I employed a degree of force scarcely justifiable, to pass more than three fingers within it. *The whole hand could not be made to pass, though it appeared certain that death would soon take place if delivery was not immediately accomplished.* On the fingers being withdrawn for a short time, the flooding continued. I made another effort to turn the child, but the resistance could not be overcome. I then pressed forward the fore and middle fingers of the right hand between the placenta and uterus, so as to reach the membranes, which I succeeded in tearing open. Pressing the fingers still forward, they came in contact with one of the feet, which they grasped and brought down into the vagina. This was pulled lower and lower till the whole extremities and nates were drawn into the os uteri; but so rigid did it continue to be, that although I exerted all the force I dared employ in dragging it down, half an hour elapsed before the pelvis of the child could be made to clear the orifice of the uterus. At last it was extracted with the placenta, and the hemorrhage ceased. A violent rigor followed, which threatened for a time to destroy the patient. Bottles of hot water were applied to the feet and pit of the stomach, the whole body was covered with hot blankets, and brandy was liberally administered. She slowly recovered from the immense loss of blood."[1]

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[1] Clinical Midwifery, p. 145.

But if we pass from individual to general facts, and attempt with this view, to ascertain statistically the actual results of the practice of turning, under the complication of dangerous unavoidable hemorrhage in connection with a rigid or insufficiently dilated os uteri, the consequences are probably more disastrous than the remarks which I have made, or the cases I have quoted, under the present head, would lead us to suppose. Dr. Collins refers to cases of Dr. Ramsbotham's in which, from the extent of the hemorrhage, and other symptoms, it was considered requisite to have recourse to turning with the os uteri still inadequately dilated. I find in Dr. Ramsbotham's work three additional instances of the same kind. In his Clinical Midwifery, Dr. Robert Lee states, that out of the thirty-five cases of placental presentation which he has recorded, "in eleven there had been more or less rigidity of the os uteri, with dangerous hemorrhage, and turning was performed in several of them where the whole hand could not be introduced into the uterus." Mauriceau, Lachapelle, and Collins, give each one or two similar instances. The following table shows an analytical view of the results of all these cases in reference to their influence upon the life of the mother.

Maternal Mortality in cases of profuse unavoidable Hemorrhage while the Os Uteri was still Imperfectly Dilated.

Reporters.	No. of Cases.	Mothers Lost.	Mothers Saved.
Mauriceau,[3]	2	2	0
Lachapelle,[4]	1	1	0
J. Ramsbotham,[5]	8	7	1
R. Lee,[6]	11	8	3
Collins,[7]	2	2	0
F. Ramsbotham,[8]	1	1	0
Total,	25	21	4

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[1] Clinical Midwifery, p. 146, Case 267.

[2] Ibid. p. 164. These eleven cases seem to be those marked as Nos. 266, 271, 272, 274, 277, 282, 283, 284, 285, 287, 289.

[3] Observations sur la Grossesse et l'Accouchment des Femmes, pp. 134, 363.

[4] Pratique des Accouchemens, tom. ii. case 9.

[5] Observations in Midwifery, part ii. Nos. 138, 139, 140, 141, 142, 144, 145, 149.

[6] Clinical Midwifery, p. 149, *et seq.* [7] Practical Treatise on Midwifery, p. 97.

[8] London Medical Gazette for 1844, p. 279.

This table sufficiently demonstrates the extreme danger which the mother incurs when forcible delivery is required, and attempted to be accomplished by the operation of version, with the os uteri still imperfectly dilated or dilatable. The result is, that calculating from the data which the table affords, 17 per cent. only of the mothers were saved and 83 per cent. of them died under the complication—a result far greater than we find in any legitimatized surgical operation, and far more fatal than even the Cæsarean section itself.[1] But under this complication, we believe, that the complete artificial detachment of the placenta would be perfectly practicable in most, if not in all cases, and would consequently lead to the saving of many maternal lives. It would at once, as we have seen, arrest the hemorrhage which is the more immediate source of danger to the patient, and allow time for the os uteri to become relaxed and the labor to be completed, either by the natural pains or otherwise, as might be afterwards found proper or necessary.

If our statistics are sufficiently extensive to be true, we might thus save 80 or 90 maternal lives out of every 100, by having recourse to artificial and complete detachment of the placenta, instead of losing 80 or 90 mothers out of every 100, by having recourse to the operation of turning. I shall afterwards take occasion to show, that in this complication the life of the child does not interfere in any material degree with these results, because the infant itself is almost invariably lost when turning is attempted under the particular complication in question.

SECOND SERIES.

In Cases of First Labor.

In another place, I have endeavored to demonstrate that almost all obstetric complications are more frequent and formidable in first than in subsequent labors—rupture of the uterus being the principal exception to this general rule. Placental presentations would seem to form another exceptional instance. I am not aware that any author alludes to this point, but it has struck me repeatedly in pursuing the statistical inquiries upon which the present memoir is founded. In the Dublin Lying-in Hospital, during the period of Dr. Collins' mastership, the proportion of first labors amounted to 30 per cent. of all the deliveries. During the same period, eleven cases of placental presentation were observed in the Hospital. In not one of the eleven did this complication occur in first labor.[1] In the various reports of cases of placenta prævia, by Giffard, Smellie, Rigby, Clarke, Collins, Lee, and Ramsbotham, I find in all 55 instances in which the number of the pregnancy is mentioned.[2] In 3 only out of these 55 cases, was the accident observed in a first pregnancy. Out of 81 cases, collected from our own General Table of Cases in Section III., with the number of the labor noted, the patient was pregnant for the first time in 8 instances. (See p. 619.)

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[1] Dr. Churchill states the following conclusions as the results of his extensive inquiries into the mortality attendant upon the Cæsarean operation. "Among British practitioners, in 40 cases, 11 mothers recovered and 29 died, or nearly three-fourths. Among Continental practitioners, out of 369 cases, 217 mothers recovered, and 152 died, or about 1 in 2¼. Taking the entire number, which amounts to 409, we find that 228 mothers were saved, and 181 lost, or about 1 in 2¼."—Researches in Operative Midwifery, p. 221.

Dr. Churchill has further calculated, that in the Cæsarean operation, 160 children were saved, and 64 lost out of 224. We shall see afterwards, that on the other hand far more children are lost than saved in turning in placental presentation, with an undilated os uteri.

Thus adding these two collections of data together, we have 136 cases of placental presentation, and 11 only of these were observed in first labors.

If the rarity of placental presentations came to be fully established in first pregnancies by more extensive data,[3] may it not afford us some clue to the explanation of the cause or causes leading to the origin or production of that deviation in the site of the development of the placenta which constitutes placenta prævia?

But though placental presentations seem to occur rarely in first pregnancies, yet when they are met with, they are liable to exhibit unusual difficulties in consequence of the maternal passages being less dilated and dilatable than in women who have previously borne a family. Hence the labor is slower, and the hemorrhage consequently longer and more exhausting, before the parts are sufficiently relaxed, to admit with safety, of either the artificial or natural delivery of the infant. In speaking upon this point, in connection with the treatment of unavoidable hemorrhage, Dr. Smellie justly observes, "The younger the woman is with child, the greater is the difficulty in opening the *os internum*, and more so in the *first* child, especially if she is past the age of thirty-five."[1] In a previous page (p. 700) I have already quoted from Dr. Smellie an account of the post-mortem appearances and state of the os uteri, which he observed in a patient that had died in her first pregnancy, of unavoidable hemorrhage. Adverting to the force required to open the os uteri in that instance after death, Dr. Smellie observes, "By this it appears how difficult it is to dilate this part in women going of a *first* child, especially when they are pretty old. Indeed it is sometimes impossible to be done before they come to their full time, and even then, not until the parts are thin, soft, and largely opened by previous labors."[2]

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[1] Practical Treatise, Table, pp. 173, 175, Cases Nos. 4, 17, 33, 84, 50, 72, 77, 83, 89, 92, 119.

[2] Mauriceau does not mention the number of the pregnancy in any of his cases of placenta prævia. Giffard mentions it in 2—see his Cases in Midwifery, pp. 208, and 492; neither of them first labors. Smellie notices it in 9 cases—Midwifery, vol. ii. pp. 308, 310, 311; and vol. iii. pp. 141, 162, 178, 409, 412, 415; one of them a first pregnancy. Rigby states it in 9 instances—Essay on Uterine Hemorrhage, 6th edit., pp. 203, 209, 218, 224, 232, 238, 240, 246, 253; none of them primiparæ. Clarke, in 4 cases—Trans. of King and Queen's Coll. of Phys., vol. i. p. 380; one of which was a first labor. Lee, in 4 cases, reports the number of the pregnancy—Clinical Midwifery, cases 268, 272, 285, 288; all the patients had borne children previously. Ramsbotham records the number of the pregnancy in 6 of his cases—Observations in Midwifery, 1st edition, pp. 195, 200, 202, 206, 216, 233; one of them only was confined for the first time.

[3] The only evidence to the contrary, that I have been able to find, is afforded by Madame Lachapelle. She states that in her 16 placental presentations, 6 were labors with first children. Is this in any degree explicable by the kind of patients resorting to the Maternité Hospital of Paris, where Madame Lachapelle's observations were made? Are the patients in a great proportion about to be confined of illegitimate children, and pregnant for the first time?

In consequence of the preceding circumstances rendering a first labor with unavoidable hemorrhage both more tedious in its course, before artificial delivery can be adopted, and the artificial delivery itself more difficult of execution, after it is had recourse to, placental presentations, with this complication, would appear to be very dangerous and fatal to the mother. Out of 10 cases of unavoidable hemorrhage in connection with first labor, which I find in the reports of Smellie, Clarke, Lachapelle, and Drs. J. and F. Ramsbotham, 7 of the mothers died. One or two cases may serve to illustrate the difficulties and dangers to which I have adverted. Dr. Francis Ramsbotham has given the following instance among his elaborate and valuable tables and notes of the obstetric practice of the London Maternity Charity.

CASE XLVIII.—*Unavoidable hemorrhage with a first child; excessive rigidity; turning and perforation of the infant's head; death of the mother.*—The patient, the subject of this case, "was more than 40 years old, and it was her first child; there was excessive rigidity of the os uteri and other structures." When first examined, the os uteri was not dilated to more than the size of a shilling. A catheter was passed into the uterus "by the side of the placenta," and the membranes were thus ruptured. This proceeding did not arrest the hemorrhage, and two hours after, delivery, by turning, was proceeded with. "Great difficulty" was experienced in introducing the hand and extracting the body of the child. The head was perforated to enable it to pass. "The placenta was immediately expelled, and although she had spoken cheerfully the minute before, she expired suddenly directly it was born."[1]

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[1] Treatise on Midwifery, vol. i. p. 332. Dr. Rigby, Essay on Hemorrhage, p. 36, and Dr. Dewees, System of Midwifery, p. 385, both advert to the difficulty which is apt to be met with in making a proper examination even of the presenting part in unavoidable hemorrhage, occurring with a first child. "It must be acknowledged," to quote Dr. Rigby, "indeed, that it may sometimes happen, that at the very first coming on of the complaint, if the discharge be small, and more especially if it be the patient's first child, and the parts be close and unyielding, the admission of the hand into the vagina as I have directed, will be attended with the utmost difficulty, and perhaps be almost impracticable."

[2] Midwifery, vol. iii. p. 414.

CASE XLIX.—*Placental presentation in a first labor; turning with os uteri, &c., offering resistance; death of the mother in three days afterwards.*—The case is detailed by Dr. Ramsbotham, in his Practical Observations. The patient was in the eighth month of pregnancy with her first child. During the preceding month she had had repeated attacks of hemorrhage, which subsided spontaneously. Upon making an examination, "although the os uteri was rigid and but little opened," the placenta could be detected. She continued free from any discharge the whole of the two following days, when a more violent return of hemorrhage occurred. The operation of turning was proceeded to. "The external parts, the vagina and the os uteri, had shown little disposition to give way." The os uteri "offered considerable resistance" to the entrance of the hand, "binding it tightly around like a cord." This was overcome, a foot was seized, and the breech brought down—uterine action became strong and "expelled the rest of the child alive." After delivery there was no farther loss; she was, however, much exhausted. On the second day she was attacked with vomitings of a dark greenish fluid, and complained of pain in the belly, which felt tender and swelled; the pulse was small and quick. In the course of the night she expired. "A post-mortem examination was not allowed; yet I could not," observes Dr. Ramsbotham, "divest myself of the suspicion, that some injury was inflicted upon the parts in the act of delivery, although I was not aware of such a fact at the moment."[2]

After what I have stated under the preceding division, upon rigidity of the os uteri in placental presentations, the practical inference which I would wish to draw from the above remarks and cases, concerning unavoidable hemorrhage in first labors, is obvious. In the present, as in the former instance, and for the same reasons, I am inclined to believe, that the complete separation and extraction of the placenta will be found more safe and far more practicable in the generality of first pregnancies with unavoidable hemorrhage, than the forced delivery of the infant by turning. The os uteri, however, in some such cases may be found, at the time required, sufficiently dilated or dilatable to allow of the artificial extraction of the infant, provided that plan be deemed, in other respects, proper and preferable. I here speak of the propriety of the separation of the placenta in first pregnancies, because I believe it will be more frequently found the proper line of proceeding in first than in subsequent deliveries, in which the passages yield earlier and more speedily and readily under the relaxing effects of the labor pains and the hemorrhage, and permit earlier and more safely of artificial delivery.

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[1] London Medical Gazette for 1844, p. 279.

[2] Practical Observations, part ii. p. 206.

We believe it to have been the intention of Dr. Simpson to render the preceding Memoir complete, by considering *at equal length* the seven remaining series of cases in which his proposed practice is indicated—viz. In premature labors; in labors supervening earlier than the seventh month; when the uterus is too contracted to allow of turning; when the pelvis or passages are organically contracted; in cases of exhaustion; when the child is dead; and when it is premature and not viable. These important additions, we trust, will yet appear.

To render the papers, however, as complete as is at this time possible, we subjoin a few farther facts and observations put forth by Dr. Simpson at various times, in explanation of his proposal and early assertions.—(Ed.)

SUMMARY OF PRINCIPLES OF TREATMENT IN PLACENTAL PRESENTATIONS, ETC.

(From Lancet, May, 1847, p. 479.)

In a late very interesting and very able paper on unavoidable hemorrhage, published in the Lancet for March 27th, I observe that the author, Mr. Barnes, argues on the idea that I recommend the complete separation and detachment of the placenta before the child, as a general rule of practice in all cases of placental presentation. Many other members of the profession appear to have had the same impression. I have always, however, maintained a very different doctrine. From the first observations which I published on the subject, up to the present time, I have upheld that the practice of detaching the placenta before the child, in unavoidable hemorrhage, was a method to be had recourse to in cases where the other recognized modes of management were insufficient or unsafe, or altogether impossible of application; and I have always looked upon this new method as possessing especial value, from its thus presenting to us a rational means of treatment in precisely those more formidable varieties of this obstetric complication, in which all former plans of practice were attended with extreme hazard or extreme difficulty.

As I am anxious to avoid future error and misconception on this head, I would beg leave here to take the liberty of enumerating briefly, and without entering into special details, the different general principles of treatment which, in my humble opinion, ought to guide our practice in this important and anxious class of cases.

Setting aside, then, those minor and palliative measures for moderating the attendant flooding that are generally adopted by practitioners in all cases of uterine hemorrhage, where time permits of their employment (such as quietude, the supine position, cold, &c.), I hold that our management of placental presentations, when either labor or such severe flooding as demands interference does at last supervene, should be regulated on the following principles:

I. *In some cases no active interference is required.*—In placental presentations, we deem ourselves called upon to interfere operatively, with the avowed object and purpose of saving the patient from the dangers of the attendant hemorrhage. Hence it necessarily follows that it would not be requisite to adopt any special form of artificial aid or delivery, if, in any case or cases, this complication were accompanied with little or no flooding. Now, in some instances of partial presentation of the placenta, the flooding ceases altogether, or abates to a safe degree, when, during the natural progress of labor, the membranes rupture and the head descends. And in some rare cases of complete presentation of the placenta, where the vascular bleeding structure of the placental mass has become obstructed and obliterated previously to the supervention of labor, little or no hemorrhage has accompanied the process of delivery. In other instances, before any operative aid can be applied, the hemorrhage suddenly and entirely ceases in consequence of the placenta becoming totally separated and expelled by the advancing head of the infant. Under such circumstances, and others, where the present or prospective dangers attendant upon operative interference, would be evidently greater than the present or prospective dangers attendant upon the existing degree of hemorrhage, any form of forced delivery would, I believe, be improper. But cases of placental presentation in which we can thus leave the delivery altogether to nature, are rare. Generally we require to adopt some active measures, with the special object of saving the patient from the actual or threatened dangers of the hemorrhage. These measures should, I conceive, be one or other of the plans which I have now to proceed to mention, viz., 1, the artificial evacuation of the liquor amnii; or, 2, the artificial extraction of the child; or, 3, the artificial separation of the placenta.

II. *Artificial Evacuation of the Liquor Amnii.*—In partial presentations of the placenta, rupturing the exposed portion of membranes (according to those principles that are generally followed in accidental floodings), is a measure which sometimes proves quite adequate to arrest or abate the hemorrhage to such an extent, that the delivery may be afterwards entirely committed to the efforts of nature. Various old authors, as Daventer, Deleurye, and Astruc, have described this same plan of treatment as applicable to complete, as well as to partial, presentations of the placenta, with this difference, that in the complete variety the liquor amnii is evacuated, not by puncturing the membranes only, but by perforating the opposing placental structure with a trocar, catheter, or other analogous instrument. And the later records of midwifery contain several cases in which this perforation of the placenta has, in complete presentations of the organ, been successfully adopted, both as regards the mother and the infant.

Several high authorities, however, in midwifery, have altogether repudiated the evacuation of the liquor amnii, both in partial and in complete placental presentations. They have done so principally under the idea, that if this measure failed to suppress the flooding, the previous escape of the waters would render any subsequent practice that might be required, more difficult of execution. This objection certainly applies to turning, as a subsequent practice, but it does not apply to artificial detachment of the placenta as an ulterior measure of treatment.

The artificial evacuation of the liquor amnii, by perforating either the placenta or membranes, affords assuredly a simple, but by no means a certain, method of restraining the flooding in placenta prævia. It is a practice which is undoubtedly attended with less success in unavoidable than in accidental hemorrhage. But still I believe it to be a mode of treatment to which we may occasionally have recourse with great advantage, especially if there is originally a large quantity of liquor amnii present, and if the flooding is great, while the os uteri is still small and undilatable. We must beware, however, of trusting too much or too long to this, or to any mere palliative measures. Whatever we do, should, if possible, be always done before the hemorrhage is allowed to proceed to such an extent as to induce any very marked symptoms of constitutional debility and depression in our patient. If a decided state of exhaustion has been allowed to supervene, either of the two remaining and ulterior measures, extraction of the child or extraction of the placenta, will be but too liable to prove futile and unsuccessful in their results.

III. *Artificial Extraction of the Child.*—This forms the general principle of management upon which unavoidable hemorrhage has hitherto been treated by most authors and practitioners. The professed object of the practice is this: by forcing the delivery of the child, and thus emptying the uterus, the organ is thrown into full contraction, and hence further loss of blood prevented. The mode in which the indication is fulfilled, is, in some degree, regulated by the state of advancement of the infant, its presentation, &c. In a great proportion of cases the accompanying hemorrhage requires interference at so early a stage of the labor, that the only proper and possible mode of delivery is by the operation of turning; and various authors, as Drs. Denman, Burns, Hamilton, Conquest, and others, speak of turning as the *sole* and *only* mode of treatment applicable to cases of placental presentation. The great objection to it is the imminent danger which the mother necessarily runs from the risk of some laceration of the cervix uteri during this mode of forcible delivery; and any degree of laceration of this part is especially dangerous in placental presentations; for in placenta prævia the structure of the cervix is extremely vascular, being permeated by those numerous and enlarged vessels which are always developed, in a high degree, in the uterine walls opposite the seat of the placenta. The laceration of these vessels leads to immediate danger, from draining hemorrhage after delivery, and to more remote danger, from inflammation being liable to spring up in the torn and wounded sinuses of this part, and extreme uterine phlebitis following as a direct consequence. But still I hold turning to be the proper mode of practice in unavoidable hemorrhages which cannot be restrained by less active measures, and where immediate delivery is demanded, with the os uteri well dilated, or easily dilatable, and the child still alive, or presenting transversely.

Besides turning, other modes of artificial delivery of the infant are occasionally resorted to in placental presentations. If the attendant flooding is such as not to require forced delivery till after the waters are evacuated, and the head well advanced in the passages, then version would be dangerous and inapplicable, and the use of the forceps offers the safest and easiest mode of extracting the infant. Further, in original pelvic presentations, extraction may be at any time effected, when required, by seizing and dragging at the feet of the child.

IV. *Artificial Separation of the Placenta.*—The arrestment of unavoidable flooding by total detachment of the placenta should, I believe, be our line of practice when the combination of circumstances is as follows, viz., the hemorrhage is so great as to show the necessity of interference, and is not restrained or restrainable by milder measures, such as the evacuation of the liquor amnii; but, at the same time, turning, or any other mode of immediate and forcible delivery of the child, is especially hazardous or impracticable, in consequence of the undilated or undeveloped state of the os uteri, the contraction of the pelvic passages, &c. Or, again, the death, prematurity, or non-viability of the infant, may not require us to adopt modes of delivery for its sake, that are accompanied (as turning is) with much peril to the mother, provided we have a simpler and safer means, such as the detachment of the placenta, for at once commanding and restraining the hemorrhage, and guarding the life of the parent against the dangers of its continuance. Hence, as I have elsewhere stated, I believe that the suppression of the flooding by the total detachment of the placenta will be found the proper line of practice in severe cases of unavoidable hemorrhage, complicated with an os uteri so insufficiently dilated and undilatable as not to allow of version being performed with perfect safety to the mother: therefore, in most primiparæ; in many cases in which placental presentations are, as very often happens, connected with premature labor and imperfect development of the cervix and os uteri; in labors supervening earlier than the seventh month; when the uterus is too contracted to allow of turning; when the pelvis or passages of the mother are organically contracted; when the child is dead; when it is premature and not viable; and where the mother is in such an extreme state of exhaustion as to be unable, without immediate peril of life, to be submitted to the shock and dangers of turning, or forcible delivery of the infant. This enumeration is far from comprehending all the forms of placental presentations that are met with in practice; but it certainly includes a considerable proportion of the cases of this obstetric complication, and among them, all, or almost all, of the most dangerous and most difficult varieties of unavoidable hemorrhage. In adopting the practice, one error, which I would strongly protest against, has been committed in some instances. Besides completely detaching and extracting the placenta, the child has subsequently been extracted by direct operative interference. If the hemorrhage ceases, as it usually does, upon the placenta being completely separated, the expulsion of the child should be subsequently left to nature, unless it present preternaturally, or the labor afterwards show any kind of complication which of itself would require operative interference under any other circumstances. Both to detach the placenta and extract the child would be hazarding a double instead of a single operation.

Comparative Mortality attendant upon Turning, and upon the Total Separation of the Placenta.—One circumstance which strongly led me to advocate, in unavoidable hemorrhage, the preference of the detachment of the placenta to the operation of turning the child, was the fact of the great mortality which followed the latter operation, as contrasted with the few mothers that died when the placenta was spontaneously expelled, or accidentally extracted before the infant. In speaking of the relative maternal mortality resulting from the two modes of practice, Mr. Barnes very properly points out that when I spoke of the mortality attendant upon the separation of the placenta before the child as amounting to 1 in 14 only, 10 mothers in 141 having died, I had included cases in which the placenta was thrown off spontaneously before the child, along with others in which it was artificially detached; and he doubts if the results would not be "widely different," if the statistics comprehended the latter class of cases only, "in which the severe operation of detaching the placenta by the introduction of the hand had been resorted to." The best answer to this objection consists in a statement of the results hitherto obtained from the practice of artificially detaching the placenta.

"Seventeen cases," says Dr. West,[1] "have been recorded in the English journals during the past fifteen months, of detachment of the placenta before the birth of the child, in cases of placenta prævia. In the case recorded by Dr. Simpson, to whom it had been communicated by Mr. Cripps, the placenta was removed by an ignorant midwife, and ten hours elapsed before the child was born, during which time, however, no hemorrhage took place. In 16 out of the 17 cases, the bleeding is said to have ceased immediately on the detachment of the placenta; but Dr. Everitt mentions, that although the flooding abated on the separation of the placenta, it did not entirely cease until after the application of cold externally; and he insists on the fact as proving, in cases of this kind, the hemorrhage comes from the uterine as well as the placental ends of the lacerated veins. The life of the mother was preserved in every case but one (out of the 17), and then the previous hemorrhage had been so profuse as almost to exhaust the patient, who died three hours after delivery. All the children were still-born, except in the case related by Mr. Stickings."

I do not stop to inquire whether in one and all of these 17 cases, the artificial detachment and extraction of the placenta ought to have been followed. At present I adduce them, not as affording evidence of the propriety of the practice, but as affording evidence of its safety.[2]

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[1] See his able Midwifery Report for 1845–46, in Dr. Forbes' Review for January, 1847, p. 286.

[2] Dr. Waller, Lecturer on Midwifery at St. Thomas' Hospital, London, and a gentleman who has had extensive experience in this and other morbid complications, two years ago published a long series of interesting cases of placenta prævia. In a late addition to his former communications in the Medical Times, he observes, "My confidence in Professor Simpson's plan in these cases, remains unshaken. It will be seen, in reference to my cases, that in nine instances the placenta was separated, either naturally or artificially, before the birth of the child. The same effect was produced in all, namely, *the cessation of the hemorrhage;* and eight of these women recovered. Surely this result could not have been accidental, as some would have us imagine. I am not," Dr. Waller adds, "aware that any cases have been recorded wherein hemorrhage has *continued* after complete separation." See a Review of Dr. Churchill's Midwifery, by Dr. Simpson, in Edinburgh Monthly Journal for July, 1850, p. 33, where the above question is discussed.—(Ed.)

In proof of the maternal mortality under the old and recognized forms of practice being greatly higher than under the proposed plan of the extraction of the placenta before the child, Mr. Barnes refers, apparently with some hesitation, to the statistics collected by Dr. Churchill and myself, as showing that 1 in every 3 mothers was usually lost in placental presentations. Among 174 cases of unavoidable hemorrhage collected by Dr. Churchill, 48 mothers died. I have now before me (see page 601), a carefully collected list of 654 cases of placental presentations reported by Mauriceau, Portal, Giffard, Smellie, Rigby, Clarke, and Collins, Schweighauser, Lachapelle, Drs. John and Francis Ramsbotham, Lee, Lever, and Wilson. Among these 654 cases, 180 mothers died, or 1 in $3\frac{6}{10}$. In corroboration of the correctness of the statistical view which Dr. Churchill and I have taken of the extent of maternal mortality in unavoidable hemorrhage, I would further beg to refer Mr. Barnes to the observations of Dr. Robert Lee.[1] Dr. Lee states a number of statistical facts regarding uterine hemorrhage from placental presentations, and, amongst other matters, he mentions the result to the mothers in a considerable number of cases. I shall throw all his evidence on this last point into a tabular form:

Maternal Mortality in 72 Cases of Placental Presentations, as noted by Dr. Lee.

Reporters.	No. of Cases Reported.	No. of Mothers Lost.
Dr. Clarke,	14	1
Dr. Collins,	11	2
Dr. Ramsbotham,	19	8
Dr. Lee,	38	14
	72	25

Hence, according to Dr. Lee's collection of statistics, the maternal mortality in unavoidable hemorrhage, amounting to 25 in 72 cases, is rather more than 1 in 3. And this evidence of Dr. Lee will probably be regarded as the stronger, seeing that it is totally unprejudiced in its character, for, in 1845, Dr. Lee called into doubt the accuracy of all collections of statistical data, made by others, which led to the idea that the general maternal mortality in unavoidable hemorrhage was so great, as to approach 1 in 3. At that time he was, I believe, unaware of the general result of his own previously published collection of statistical data relative to the point in question.

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[1] Midwifery Lectures, pp. 370, 371, published in 1844.

SPECIAL AVERAGE AMOUNT OF MATERNAL MORTALITY OBSERVED IN CASES OF TURNING FOR PLACENTA PRÆVIA.[1]

On another occasion I took an opportunity of stating, and proving by comparative statistics, that "those mothers who are the subjects of placental presentations are submitted to as great peril of life from this obstetric complication as they would be if seized with yellow fever or malignant cholera." This refers to placental presentations taken irrespectively of any special forms of treatment, or, indeed, of any treatment at all. The table on page 601 is a proof of this, 180 mothers dying out of 654 cases, or 1 in $3\frac{6}{10}$. But though placental presentations, taken as a whole, may thus be so very fatal, it has been repeatedly argued, that in cases of unavoidable hemorrhage, in which the child is extracted by the operation of *turning*, the mortality must be different and less in amount. I doubt this entirely. Out of the 654 cases forming the table previously published, in 421 the child was extracted by turning. The following table shows the result:

Table of Maternal Mortality in Placental Presentations treated by the Operation of Turning the Child.

Reporters.	Number of Cases.	Mothers lost.
Mauriceau,	14	1
Portal,	12	1
Giffard,	18	5
Smellie,	8	3
Rigby,	35	9
Clarke and Collins,	8	4
Busch,	5	2
Schweighauser,	46	11
Lachapelle,	13	6
J. Ramsbotham,	86	40
F. Ramsbotham,	96	37*
Lever,	30	7
Lee,	28	10
Wilson,	22	8
	421	144

From the above table it appears, that out of 421 placental pre-

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[1] From Lancet, October, 1847, p. 381.

* In the controversy with Dr. Lee, the latter considered that 26 of the Drs. Ramsbotham's cases ought not to be included in this table. If these, however, be omitted, there will remain 156 cases; the result of which was as follows:

Table of 156 selected Cases of Turning in Placental Presentations, the operation not having been over-delayed.

No. of Cases.	Maternal Deaths.	Proportion of Maternal Deaths.
156	48	1 in $3\frac{3}{10}$

See particulars in Lancet for November, 1847, p. 520.

sentations in which the child was removed by turning, the result was fatal to the mother in 144 instances; or, in other words, the mothers were lost in the proportion of more than one in three (one in two and nine-tenths). Upon the continent of Europe, the Cæsarean operation—reputed the severest in midwifery—has been fatal to the mothers, according to Dr. Churchill, in 154 out of 371 cases, or in 1 in $2\frac{4}{10}$.

Lithotomy—generally regarded as one of the most formidable operations in surgery—is calculated by Drs. Willis and Inman to have an average mortality of one in every seven or eight operated upon. The fatality attendant upon placental presentations treated by the obstetric operation of turning, is more than twice greater than that accompanying the surgical operation of lithotomy.

When turning is had recourse to in some of those more severe complications of unavoidable hemorrhage, which I have proposed to treat by the total detachment of the placenta, the maternal mortality is still even greater and more startling. Out of *eleven* cases of placental presentation reported by Dr. Lee in his "Clinical Midwifery," and where there was the combination of "more or less *rigidity* of the os uteri with dangerous hemorrhage," *eight* of the mothers died, or seventy-two per cent.; and two out of the only three who did survive, evidently made very narrow escapes from death.

After giving a tabular view of these eleven cases on another occasion,[1] I observed, that consequently "the operation of turning and artificial delivery in unavoidable hemorrhage, with the os uteri imperfectly dilated, would, from these and other cases, appear to be more deadly than any operation that is deemed *justifiable* in the whole circle of surgery." And I sincerely believe that in such cases the simple but complete separation of the placenta without its removal, which could be accomplished in circumstances under which turning was utterly impracticable—would at once arrest the hemorrhage, and thus place the mother in a state of comparative safety. "But," anxiously argues Dr. Lee, "to have attempted in those cases to tear away" (separate) "the placenta, would have been an act of insanity." "It would, undoubtedly," as anxiously argued Dr. Brown, some twenty or thirty years ago, "be *downright* madness to imagine they will condescend to encourage vaccination."[2]

For details of the controversy with Dr. Lee concerning the treatment of Placenta Prævia, see London Medical Gazette, 1845; and Lancet, 1847, 1848, and 1851.—(Ed.)

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[1] Medical Gazette for 1845, p. 1013.

[2] Edinburgh Medical and Surgical Journal, vol. xv. p. 64.

TRANSFUSION IN HEMORRHAGE.[1]

(From Edinburgh Monthly Journal of Medical Science, February, 1849, p. 560.)

Dr. Simpson had seen transfusion tried in several consultation cases, where the patients were dying from the results of uterine hemorrhage. In one case, the lady, who was infirm and jaundiced before, kept well for an hour and a half after delivery. Sudden hemorrhage then supervened, which was speedily and entirely arrested by Dr. Purdie; but the patient was left by it in a collapsed, pulseless state, and was incapable of being rallied by the strong stimulants used. Dr. S. saw her about an hour subsequently, and injected into the basilic vein twelve ounces of a saline solution, like that used here in the last cholera epidemic. The solution roused the patient for a short time, but not much; and she remained three hours afterwards in the same sunk, collapsed state, and died without any renewal of the flooding. In another case he had seen the same practice followed, and when the patient was quite moribund, with no result and no rallying. Three or four years ago, he was called to a case a few miles from Edinburgh, where he found the patient left in the same collapsed and pulseless state, after an early miscarriage, accompanied with much hemorrhage. In this as in the two other cases, the state of pulseless collapse, not capable of being affected by brandy or other stimulants, continued for hours after the hemorrhage was stopped, and before the patient died. A surgeon was called from town, but failed in finding a vein large enough to inject. These results did not seem to promise much or any benefit from the injection of saline fluids, intended merely to fill and re-distend the vascular system. He had injected pure blood in another case, of Dr. Millar's, similar to the last; the patient rallied and recovered. But he had seen the treatment fail in various other instances—lately in two extreme cases to which he was called in the course of the same night.

DOES AIR EVER BECOME INTRODUCED INTO THE VENOUS SYSTEM

THROUGH THE UTERINE SINUSES AFTER DELIVERY?[2]

(From Edinburgh Monthly Journal of Medical Science, April, 1849, p. 707.)

A series of cases that Dr. Simpson saw, in consultation, and that occurred in rapid succession, six or eight years ago, seemed to him to suggest the idea whether air might not become introduced into the venous circulation of the mother after delivery, and whether one common result of this accident was not the occurrence of a red or scarlatinoid eruption upon the cutaneous surface of the patient.

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[1] Extracted from Proceedings of Edinburgh Obstetric Society, 1848.

[2] Ibid. Jan. 10, 1849.

The first case occurred in the Lying-in Hospital. The patient had been delivered of twins. Dr. Ziegler was called to visit her, in consequence of some difficulty connected with the birth of the second child. Considerable post-partum hemorrhage, with alternate contractions and relaxations of the uterus, supervened, and she seemed to rally very imperfectly from the effects of the flooding. In consequence of this, Dr. S. saw her an hour or two subsequently. The pulse at that time was very rapid and weak, almost imperceptible. The countenance extremely anxious, and here and there was an evanescent red scarlatinoid rash over the body. The patient died in a few hours. The body was opened a short time after death, because it was considered desirable not to incur the fallacy of air being present from decomposition; and Drs. Simpson and Ziegler were anxious to ascertain if the anomalous symptoms that were present during life could possibly arise from the entrance of air into the veins. To make the examination the more certain, the abdomen was opened under water. The lower vena cava, but especially the uterine and hypogastric veins, were distended with frothy blood, and air bubbled up through the water when any of these tubes were opened. The larger veins in the extremities were in the same state.

Some search was subsequently made into the literature of cases in which air had been introduced into the veins during surgical operations upon the neck, &c., in order to ascertain if the evanescent patches of red or scarlatinoid rash remarked in the preceding instance, had been observed in any of these surgical cases. Dr. Warren of Boston, in an article on the subject in the American "Cyclopædia of Practical Medicine," relates two cases which occurred in his own surgical practice. The first patient recovered after being insensible for a considerable time. While still comatose, "the leaden color in the cheeks," says Dr. Warren, "assumed a *reddish* tinge, and the alarming character of the symptoms was evidently diminished." The second case was fatal. During the period elapsing between the entrance of the air, and the death of the patient, Dr. Warren, in describing the symptoms, remarks, "The livid color of the cheeks gave place to a suffusion of *vermilion red*, and no glow in the cheek of a youthful beauty ever gave one so much pleasure as that flush. But the flush soon passed off."

If the red or scarlatinoid rash which appeared in the preceding obstetric case upon the surface of the skin were owing to the entrance of air into the uterine veins, might the symptom be accounted for on the idea, that the introduced air directly mixed with, and oxygenated, the blood in the capillary vessels?

After the preceding case occurred, Dr. Simpson was called to three or four other instances in which a similar train of symptoms was present—namely, great depression after delivery, a rapid and almost imperceptible pulse, and patches of an erratic scarlatinoid rash upon the surface. All the patients died within two or three days after delivery. In one case, which he saw with Mr. Kerr, and where the death was much more rapid, no red rash appeared, although the other symptoms were analogous. The first was the only case in which an autopsy was procured. Two or three of the cases had been considered as malignant scarlatina, a disease which was acknowledged by all our highest obstetric authorities to be exceedingly fatal to the puerperal female, but which may possibly in many other cases have been mistaken for, and confounded with, the affection to which this notice refers.

Supposing the introduction of air into the venous system after delivery to be the cause of the symptoms or affection in question, it is not perhaps difficult to understand the mere mechanism of its introduction. Surgical pathologists generally hold, that the air cannot be *drawn* into open veins, unless these veins are not very distant from the heart. But air may perhaps be *forced* into open veins when the open orifices are at a distance from the heart, and when circumstances exist capable of causing it to enter into such orifices.

A mechanism calculated to produce the entrance of air in this last way, exists in the uterus after delivery. The interior surface of the organ, especially opposite the late seat of the placenta, has a number of ruptured venous orifices opening upon it. Supposing air once introduced into the uterine cavity, which in some cases may occur in consequence of the alternate relaxations and contractions of the walls of the organ following delivery (as in after-pains, post-partum hemorrhage, &c.); and supposing, further, that under the returning contractions of the organ, the expulsion of this air from the cavity was prevented by the presence of a clot at the os uteri, or other such obstructing cause, it will then, under the compression to which it is subjected, be liable to be driven into the open venous orifices existing in the lining membrane of the uterus. A bottle of caoutchouc of the shape of the uterus, and with tubes like the venous tubes of the uterus opening upon its interior, would act in this way on the principle of the force-pump, in sending any air sucked or drawn up into its cavity along these open tubes, if, after being full, its parietes were compressed, and the aerial contents prevented from escaping through its mouth, as we fancied to be the case in the uterus under the conditions mentioned.

See also Dr. John Reid's Researches, 1848, p. 578. Appendix by Dr. Simpson to the Essay on Air introduced into the Veins.—(Ed.)

SUDDEN DEATH AFTER DELIVERY: HEART DISEASE.[1]

(From Edinburgh Monthly Journal of Medical Science, May, 1849, p. 767.)

Dr. Simpson stated that, ten or twelve years ago, when acting as resident house surgeon in the Lying-in Hospital, he was called by a midwife to see a patient who had suddenly fainted and died, immediately after the expulsion of the infant. The uterus, as felt through the abdominal parietes, appeared firm and contracted. The labor had been natural, without any hemorrhage or other complication. An autopsy was not obtained; but the anterior history of the patient showed the probability of the existence of diseased heart.

A short time afterwards a patient, attended by one of the pupils of the hospital, rose up and stood for the first time, about a week after delivery. She immediately fainted and expired. There had been nothing particular in the history of the labor or convalescence.

In a case, in regard to which he was consulted by Dr. Buchanan, the patient died suddenly about three hours after delivery—the fatal event being preceded by cough and hæmoptysis. The symptoms before delivery were those of disease of the mitral valve, and in consequence, Dr. S. advised her not to have chloroform in her labor.

Drs. Denman, Clarke, Blundell, Meigs, &c., had recorded notices of cases of this kind of sudden death during labor, or after it, without convulsions or hemorrhage.

Dr. S. had always taught that the existence of heart or chest disease formed a kind of complication in labor, which was not only extremely anxious in itself, but which generally incapacitated the patient from bearing with impunity, for any great length of time, the struggles and efforts of a prolonged second stage. He considered such a complication an indication for earlier instrumental assistance than would be deemed necessary under other circumstances.

INVERSION OF THE UTERUS, ITS PATHOLOGY AND TREATMENT.[2]

(From Edinburgh Medical and Surgical Journal, January, 1840, p. 170.)

Inversion of the uterus is an accident which is sometimes concealed by the practitioner, because it is an occurrence which is very

[1] Extracted from Proceedings of Edinburgh Obstetric Society, Jan. 10, 1849.

[2] From a Review of Dr. Radford's Obstetric Essays.

generally, but very undeservedly, conceived both by the patient's friends, and by the medical man himself, to attach blame to his character. This feeling is apt to paralyze the energies of the surgeon, and to prevent him from immediately adopting those measures which are necessary for the reduction of the inverted organ. It is this procrastination which is blameable, and not the accident itself, which is one, as Dr. Radford has shown, that may happen in the best hands, seeing that it sometimes depends not so much upon any practice which the surgeon may have adopted, as upon morbid actions taking place in the uterus over which he has no control.

Most authors attribute inversion of the uterus to causes purely mechanical—such as pulling rudely and violently at the funis in order to remove the placenta. But that this is not always the origin of inversion, is evident from a number of circumstances. The accident has happened repeatedly when the cord was of its usual length, before any force has been applied to it. It has been found before the child was attempted to be separated by dividing the cord; and it has occurred when the cord has been so putrid as to break with the slightest effort. We might refer also to cases where it has happened when the process of parturition was left entirely to nature; and in support of the same view of the subject, Dr. Radford might have quoted the cases recorded by Boerner[1] and Klaatsch,[2] in which this inversion took place in instances where the child was expelled some hours after the death of the mother, and when there was no person present to interfere with the process. According to the prevalent idea above alluded to, of the mechanical origin of inversion of the uterus, it is believed that the uterus is in a state of relaxation and atony, so that it offers no resistance to any force applied through the medium of the funis. Dr. Radford, however, has shown, by the history of several cases which he has himself met with, and of others recorded by Cleghorn, Brown, Merriman, &c., that the fundus and body of the uterus, so far from being in a state of relaxation, are generally, if not always, in a state of unnatural contraction, as evinced by the strong and violent uterine pains which are present, and by the firmness and hardness of the inverted viscus. At the same time, however, that the fundus and body of the uterus are in this state of unnatural excitement and action, the os uteri appears to be comparatively relaxed and dilatable, seeing that it offers no resistance to the protrusion of the tumor through it. In other words, it is evident, that the fundus and os uteri are in directly opposite conditions; the former is in a state of violent contraction, the latter in a state of atonic relaxation, and this relative difference in these two parts of the organ is indispensably necessary, as Dr. Radford conceives, for the possibility of the occurrence of inversion. In other words, it arises, like the hour-glass contraction of the uterus, from an inequality in the time and degree of contraction of the different parts of the organ; and in Herbin's Thesis on Incarcerated Placenta, Dr. Radford will find a case of this kind highly illustrative of his views of the pathology of inversion, inasmuch as in the case alluded to, the portion of the uterus which formed the chamber retaining the placenta was, notwithstanding the bulk of its contents, so firmly contracted, that it passed through the very relaxed os uteri, and projected into the vagina.

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[1] Loder's Journal für Chirurgie, &c. vol. i. p. 519.

[2] Henke's Zeitschrift for 1826, vol. iii. p. 45.

At the same time, while we freely confess that we believe Dr. Radford has proved that his explanation applies to the majority of cases of inversion of the uterus, we can easily conceive other instances in which, the whole uterus remaining uncontracted, a mechanical cause, such as strong traction at the cord of an adherent placenta, may lead to the same frightful accident, in the way pointed out by Spence, Clarke, Gooch, Gardien, and most of our other systematic writers on midwifery.

The proper diagnosis of such a dangerous accident is often a point of great importance. In a case detailed in the Gazette Medicale for 1832, the inverted uterus was mistaken for a polypus, and the woman died in consequence of inflammation excited by an attempt to remove it. Dr. Radford details two cases in which the tumor formed by the inverted uterus was mistaken for the head of a second child. We know of one distressing case of this kind, where the midwife in attendance fell into the same mistake, and had dragged the uterus far down between the thighs of her patient before she discovered her error, and sent for further assistance. We have heard of another case where the uterine tumor in the vagina was mistaken for the body of the placenta, and pulled outwards, under this idea, with fatal violence.

Dr. Radford's remarks on the diagnosis of inversion are extremely valuable on this account; that he has shown, contrary to the opinion generally entertained by obstetric authors, that hemorrhage rarely occurs to any great extent in cases of this accident, except when the placenta is partially detached or broken up in its structure, or where the uterus itself has been lacerated.

In the *treatment* of inverted uterus, and in attempting that reduction of the organ which always forms our most immediate indication, Dr. Radford strongly advises us, provided the inversion is complete, and the placenta still attached, to remove this latter mass, before we endeavor to compress and return the uterus itself. He has, as we have just hinted, fully shown that we need not be deterred from this practice by any dread of hemorrhage. If the placenta, he observes, be completely detached from the uterus, this organ contracts as under ordinary circumstances, and the bleeding ceases. We need scarcely point out how much the separation of the placental mass will facilitate our attempts at reduction, and how highly necessary it is to effect this, if possible, before the inverted viscus becomes enlarged from congestion, and before the os uteri becomes too much irritated and contracted to admit of its easy return. In one case, Dr. Denman was unable to reduce the organ after the lapse of four hours. We must not, however, despair, if unsuccessful in our first efforts, or during the first stage. The inverted uterus has been reduced now in a number of instances, at a distance of several days from the occurrence of the accident, in cases where the threatened inflammatory symptoms had been kept in abeyance by the active measures that were adopted. Mr. Cawley,[1] in one instance, reduced the organ after it had been down three days; and in other cases Dr. Radford reduced it after seven, and Mr. Ingleby after eight days. In one case Dr. Belcombe[2] succeeded, though the inversion had existed as long as twelve weeks. Cautious attempts of this kind, where there is no inflammatory complication, may, therefore, in some cases be warrantable, at a late period after the accident, with a view of saving the patient from those constant discharges, and other annoyances and miseries which the disease, when not remedied in its first stages, very generally entails.

It is perhaps unnecessary to point out how very greatly the replacement of the organ is facilitated by the use of anæsthetics. On the diagnosis of inversion, see previously, p. 120.—(Ed.)

ALBUMINURIA

IN PUERPERAL AND INFANTILE CONVULSIONS AND IN PUERPERAL AMAUROSIS, ETC.

(From Edinburgh Monthly Journal of Medical Science, October, 1852, p. 369.)

Above fifty years ago, Hamilton[4] and Demanet[5] first stated the important fact, that puerperal convulsions were liable to be preceded by symptoms of anasarca in the pregnant mother. The truth of this remark has been subsequently confirmed, in incidental observations made by Duges, Burns, Montgomery, Ingleby, Johns, and others. The special pathological nature, however, of the œdema or anasarca preceding and predisposing to puerperal eclampsia remained uninvestigated, nor was any direct morbid relation attempted to be traced between the dropsy and convulsions. Previously to my first course of lectures on midwifery in the University of Edinburgh in 1840–41, I had, in more than one case, ascertained the œdema or anasarca seen in patients affected with puerperal convulsions, to be one of the numerous and important forms of dropsical disease which Dr. Bright had shown to be connected with the existence of albumen in the urine. Up to 1843 I had detected, in repeated instances, this connection of puerperal convulsions with albuminuria, but hitherto I had found it only by examination of the urine during life. In the spring of 1843 I saw a fatal case of puerperal convulsions, in which, in addition to the detection of albuminuria during life, I had an opportunity of observing the usual granular disease of the kidney on post-mortem inspection.

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[1] Lond. Med. Journal, vol. vi. p. 366.

[2] Med. Gazette, vol. vii. p. 783.

[3] We have preferred including convulsions among the complications of labor, to which, or to pregnancy, it as justly belongs, rather than among the more peculiar affections of the puerperal state.—(Ed.)

[4] Duncan's Annals of Medicine for 1800, vol. v. p. 313. "Where œdematous swellings of the lower extremities take place to a considerable extent in the latter months of pregnancy, in women of an unimpaired constitution, copious bloodletting alone prevents the occurrence of convulsions either before or during labor." See also his Practical Observations, p. 354. "The fits are preceded most frequently by lancinating pain of the head, sometimes by crampish pain of the stomach, and sometimes by œdematous swelling of the face and upper parts of the person."

[5] Recueil Periodique de la Societé de Médecine, tom. ix. 1801-2, p. 110. He regards "l'anasarque comme une de leurs causes *essentielles*."

Case I.—A woman, pregnant for the third time, and whose health had latterly been impaired, was seized with severe puerperal convulsions, in consequence, as her friends supposed, of strong mental excitement. I saw her, along with her medical attendant, about twelve hours after the convulsions began. She was at that time quite comatose in the intervals between the fits. In spite of venesection and various other measures of treatment, which were tried with the hope of relieving and rousing the patient, the convulsions continued, the coma deepened, the extremities became cold, the circulation began to fail, and the patient was evidently hopelessly moribund. The child's heart, however, still continued to beat, as was ascertained from time to time by the stethoscope; and the principal indication left was the preservation, if possible, of its life. The os uteri had, from the imperfect labor which had supervened from the attack of the convulsions, opened to about the diameter of a shilling, but its structures were rigid. In order to extract the child, I followed the plan of turning recommended by Dr. Hamilton, and with far more facility than I anticipated *à priori*—viz., I tilted and pushed upwards and aside the presenting head with two fingers of the right hand introduced per vaginam, while, at the same time, by manipulating upon the child with the left hand through the abdominal and uterine parietes, a lower limb was at last brought near the opening of the os uteri, and seized. The relaxed state of the uterus and adjoining parts, resulting from the deep coma of the patient, no doubt greatly facilitated this version. On attempting to drag the body of the child through the os uteri, by pulling at the extended limb, I found the rigid structures of the undilated cervix to resist altogether the passage of the trunk. Under these circumstances, I made two slight incisions into the cervix, one on each side, and on the reapplication of extractive force, the breech now passed the yielding os uteri, and the birth of the child was readily effected. The child was born alive, and it survived and throve well. The placenta escaped without any hemorrhage. The mother died in the course of two or three hours after the birth of the child. A post-mortem examination of her body was made by Professor Bennett. The lateral incisions into the lips of the undilated os, and the laceration or fissuring accompanying these incisions, were found not to extend beyond the duplicature of tissue forming the vaginal portion or projection of the cervix uteri; and had not been followed by any hemorrhage. There was no blood, fluid or coagulated, in the cavity of the uterus or vagina, or around the incisions. The kidneys presented a well-marked specimen of granular degeneration, probably of some standing.

In allusion to the preceding instance (see p. 733) I stated in the "Monthly Journal" for 1843, that this case "offered me the first opportunity of confirming, by inspection after death, an opinion that I had been led to adopt from the examination of the symptoms during life, and had publicly taught for the last two sessions, viz., that patients attacked with puerperal convulsions had almost invariably albuminous urine, and some accompanying, or rather preceding dropsical complication, and hence probably granular renal disease."[1]

My friend Dr. Lever of London, who happened also to be directing his attention at the same time to the connection of puerperal convulsions with albuminuria, published in the last number of "Guy's Hospital Reports" for 1843, several cases of puerpera-eclampsia, in five of which he had found the urine to be albumilnous; and to these cases he appended some excellent remarks on the probable relations between these two morbid states. Of late years, the same subject has been investigated, and the connection between albuminuria and puerperal eclampsia more or less elaborately traced and discussed by various continental authors, particularly by Cahen and Bouchut, Rayer, Devilliers and Regnauld, Depaul, Caseaux, &c.

Since the above date (1843) I have seen a variety of cases of puerperal convulsions in consultation and hospital practice, and have always (with very rare exceptions indeed) detected the existence of albuminuria in the urine of the mother. In one or two instances (see p. 733) I have found the kidneys presenting traces of recent acute inflammation; as pus, &c. Sometimes, as in the case detailed above, convulsions, or symptoms threatening them, recur in successive labors in the same mothers in connection with established granular disease. Usually, however, the state of albuminuria which leads to puerperal convulsions, is a transitory morbid condition, from which the patient recovers within the course of a few days after delivery; and the affection does not depend upon, or result in, any actual change of structure in the kidney. And it may be that the premonitory œdema, headaches, &c., and the actual convulsions themselves, do not stand in the relation of effect, to albuminuria or renal disease as a cause, but that all of these circumstances—the dropsy, the convulsions, and the albuminuria—are simultaneous or successive effects of some one common central cause, viz., a pathological state of the blood, to the occurrence of which, pregnancy in some way peculiarly predisposes, probably from various acts of secretion, nutrition, and depuration being vastly increased and altered by the conditions of utero-gestation. Occasionally, however, the state of albuminuria, when once induced, will continue for several weeks after delivery. Some time ago I attended, with Dr. Fairbairn, a case that was peculiar in this respect, as well as from the lateness of the occurrence of fatal convulsions.

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[1] Monthly Journal of Medical Science, Nov. 1843, p. 1015.

CASE II.—The lady had been confined in the country without any symptoms of eclampsia. She came to Edinburgh about seven weeks after her accouchement. When Dr. Fairbairn and I then saw her together, her particularly leuco-phlegmatic color, some lesions of the senses, her occasional fits of stupor and want of memory and the other undefined symptoms of which she was complaining, led me to suggest the propriety of testing the urine, in order to ascertain if it contained albumen. It was found highly albuminous. In the course of eight or ten days subsequently, our patient was suddenly seized with convulsions, followed by coma, under a repetition of which she soon expired. No case has, so far as I know, been put on record, in which eclampsia supervened so very late after delivery.

In the instance just now referred to, the albuminuria was, there was reason to believe, present before the termination of the patient's pregnancy; but did not lead to any attack of convulsions during or immediately after labor. And I have now seen a number of such instances in which the urine was albuminous during the last days or weeks of pregnancy, without convulsions, or any other special morbid phenomena, supervening in connection with labor.

In several of these instances, temporary, and in one or two, more permanent, amaurosis supervened at the time of delivery; accompanied in most, but not in all, with intense headache.[1] During the course of the present year, I saw one such case of puerperal amaurosis in connection with albuminuria, in a patient at the sixth month of pregnancy; and the albuminuria in this, as in some other cases, tended to bring on premature labor.

CASE III.—It was the patient's second pregnancy. Her face had looked swelled for a day or two previously. During the night she complained of intense headache. In the morning she complained of such a degree of blindness, that she could not distinctly see objects and persons. The urine was highly albuminous. She was freely bled. True labor pains supervened early in the forenoon. She was placed under the influence of chloroform for some hours, and delivered of a premature child, which was alive, but did not survive. The amaurosis in a great measure disappeared after the bleeding, and the patient's recovery after delivery was speedy and perfect, the albuminuria passing off within a week subsequent to her confinement.

Lately I have visited, with Mr. Sidey, an interesting case of more permanent amaurosis connected with the puerperal state and chronic albuminuria:

CASE IV.—The patient, now aged 36, is the mother of six children. In 1847, two days after the birth of her fifth child, she became totally blind in the course of a single night, the amaurosis being found complete when daylight came on. The blindness, however, gradually and entirely passed off in a few days. During the second week following the birth of her last child, in July, 1850, she again became suddenly and completely blind, with some accompanying symptoms of stupor, and a very slow pulse. The amaurosis has not, however, altogether disappeared on this occasion as after the former attack. The patient's vision is still (September, 1851) so imperfect that she cannot read; her memory is extremely defective; she often forgets the proper word to use in the middle of a sentence; the iris now contracts, but for some time was dilated and immobile. The last child died of convulsions about a week after birth. Mr. Sidey discovered the urine to be highly albuminous upon her first attack of amaurosis, and has found this state continuing in repeated examinations of it from that time to the present.

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[1] Drs. Bright and Barlow observed amaurosis in four instances of albuminuria; but these cases were not connected with pregnancy. M. Landouzy states, that he has seen thirteen cases of weakness of vision commence, cease, and reappear with albuminuria, and without any appreciable change in the eye or its appendages. And he considers some degree of amaurosis as a common complication with albuminuria.—See Archives Générales de Médecine for Nov. 1849, p. 370. Hamilton and other authors incidentally mention amaurosis as a symptom connected with cases of convulsions. Dr. Ingleby has published a case in which a patient was affected with common puerperal convulsions in her first pregnancy; and in a subsequent accouchement, was attacked with complete amaurosis, which continued during the whole period of her labor. Vision was gradually restored.—See his Facts and Cases in Obstetric Medicine, p. 53.

Four years ago, I met with the following instance of the complication in question:

CASE V.—A patient who was to be under my care at her confinement, sent for me several weeks before her expected time, to tell me that her vision had become so imperfect, that she found she could not distinctly see the trees before her window. There was no other special symptom present; but this degree of amaurosis led me to examine the urine, which I found to be highly albuminous. During the few succeeding weeks, the amaurosis increased, and in addition, symptoms of hemiplegia slowly and gradually came on. Convulsions did not supervene, as I feared they would, during the labor, which was somewhat premature. The child survived. After delivery, the mother recovered greatly, but not entirely, from the nervous lesions, and is still suffering under a slight degree of hemiplegia.

Other lesions of the nervous system may present themselves under the same circumstances. Some of these lesions I have already noticed elsewhere (see p. 732). In fact, writers upon midwifery have long stated to us as premonitory symptoms of puerperal convulsions, various lesions of the nervous system, as headache, giddiness, derangements of sight and hearing, &c., and have told us, that when more or fewer of these symptoms make their appearance, an attack of convulsions is to be feared in connection with labor, but does not always supervene. These so-called premonitory symptoms of convulsions, however, are only in fact so many symptoms of acute albuminuria. They are indicative of the future probability of puerperal eclampsia, inasmuch as they are indicative of the actual presence of albuminuria. And, consequently, whenever they do present themselves, their existence should lead us to examine accurately into the state of the urine, assured that, if they are found to be connected with albuminuria, we may be certain of the liability of our patient to the supervention of convulsions—a liability that, no doubt, may be often lessened or averted by proper antiphlogistic treatment before labor, and by using such means as excite and act freely upon the intestinal, renal, and cutaneous secretions.

A few weeks ago, I saw an instance in which convulsions in a child after birth were connected with the presence of albuminuria in its urine—or connected, as it should be perhaps more correctly stated, with that condition of blood-poisoning or uræmia, which is the result of albuminuria—whether that condition consists in a morbid accumulation of urea, or is produced, as Frerichs[1] supposes, by the presence of carbonate of ammonia in the blood, produced by decomposition of the urea; or is, as is more probable, the effect of same other morbific agent in the circulating system, capable, like strychnia, of increasing the centric irritability or polarity of the spinal system to such an excessive degree that, under this super excitability, comparatively slight eccentric causes of irritation in the stomach, intestines, uterus, bladder, &c. &c., readily induce convulsive attacks of a general form, like those of puerperal eclampsia.

CASE VI.—A lady, pregnant for the first time, was suddenly, when near the full period of utero-gestation, attacked, when rising in the morning, with a severe headache, faintness, and threatening of convulsions. My friend Dr. Weir, under whose professional charge she was, saw her immediately, and bled her largely at the arm, &c. On making a second visit about three hours subsequently, she took, when he was present, a most severe fit of convulsions, which left her in a state of deep coma. Two hours afterwards I saw the patient with him. She was still comatose, and remained so for some hours subsequently. The child's heart, when examined by Dr. Weir and myself, with the stethoscope, and while the mother was still comatose, had only 88 beats in the minute; but in the evening it had risen to its usual rate of 130. Next afternoon labor supervened; the patient was put under the influence of chloroform for some hours; and a living child was born without any recurrence of the puerperal convulsions. The mother made a slow but perfect recovery. On the third day after birth, the child began to suffer under a succession of convulsive attacks, which gradually increased in severity during the next twenty-four hours, when it was placed for a considerable time under the influence of chloroform inhalation, and the fits ceased. After the convulsive attacks supervened in the child, Dr. Weir and I had two opportunities of examining its urine; and on both occasions we found the renal secretion of the infant, like that of the mother, highly albuminous. Some time subsequently, the infant died of inflammation of the cellular tissue of the loins and pelvis. We were not permitted an inspection of the body.

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[1] If the blood poison, which in albuminuria produces convulsions and coma, be, as Frerichs believes, carbonate of ammonia resulting from decomposition of urea, can we account for the power of chloroform in restraining and arresting, as it does, puerperal convulsions, upon the ground of its preventing this decomposition? The inhalation of chloroform produces, as various chemists have shown, a temporary diabetes; sugar appears in the urine, and hence probably also in the blood. The addition of a little sugar to urine *out* of the body, prevents, for a time, the common decomposition of its urea into carbonate of ammonia.

I am not aware that any one has hitherto observed albuminuria coexisting with infantile convulsions; but future observation may perhaps show it to be a common pathological condition in some forms of that disease; and probably in the Trismus Nascentium. In such cases, indeed, the urine has hitherto seldom or never been examined, in consequence of the trouble and difficulty connected with obtaining specimens of it in the affections of infancy. Albuminuria may yet be found to play also an important part in other diseases of infancy. The induration of the cellular tissue, or skin-bound disease (Sclérème—"*L'Endurcissement ou l'Œdeme du Tissu Cellulaire*" of French authors) is an extremely rare affection in Edinburgh. I have only seen two cases of it; and, as was stated in the "Monthly Journal of Medical Science" for 1843, page 699, in both of these instances the urine was coagulable. Hence, at that time, I ventured to suggest, that the skin-bound disease itself, or at least some forms of sclerema, may be a variety or effect of Bright's disease in early infancy; the effusion into the cellular tissue which constitutes the marked feature of the affection, being so far analogous to the anasarca occurring with albuminous nephritis. For the solution of this point, affirmatively or negatively, we can only look to some of those continental pathologists who have ample opportunities of studying the disease in question.

CASES OF PUERPERAL CONVULSIONS CONNECTED WITH INFLAMMATION OF THE KIDNEY.[1]

CASE I.—In this case, the patient, a delicate female, was exhausted by the pains of labor, and complaining of severe headache when the convulsions supervened. Dr. Niven promptly and easily delivered the child, which was dead, by turning. The convulsions gradually subsided, but reappeared several times. In the intervals she was profoundly comatose; and in this state, she died about forty hours after the first attack.

Post-mortem Appearances.—When we opened the lateral ventricle of the right side, fluid blood escaped. The corpus striatum and outer part of the optic thalamus were broken up, and mixed with a large quantity of coagulated blood, forming a clot of large size. The fluid blood was found in the opposite lateral ventricle, also in the third and fourth ventricles. The right kidney was converted into numerous cysts, of about the size of a walnut, containing unhealthy pus, which passed along the ureter and filled the bladder. The left kidney exhibited an advanced stage of Bright's disease.

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[1] Extracted from Proceedings of Edinburgh Obstetric Society, June 15, 1847.—Edinburgh Monthly Journal of Medical Science, September, 1847, p. 212.

CASE II.—I lately saw this case with Dr. Carmichael. The lady had so perfectly recovered after a labor which was quite natural, as to have been out at church, &c. Seven weeks, however, subsequent to delivery, after some sudden anomalous affections of sight and hearing for thirty or forty hours previously, she was seized with the most severe convulsions. Despite free evacuations, &c. &c., they continued to recur from time to time, and proved fatal in three hours; the patient during that time never being perfectly sensible. The pelvis of each kidney was filled with a whitish purulent-like matter, and its mucous lining membrane coated with large patches of adherent coagulable lymph, or false membrane. The ventricles of the brain were distended with serous fluid. The urine, when tested, presented no sign of albumen.

CASE III.—In a third case, one fit of convulsions came on a month before delivery, and recurred again in a severe and fatal form fourteen days after confinement. During the intervening six weeks she was free from any symptoms, and the labor was natural. The last attack came on suddenly in the evening, about nine o'clock; the convulsions were again and again repeated, and she died comatose in eight hours.

With Dr. Maclagan, and Dr. Handyside, I examined the urine during this last attack, but found in it no traces of albumen. On inspecting the body, some whitish turbid fluid was found in the renal pelvis, and could be pressed out abundantly from the renal papillæ. It looked like pus. On microscopic examination, however, it seemed to contain merely a very large quantity of epithelial cells, and no pus-globules. Was this inflammatory? There was no effused fibrin or coagulable lymph.

LESIONS OF THE NERVOUS SYSTEM, ETC., IN THE PARTURIENT FEMALE, CONNECTED WITH ALBUMINURIA.[1]

1. Albuminuria, when present during the last periods of pregnancy and labor, denotes a great and marked *tendency* to puerperal convulsions.

2. Albuminuria, in the pregnant and puerperal state, sometimes gives rise to other and more anomalous derangements of the nervous system without proceeding to convulsions; and I have especially

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[1] From Edinburgh Monthly Journal of Medical Science, Oct. 1847, p. 288.

observed states of local paralysis and neuralgia in the extremities, functional lesions of sight (amaurosis, &c.) and hearing; hemiplegia and paraplegia more or less fully developed.

3. Œdema of the face and hands, going on occasionally to general anasarca, is one of the most frequent results of albuminuria in the pregnant female.

4. The presence of this œdema (3), or of any of the lesions of the nervous system (2), with or without the œdema, should always make us suspect albuminuria; and, if our suspicions are verified by the state of the urine, we should diligently guard, by antiphlogistic means, &c., against the supervention of puerperal convulsions.

5. Albuminuria, and its effects (1, 2, 3), are far more common in first than in later labors, and these constitute a disease which in general disappears entirely after delivery. But I have seen one case commencing with slight blindness, but no œdema, and ending gradually in hemiplegia, where the palsy partially remained after delivery, and after the disappearance of the albuminuria. In another, amaurosis came on with delivery, and had been present for six months when I first saw her. She had no œdema or other symptom of albuminuria except the amaurosis; but, on testing the urine, it was highly albuminous.

6. Albuminuria, with convulsions, &c., occurring in any labor later than the first, generally results from fixed granular disease of the kidney, and does not disappear after delivery.

7. Perhaps in puerperal convulsions, &c., produced by albuminuria, the immediate pathological cause of the nervous lesions is some unascertained but poisoned state of the blood. Was there a morbid quantity of urea in the blood? In several specimens of the blood of patients suffering under severe puerperal convulsions, furnished by me to Dr. Christison and Dr. Douglas Maclagan, these gentlemen have been unable to detect any great traces of urea. Is the poisoning material, caseine in morbid quantity or quality? The dependence, shown by Gluge and others, of albuminuria upon stearosis of the kidney, makes this connection worthy perhaps of some inquiry.

8. In cases of severe puerperal convulsions, &c., from albuminuria, the renal secretion is in general greatly diminished, and I have found active diuretics apparently of great use along with or after venesection, antimony, &c., especially where the case was offering to become prolonged.

9. Sometimes hemiplegia supervenes during pregnancy and labor without albuminuria, but this form does not seem to interfere materially, or very dangerously, either with the pregnancy or labor—the disease running its own usual course. In more than one case

I have seen the patient gradually but imperfectly recover the use of the palsied limbs after delivery. In another no improvement occurred.

Dr. Simpson, on May 10, 1848, further remarked to the Obstetric Society,[1] that he had on a previous occasion attempted to show that one of the most common predisposing causes of puerperal convulsions was albuminuria, or rather some morbid state of the blood dependent upon albuminuria—a doctrine which he had taught for the last seven or eight years,[2] and which Dr. Lever[3] had studied and successfully illustrated. He believed this diseased condition of the blood to produce a preternatural excess of irritability or polarity of the nervous system, and more especially of the spinal system of nerves; and in this way a morbid predisposition in it to convulsions under various forms and degrees, from irritation of the uterus, or bladder, or intestinal canal, &c., that would be incapable of producing this convulsive effect were not the polarity of the nervous system for the time being highly exaggerated. Dr. Todd of London had shown last year, that, when the polarity of the nervous system was exaggerated in the lower animals by the exhibition of strychnia, the convulsive tendencies resulting from the strychnia could be afterwards so far subdued and decreased by making the animals inhale ether. The ether reduced the excessive polarity and consequent convulsions. And the application of chloroform in the same way, in puerperal convulsions, had been tried with success by Dr. Wilson of Glasgow, Mr. Fearn, &c. &c.

COMPLICATION OF LABOR BY LARGE FIBROUS TUMORS.[4]

PROPRIETY OF INDUCING PREMATURE LABOR.

(From Edinburgh Monthly Journal of Medical Science, August, 1847, p. 138.)

Dr. Simpson stated that it was well known that fibrous tumors were very common organic changes in the unimpregnated uterus. He had seen several cases in which they were still small, and showed themselves in the form of nodulated irregularities on the surface of the pregnant and puerperal uterus. He had seen also a number of instances of pregnancy and parturition complicated with large tumors of this kind; and he mentioned the particulars of the three following cases in consequence of unusual circumstances connected with each.

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[1] See Edinburgh Monthly Journal of Medical Science, Sept. 1848, p. 197.

[2] Ibid., Nov. 1843, foot-note, p. 1015.

[3] Guy's Hospital Reports, vol. vii. p. 325.

[4] Extracted from Proceedings of Obstetric Society, May 12, 1847.

CASE I.—A patient, after being married for 10 or 12 years without any issue, passed two menstrual periods. A very large pelvic tumor, which had long been present, began to increase in size. He then saw her with Mr. Dixon and Dr. Taylor. There was a very large, hard, fibrous tumor of the uterus, and low on the left side and front, a soft elastic part, having much the character of a dropsical ovary. It is well known, that fibrous tumors of the uterus and multilocular dropsy of the ovary sometimes coexist; but he knew no other kind of cystic collection ever existing along with fibrous uterine tumors. In this case, however, the soft fluctuating part was not the ovary, for it was situated *anteriorly* to the hard uterine tumor, whilst diseased ovaries lie *posteriorly* to the uterus. This led him to suspect pregnancy, improbable as it otherwise was, and to hazard a diagnosis to that effect. It proved correct. The fœtal heart was heard about the fifth month, and pregnancy went on to the full time. During labor, one portion of the tumor filled up so much of the brim of the pelvis, that the child required to be extracted by turning. It was still-born. The mother made a good recovery.

CASE II.—In a woman suffering from dysuria, &c., and who had not menstruated for three months, he found the uterus retroverted, and an enormous fibrous tumor in the walls of the organ. The case was at this time seen by Dr. Renton, Dr. Ziegler, and others. The uterus was replaced with difficulty, and some weeks subsequently the fœtus was expelled. About a year afterwards, the same patient again became pregnant and went to the full time. The labor was extremely tedious—a portion of the tumor diminishing the brim of the pelvis—and at last the child was expelled by the spontaneous efforts of the uterus, but with the head greatly compressed and flattened. The mother recovered rapidly. In the unimpregnated state, the fibrous tumor, in this instance reached to the umbilicus and was as large as a uterus at the fifth or sixth month of pregnancy. The uterine cavity was found by the uterine sound to be six inches in length.

CASE III.—A patient, pregnant for the first time, who had long been delicate, arrived at the full term of utero-gestation, and after a somewhat tedious labor, was delivered of a dead child. There was a slight degree of hemorrhage, but it was easily arrested. From the time, however, of delivery onwards, the patient continued to sink

—became faint and listless, and then comatose—and died in this state five or six hours subsequently. She seemed never to rally from the "shock" accompanying delivery. Dr. Malcolm saw her with him. On opening the body, they found the uterine parietes thickly studded with fibrous tumors, and counted as many as forty hanging in a more or less pediculated form, from its external or peritoneal surface; and of all sizes, from an orange downwards. These were easily diagnosticated through the abdominal parietes during her life. The cavity of the uterus contained no collection of blood.

Case IV.—A lady pregnant for the first time, was supposed by her medical attendant to have an extra-uterine fœtation from her unusual shape. There was a fibrous tumor projecting from the anterior wall of the uterus. She died of peritonitis on the second week after her confinement. Dr. S. found the fibrous tumor adhering firmly to the peritoneum lining the anterior walls of the abdomen, while its pedicular attachment to the uterus had stretched and lacerated during the involution of the organ after delivery.

In addition, Dr. Simpson alluded to two cases of large fibrous tumors complicating labor, published in the Dublin Medical Journal, the one by Dr. Montgomery, the other by Dr. Beatty. In the former, the fibrous tumor, having entered the pelvis, formed such a complete and perfect obstruction to the maternal passages, that the Cæsarean section was required. In Dr. Beatty's case, it was supposed, before the commencement of parturition, that the same proceeding would be necessary; but, in the course of the labor, the tumor was gradually raised out of the pelvis by the uterine contractions, and the child spontaneously expelled.

WHAT PRACTICE SHOULD BE FOLLOWED IN CASES OF LARGE FIBROUS TUMORS COMPLICATING PREGNANCY?

Dr. Simpson adduced the opinion of Dr. Ashwell, who, in discoursing on this subject, inculcates the propriety of inducing premature labor, in order to evade the danger of inflammation of the pelvic tissues and peritoneum, and the still more hazardous evils of unhealthy softening, suppuration, and ulceration of the tumors themselves. But Dr. Simpson stated that he entertained very serious doubts of the correctness of Dr. Ashwell's observation, that fibrous tumors had a tendency to soften during the latter months more than at any other period of pregnancy; and he disapproved of the induction of premature labor as a general rule of treatment in such cases, believing, that the excitement of the uterus by artificial means to the premature expulsion of its contents, would be as likely to induce such anticipated morbid actions, as the supervention and completion of a natural pregnancy and labor. He believed, that the only cases of this kind which demanded the induction of premature labor were those in which the tumor encroached, as in two of the preceding cases, upon the brim or cavity of the pelvis, and thus produced such mechanical contraction of the maternal passages as rendered a natural labor impossible. Where the tumors were, as was usually the fact, àbdominal and above the brim, he did not consider premature labor in any way called for, or a practice that ought to be followed.

ON THE INDUCTION OF PREMATURE LABOR.[1]

MODES OF EFFECTING IT.

A VARIETY of means or plans have been proposed for the artificial induction of premature labor, in those various and important complications which are now so generally recognized by the obstetric profession as demanding this mode of operative interference.

Thus it has been attempted to excite the uterus into parturient action—1. By external abdominal frictions, so as to irritate its outer surface; 2. By passing currents of electricity or galvanism through its walls; 3. By irritating other, and even distant, parts or surfaces, as the vagina, rectum, or nipple, that are known to possess a marked reflex power over the contractility of the uterus; 4. By the internal exhibition of ergot of rye and other oxytoxic remedies; 5. By the evacuation of the liquor amnii; 6. By the dilatation of the os uteri; and, 7. By the separation of the membranes from the cavity of the cervix or body of the uterus by the finger, by instruments or sponges, or by the injection of fluids.

The first three of these modes of inducing premature labor are—alone and singly—so very uncertain in their results, and so generally and entirely fail, that few or no accoucheurs place any confidence in them;[2] and to the fourth the same objection applies, with this addition, that the ergot, even when it has succeeded, has proved too dangerous in its effects upon the child to be used in an operative procedure, instituted, as this usually is, for the very purpose of saving the infant.

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[1] See Proceedings of Obstetric Society for 1844, 1845, 1851, &c.

[2] Several years ago I attended a case with Dr. Thatcher, in which we applied a child to the breast with the object of exciting pains. Some hours before, I had introduced a large sponge-tent into the os uteri. There was a wet nurse in attendance to suckle our patient's infant as soon as it was born. It was the nurse's child which we applied to the nipples; and, as she thought, with the effect of increasing the uterine contractions and pains, which had already begun to appear. I have never, however, seen such an application of an infant to the nipples *originate* uterine contractions, nor in the two or three cases in which I have tried the plan of Schoeller and Braun, of distending, and consequently irritating the walls of the vagina with masses of sponge or a dilating caoutchouc bottle, have I been at all successful in exciting the uterus to parturient action. I have not seen the abdominal frictions of D'Outrepont and Ulsamer tried.—On Galvanism, see p. 336.

The fifth mode which we have enumerated above, viz., the evacuation of the liquor amnii, is, of all the methods proposed, both the oldest and assuredly the most sure and fixed in its effects. But, as a common means, and when labor is induced to save the infant, it is liable to one strong objection, viz., that it is undoubtedly much more dangerous to the child than the employment of operative procedures, which—as the dilatation of the os, or the separation of the membranes—allow the bag of membranes to remain entire, and thus keep the fragile and premature infant protected by the amniotic fluid during the progress of the labor, or at least, during the earlier stages of it.

In by far the greater number of instances in which I have had occasion to induce premature labor in private and consultation practice, I have always, in the first instance, avoided the artificial evacuation of the liquor amnii, and have proceeded upon the principle either—I. Of dilating the cervix uteri, or, II. Of separating the membranes; or rather I have acted upon both of these plans conjointly, for it is difficult or impossible to follow out thoroughly the one indication without, in some respect at least, following out the other also.

I. DILATATION OF THE OS AND CERVIX UTERI.

In exciting premature labor upon this principle, accoucheurs have used three different means—1. The finger; 2. Metallic dilating forceps and instruments; and 3. Sponge-tents. To stretch, however, and open the os uteri by the finger or by metallic dilators is a process so irritating and painful, that few or no practitioners now use it; especially as the same object can be effected more easily and safely by the introduction of compressed sponge.

Sponge-tents were first proposed as a means of inducing premature labor by Kluge and Brunninghausen; and they have been much employed for the purpose both in Germany and France. All the continental accounts, however, of their employment, up even to the present day, describe the introduction of the tents into the os uteri as a complicated operation, requiring always the aid of the speculum, and the use of a vaginal tampon, or other means, to keep the tent in situ. But there is no necessity whatever for such formidable arrangements. In 1844, when first mentioning the induction of premature labor in this country by sponge-tents,[1] I attempted to show that they could be easily introduced and employed without any vaginal speculum or tampon, or in the simple mode already described in a preceding paper on Intra-Uterine Polypi (see p. 126). And for several years subsequent to that date, I had recourse to this mode of inducing premature labor in a long series of cases; always with perfect success as regarded the mother, and in a large proportion of cases with safety also as regarded the child.

I never found this means fail, although in a few instances, I have seen the dilatation effected to the size of a half-crown or more, for thirty or forty hours before true uterine contractions set in. Generally, however, parturient action began long before the dilatation of the os uteri had reached these dimensions; and when it did so, a considerable part of the first stage of labor was thus, as it were, found finished before actual labor commenced. Sometimes uterine pains and contractions began as early as four or six hours after the sponge-tent was introduced, especially if the tent were of considerable size, and means were used for its rapid development. In almost every case, the first tent employed may be as thick as the little finger; and the patient should be directed to have injected into the vagina every hour or two, a small quantity of warm water for the imbibition and expansion of the compressed sponge. She should lie on the back during, and for some time after, each injection, in order that the water may be more thoroughly retained. After the first sponge is fully dilated, it may be withdrawn, and a second and larger one introduced; or, without removing the first, tents of a greater and greater size may be introduced at intervals of six or eight hours, till the os uteri is thoroughly dilated or labor supervenes.

To the induction of premature labor by the use of sponge-tents, I have heard some accoucheurs object, on the ground that, from want of practice, they have had difficulty in introducing the compressed sponge into the os uteri. A much more important drawback to the method, will be found in the circumstance, that the presence of a large sponge-tent in the canals of the cervix uteri and vagina, sometimes, as a foreign body, produces such a degree of local uneasiness and irritation, as to inflict no small amount of discomfort and continuous pain upon the patient. It is principally on this account, and to avoid this difficulty, that, of late years, I have in my own practice commonly brought on premature labor by the other means already alluded to, namely, the detachment of the membranes—a process not requiring the permanent retention of any material in the maternal passages, and capable of being effected with probably less difficulty and trouble to both practitioner and patient.

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[1] See Edinburgh Monthly Journal of Medical Science for Aug., 1844, p. 735; and Feb. 1845, p. 122.

II. SEPARATION OF THE MEMBRANES.

In the induction of premature labor, the membranes of the ovum have been proposed to be mechanically separated from the interior of the uterus, by different means, to different degrees, and in different localities.

The idea that the partial artificial separation of the membranes would lead on to labor occurred first to the late Professor Hamilton; and he was himself the first also to put it in practice as far back as 1795.

Dr. Hamilton's method, by the Finger, &c.—In operating, he detached "a portion of the decidua from the cervix uteri,"[1] by the introduction, first, of his finger, and ultimately of a bent brass wire. His friend, Dr. Burns, describes Dr. Hamilton's operation as consisting of "insinuating a finger within the os uteri, and gently dilating it, and detaching a part of the membranes from the portion of the cervix in its immediate vicinity. If," he continues, "we have not thought it prudent to dilate at once the os uteri, so as to admit the finger freely to touch the membranes, we may repeat the dilatation gently at the end of a few hours, and then detach the membranes cautiously from the cervix uteri by the finger to the extent perhaps of two inches. But for this purpose," Dr. Burns adds, "it may be necessary, if the os uteri be high, to have the *hand* introduced into the vagina; or sometimes the detachment has been accomplished with a catheter or other small instrument."[2] As thus pursued, this mode of inducing labor by separating the membranes from the cervix, was not always unaccompanied with pain, particularly when the fingers, and especially the hand, were introduced; it was often very tedious, and sometimes it failed, as Dr. Hamilton himself states, and the operation required to be completed by puncture of the membranes and evacuation of the liquor amnii.

Dr. Kiwisch's method, by injection of water.—In 1846, Professor Kiwisch proposed to bring on premature labor, by injecting a stream of tepid water into the vagina, and against the cervix and os uteri.[3] His apparatus, as delineated by Scanzoni,[4] consists of a small, square tin box or reservoir of water, fastened to the wall at the height of nine or ten feet, and from the bottom of this reservoir, a tube hangs down, the end of which is, when required, introduced into the vagina, so as to allow a strong continuous stream to pour through it, against the cervical portion of the uterus.

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[1] Practical Observations on Midwifery, p. 285.

[2] Principles of Midwifery, p. 503.

[3] In a chapter on the induction of premature labor, Schweighauser in 1835 speaks of, but does not venture to recommend us—"de provoquer l'accouchment prematuré au moyen d'injections pour decoller les membranes de la matrice."—La Pratique des Accouchemens, p. 274.

[4] Lehrbuch der Geburtshilfe, vol. iii. p. 54.

The douching or injection was recommended to be repeated morning and night, and commonly labor supervened on the fourth or fifth day.

This plan of Dr. Kiwisch's was shortly afterwards tried successfully in Vienna, Berlin, &c., by various continental practitioners. In April, 1851, I described a case to the Edinburgh Obstetric Society, in which I used this method.[1] It was an instance where the patient had repeatedly found the child to die a short time after quickening, and retained it for six or eight weeks subsequently. During her last pregnancy, the same occurrence took place with the same symptoms. A few weeks having elapsed, she threw up tepid water at my request, twice a day, with the view of bringing off the dead fœtus. After nine douches, applied night and morning with a common syringe, expulsive pains came on, and a dead and shrivelled fœtus and placenta were expelled. In the course of that and the subsequent years, I had various opportunities of bringing on premature labor by the same means, and, as I always found, with almost perfect certainty as to the power of its induction.

Professor Kiwisch imagined that the vaginal water injection induced labor by the imbibition of the fluid relaxing the soft parts. The flow of a gentle and small stream of water into the vagina ought, if this were the true principle, to act as well as a stronger current. But a short experience convinced me that this was not the fact; and it soon became evident—1. That the water douche was liable to fail, unless the injected fluid accumulated and distended the vagina, so as to expand that canal and enter the os uteri; and 2. It seemed the more rapid and certain in its action, in proportion as it entered freely into the uterine cavity itself, and in proportion, therefore, as it separated more of the surface of the fœtal membranes from the interior of that cavity.

In only two or three cases did I try an elevated box and syphon tube, like that originally suggested by Kiwisch. From the first, I found a common enema syringe a far better and more manageable apparatus. Usually I have employed the India-rubber syringe of Dr. Kennedy, or that of Mr. Higginson. At first I merely injected and distended the vagina, retaining the fluid in it by closing the vulva with pressure of the fingers or hand, and thus forcing the water to pass upwards through the os into the uterine cavity; but

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[1] See Monthly Journal of Medical Science for July, 1851, pp. 88, 492, &c.

I soon found it a simpler and more direct plan to introduce the end of the syringe through the uterine orifice, and thus send the stream directly into the interior of the uterus, without unnecessarily distending the vaginal canal. In most cases it is easy to pass for this purpose the common ivory nozzle of the enema syringe through the os uteri; but when that opening is placed very high, or far backwards, I have found that the addition of a longish gum-elastic pipe or bent silver catheter to the nozzle of the tube greatly facilitates the requisite introduction of the instrument through the os and upwards for an inch or two, between the membranes and the anterior or posterior wall of the uterus.

While the practitioner is using the syringe and injecting the fluid, the patient should lie on her left side, and with the pelvis placed near the edge of the bed or sofa which she is occupying. A basin properly placed immediately below, both contains the water to be used, and receives it again after it re-escapes from the vulva. The tubes of the catheter and syringe should be carefully filled with the water before commencing the injection, lest a quantity of air be thrown into the uterine cavity. Usually the injection is carried to the extent of the patient complaining of a feeling of distension or fulness; and it may be repeated twice a day, or oftener, according as it is an object or not to expedite as much as possible the supervention of labor.

It was not till I had used this method for a considerable time, and in a number of cases, that I discovered that a similar method had been suggested and described by Dr. Cohen of Hamburg.[1]

In several cases where the child was placed with the head over the os uteri, I have found it change its position as the water injection proceeded, and an upper or lower extremity to present. Occasionally this preternatural presentation has remained; but more frequently the child has again rotated, and the head again become replaced over the uterine orifice. In no case have I seen any great amount of hemorrhage from partial separation of the placenta. But the repetition of the injection sometimes becomes irksome to the mother as well as to the accoucheur.

Detachment of the membranes, by the uterine Sound, from a portion of the body of the uterus.—Believing that labor was, at the ninth month, induced naturally through the degeneration and loosening of the decidua (see p. 316), I was encouraged last year to try to induce it artificially by the mechanical separation of a portion of the membranes from the interior of the body of the uterus.

In general the stethoscope sufficiently certifies to us the locality of the placenta, and what part or side of the uterus we ought consequently to avoid;[1] and nothing in the way of an operation could possibly be more simple or more easy and painless than the introduction of a sound, through the dilatable os, and upwards for five or six inches, between the membranes and the anterior wall of the pregnant uterus.

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[1] See Scanzoni's Lehrbuch, vol. iii. p. 53.

In the first case in which I tried this plan, the patient, after having been always delivered in the country by craniotomy, has thrice had premature labor induced under my care. Her three children are alive. On the first occasion, in 1851, she had an apparatus upon the plan of Kiwisch's erected; but it required to be used, and that frequently, for five or six days before labor supervened. On the second occasion, I injected a quantity of tepid water by an enema syringe into the uterine cavity, and the child was born in about twenty-four hours afterwards. Last year, on the third occasion, I saw her late at night along with my friend Dr. Ziegler, and passed a uterine bougie for five or six inches upwards between the membranes and the anterior wall of the uterus. The child was born before noon next day. At the time of passing the bougie, the patient herself was not aware that anything special had been done, but believed that I was merely making a common digital examination, in order to ascertain the exact stage of pregnancy, &c.; and she subsequently declared, that, in her experience, this last method was too simple to be capable of being compared with the two other methods to which she had been formerly subjected. But in all cases, a single introduction of the bougie will by no means suffice. Like the tents and douching, it requires in most instances to be repeated more than once. During the past three months of the present year, I have induced labor six or seven times by this method. In one case, in my own private practice, and in another under the care of Dr. Scott of Musselburgh, the labor was terminated within eighteen hours. In the others, parturition did not come on till the second or third day after the act of separation. In a case which I saw with Dr. Thomson, he used a water injection next day, and on the subsequent day I again separated the membranes with the bougie. Parturient action began that night. In a previous labor of this woman, the child was rotated, and made to present preternaturally by the employment of the water injection. All the children have been born alive in the ten or twelve cases in which I have induced premature labor by the uterine sound.

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[1] In injecting water we have no control on the *direction* it will take in the uterine cavity, while we can regulate perfectly that of the Sound. In one case, from inattention to the uterine souffle, I probably separated the edge of the placenta, as a clot was found at that spot. The child was born alive; and the mother recovered perfectly. But with due caution such an accident should be easily avoided.

The relative degree of facility or difficulty with which labor is induced artificially in different women, or even in the same woman in different pregnancies, varies very greatly. Where one plan fails, the addition of a second, or of a third method, will sometimes enable us to succeed; and if all modes less safe for the child prove ineffectual, as the separation of the membranes with a uterine bougie, the water injection, and the sponge-tent, we may always at last determine the certain occurrence of uterine contraction by the puncture of the membranes. And if we have recourse to this puncture, we may still in a great measure save the liquor amnii for the protection of the child during labor by making the seat of the opening oblique and as high as four or five inches above the os, as recommended by Hamilton and Meissner. One of the best instruments for effecting this object is that long ago recommended by Dr. Hamilton, viz., a male catheter having an open or truncated extremity, and provided with a silver wire to pass through it for the puncture of the membranes. The membranes, I believe, will sometimes be found to rupture high up when and where they are simply separated from the body of the uterus by the introduction of the knobbed uterine sound or bougie.—(April, 1855.)

REPORT OF EDINBURGH ROYAL MATERNITY HOSPITAL

FROM 1844 TO 1846.[1]

(From Edinburgh Monthly Journal of Medical Science, November, 1848, p. 329.)

The Edinburgh Maternity Hospital was opened in St. John's Street in May, 1844, and continued in this locality till May, 1846, when the Charity was removed to Milton House, Canongate. The following Report comprehends an analysis of the obstetric practice of the Institution during the two years in which it was located in St. John Street. The Report was drawn up and communicated to the Medico-Chirurgical Society twelve or eighteen months ago; and its publication was for a time postponed, under the hope that I might find leisure to render it still more minute and extensive. As, however, I have little prospect of fulfilling that wish, I now publish it; and I trust, that the continuation of the reports of the Institution will, betimes, be drawn up and communicated to the Society by some of the younger and more active medical officers attached to the hospital.

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[1] Extracted from the Proceedings of the Edinburgh Obstetric Society, July 12, 1848.

At the time the hospital was opened, the directors appointed to it the following medical staff: Drs. Campbell and Beilby, consulting physicians; Dr. Pagan, consulting surgeon; Drs. Moir and Simpson, ordinary physicians; Dr. Ziegler, ordinary surgeon; and Drs. Bell, Thomson, Niven, and Carmichael, assistant medical officers. One or two advanced pupils were appointed to live in the house as resident house-surgeons.[1]

ANALYSIS OF THE CASES.

I. *The Number of Cases.*—Of women delivered in hospital, there were 374. Of women delivered at their own homes in various parts of the town, there were 1101; making a total of 1475 women delivered under the superintendence of the Institution. Among these are included 58 cases of miscarriage or premature labor.

II. *The Ages of the Women.*—In 1457 of the cases, the ages of the women were noted. In 18 cases the ascertaining of this point was omitted. The following table exhibits, in detail, the number of patients delivered, with their respective ages:

Ages of the Women.	Number of Women.	Ages of the Women.	Number of Women.
16	4	33	47
17	10	34	40
18	30	35	49
19	57	36	46
20	64	37	23
21	75	38	33
22	101	39	18
23	90	40	37
24	84	41	8
25	77	42	11
26	107	43	9
27	69	44	6
28	94	45	2
29	69	46	2
30	107	47	1
31	34	48	1
32	52		
Total, 1457.			

III. *The Quickening.*—The average period of quickening, as calculated from the intern cases where it was noted, was 183 days after the disappearance of the last catamenia.

IV. *The Duration of Pregnancy.*—In the intern cases where the data are afforded, the average duration of pregnancy, calculating from the end of the last catamenia, was 273 days. But on the preceding computation little confidence can be placed, considering the class of patients from which the data are drawn.

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[1] To one of these gentlemen, Dr. Martin Barry, the Institution was specially indebted for his valuable and unwearied efforts, and for characteristic accuracy in the statistical returns of the cases, &c.

V. *The Number of Pregnancy.*—In 1459 cases, the number of the pregnancy is stated, namely, whether it is the first, second, or third time, &c., the woman has been pregnant; in 16 cases it is omitted. 398 women were delivered of their first children; and thus the frequency of first deliveries was to that of all subsequent deliveries in the proportion of about 1 to 4. The following table exhibits at length the number of cases, with the corresponding number of the pregnancy in each.

Number of the Pregnancy.	Number of Women.	Number of the Pregnancy.	Number of Women.
1	398	10	38
2	294	11	24
3	161	12	11
4	158	13	7
5	116	14	2
6	88	15	1
7	65	16	1
8	57	17	1
9	36	20	1
Total, 1459.			

VI. *The Number of Children.*—The children resulting from the 1417 labors, at or near the full time, were 1436 in number. Of these there were born

In single births,	1399	children.
In twin births,	34	"
In triplet birth,	3	"
Total,	1436	

VII. *The Sex and State of the Children.*—The children are further to be distributed as follows:

Sex of the Child.	Number of Children.	Number Born Alive.	Number Born Dead.	Proportion of Still-births.
Male,	739	682	57, or	1 in every 13
Female,	693	650	43, or	1 in every 16
Sex not stated,	4	3	1	
Total,	1436	1335	101	1 in every 14

Excluding from the above list of still-born children 37 which were putrid, there remain 64 whose death was recent. Of these 64, there were born

Under cranial presentations,	40
" breech presentations,	8
" footling presentations,	4
" arm presentation,	1
" shoulder presentation,	1
" presentation or prolapse of the cord,	8
" placental presentation,	1
Presentation not stated,	1
Total,	64

In one of the cases of still-birth under cranial presentation, the mother had convulsions during labor; and, in another, the child was a male twin; in a third, the cord was twisted twice around the neck; in a fourth, the child exhibited marks of abdominal disease; and in a fifth, the delivery was completed by perforation. Six of the dead children were born in labors complicated with hemorrhage during or before labor.

Of the 37 children born putrid, 18 were male and 19 female infants; and the presentations were as follows:

The head presented in	26 cases.
" breech	6 "
" feet	1 "
" shoulder	1 "
Presentation not stated in	3 "
Total, . .	37

During pregnancy two of the mothers whose children were born putrid, labored under syphilitic complaints; and a third was affected with ascites and anasarca. In several of the still-born children which I took the opportunity of examining along with the pupils, effusion of coagulable lymph (the consequence of extensive inflammation) was found covering various parts of the peritoneal membrane.

VIII. *The Length of the Children.*—The extreme length or height of the child at birth is stated in 330 cases of single birth completed in the hospital. 169 of these children were male, and 161 female.

Of the male children there measured in length—

Under 15 inches,	1 child.	Under 20 inches,	53 children.
" 16 "	1 "	" 21 "	28 "
" 17 "	3 children.	" 22 "	21 "
" 18 "	14 "	" 23 "	3 "
" 19 "	45 "	Total, . .	169

Of the female children there measured in length—

Under 15 inches,	1 child.	Under 20 inches,	40 children.
“ 16 “	1 “	“ 21 “	32 “
“ 17 “	1 “	“ 22 “	13 “
“ 18 “	10 children.	“ 23 “	2 “
“ 19 “	60 “	“ 24 “	1 child.
			161

Average length of male child, 19½ inches.
“ female child, 18¾ “
“ both sexes 19⅛ “

IX. *The Weight of the Children.*—The following table gives in detail the respective weights of 337 children delivered within the hospital, including 171 males and 166 females. The children born in plural births are excluded from the table.

Of the male children there were:

Under 4 lbs.,	4 children.	Under 8 lbs.,	60 children.
“ 5 “	7 “	“ 9 “	31 “
“ 6 “	17 “	“ 10 “	3 “
“ 7 “	45 “	“ 11 “	4 “
		Total, . .	171

Of the female children there were:

Under 4 lbs.,	2 children.	Under 8 lbs.,	50 children.
“ 5 “	4 “	“ 9 “	27 “
“ 6 “	17 “	“ 10 “	7 “
“ 7 “	59 “		
		Total, . .	166

The lightest child (premature ?), a male, 3 lbs. 4 oz. in weight.
The heaviest child, a male, 10 lbs. 8 oz.
The heaviest female child, 9 lbs. 7 oz.
The average male child, 7 lbs. 3 drs.
The average female child, 6 lbs. 10 oz. 11 drs.
The average of both sexes, 6 lbs. 13 oz. 7 drs.

X. *The Length of the Umbilical Cord.*—In 327 cases of children delivered within the hospital, the length of the cord was noted. Among these—

The shortest cord measured 12 inches in length.
The longest “ 43 “
The average “ 23½ “

In a note attached to the details of an extern case the cord is stated to have been 50 inches long.

XI. *Weight of the Placenta.*—In 325 of the cases delivered within the hospital, the weight of the placenta is noted.

The lightest placenta weighed 7 oz. Premature child?
The heaviest ditto, 2 lbs. 15 oz. The single placenta of twin children.
The average ditto, 1 lb. 4. oz. 14 drs.

XII. *The Duration of Labor.*—The duration of labor from the first commencement of pains was ascertained in 311 cases, all delivered within the hospital. The following table exhibits the average length of the whole labor, and of the first, second, and third stages separately in these 311 cases:

Average duration of first stage of labor, 11 hours 50 minutes.
" " second " " 1 " 37 "
" " third " " 0 " 38 "
" " whole labor " 12 " 57 "

The following table exhibits in detail the duration of the whole labor, and of each stage of labor respectively, in all the cases. It reads thus:—The whole labor was completed in 1 hour in 4 cases; in 2 hours in 4 cases; and so on. The first stage of labor was completed in 1 hour in 3 cases; in 35 hours in 5 cases, &c.

Duration in Hours.	Whole Labor.	First Stage.	Second Stage.
1	in 4 cases.	in 3 cases.	in 161 cases.
2	4	9	65
3	7	11	20
4	16	20	16
5	17	26	10
6	16	17	2
7	28	18	4
8	21	18	1
9	17	16	2
10	20	16	1
11	20	15	4
12	12	16	0
13	23	9	0
14	14	9	1
15	8	5	0
16	6	6	0
17	6	3	0
18	8	5	0
19	10	6	0
20	3	7	0
25	22	14	1
30	12	8	0
35	5	5	0
Above 36	14	9	0
Duration not stated,	61	85	103

The third stage was completed in 5 minutes in 40 cases.
" " " 10 " 88 "
" " " 15 " 80 "
" " " 30 " 104 "
" " " 1 hour 16 "

Third stage completed within 1 hour in 328 "

Third stage completed within 1½ hours in	7 cases,
" " " 2 "	1 "
Duration not stated in	38* "

XIII. *The modes in which the Children presented.*—In 1421 of the births the presentation of the child is given. The following table exhibits in detail the various presentations, and the number of each respectively in 1421 cases:

Presentation.	Number of Cases.
Head, ...	1333
A hand descending with the head, ...	31
Both hands descending with the head, . . .	1
Face, ...	6
Shoulder, ...	3
Arm, ...	3
Breech, ...	27
Breech and feet, ...	2
One or both feet, ...	15
Total, . . .	1421

The cord presented along with some part of the fœtus in 5 cases.

The cord became prolapsed in 13 cases.

The placenta presented partially, along with some part of the infant, in 3 cases.

In the preceding table of presentations I have entirely omitted to classify those of the cranium, according to the different positions in which the head may present; because I do not place much reliance upon the perfect accuracy of the reports of positions entered in the hospital records by the younger pupils. In fact, one of their principal objects in hospital and dispensary instruction is to acquire, among other things, a practical knowledge of the somewhat difficult subject of positions. But I have great pleasure and confidence in citing the notes on this subject of Dr. Martin Barry, who acted for sixteen months as House-Surgeon to the hospital, more especially as the data which he has obtained entirely coincide and agree with the results of my own observations. Dr. Barry himself carefully

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[*] In reference to these calculations regarding the duration of labor, it is necessary to bear in mind that, in collecting the data for them, Dr. Martin Barry and the House-Surgeons reckoned labor as commencing with the first periodic pains felt by the patient. In other hospital returns the date of the commencement of labor is calculated differently, and consequently very different averages of actual duration, &c., are given. The average duration of labor in the Arran-Quay Hospital of Dublin, as given by Dr. Churchill, is about eighteen hours, while in the Rotunda Hospital of Dublin, and consequently in the very same community, the average duration of the process, as given by Dr. Collins, is less than five hours—Dr. Collins reckoned labor as only commencing when the os uteri was ascertained to be opening under the uterine contractions; and in some of his cases, the duration of the process seems counted as beginning from the hour the patient entered the hospital, although she had been previously for a considerable time suffering under the pains of parturition.

observed and noted the position of the head in 335 cases of cranial presentation, among the patients of the Institution. I shall classify these 335 cases according to the four positions and numerical nomenclature used by many of the German schools:

I. *Occipito-anterior Positions.*	
1st Position; or occiput directed to left foramen ovale, in	256 cases.
2d Position; or occiput directed to right foramen ovale, in . . .	1 "
II. *Occipito-posterior Positions.*	
3d Position; or occiput directed to right sacro-iliac synchondrosis, in	76 "
4th Position; or occiput directed to left sacro-iliac synchondrosis, in	2 "
Total, . . .	335

In the cases of occipito-anterior position the head was invariably found to descend and emerge, without any change or movement, from the same oblique diameter of the pelvis as it was originally placed in, with one exception. In this solitary case the head rotated from the first into the fourth position, in which it was born.

Out of 78 cases of occipito-posterior position carefully watched, in 75 the occiput turned forwards, and emerged first from under the arch of the pubis. In 2 cases of the third position the occiput continued directed backwards, and the child was expelled without the usual rotation into the first position. Thus the exceptions to the rotation did not occur oftener than about once in thirty cases.

Of the two cases which presented in the fourth position, in one the rotation was performed into the first position. In the other, the head continued to maintain the same position till it was born.

XIV. *The Plural Births.*—Among the 1417 women delivered, 17, or 1 in every 83, gave birth to twin children, and in one case triplets were born.

In the *twin cases* the presentations and state of the children were as shown in the following table:

Presentation.	Number of Cases.	Number of Children Born Alive.	Number of Children Born Dead.
Double cranial presentation,	8	13	3
1st child, head; 2d child, breech,	6	11	1
1st child, feet; 2d child, head,	1	2	
1 child, arm; other not stated,	1	2	
1 child, head; other not stated,	1	2	
Total,	17 cases or 34 children.	30	4

The sexes of the twin children were as follows:

In 3 cases, both children male, . . .	6
In 5 cases, " " female, . . .	10
In 9 cases, 1 child male and 1 female,	18
Total, 17	34

The average weight of the twin child was 6 lbs. 1 oz.

In the *triplet* case, two of the children presented the head, the third presented the feet. Two of the children were male, one female. All were born alive and vigorous, but, before leaving the hospital, one of the children was accidentally overlaid by its mother. The male children weighed respectively, 5 lbs. 11 oz. and 5 lbs. 2 oz.; the female child weighed only 3 lbs. 12 oz. The labor was 8½ hours in duration.

XV. *The Mode of Delivery.*—Deducting the abortions and miscarriages, which amounted in number to 58, there remain 1417 cases of delivery at about the full time. The following table shows the mode of delivery in these 1417 cases, and their relative frequency to each other. Among those naturally delivered, is included a case of spontaneous cephalic evolution or expulsion:

Mode of Delivery.	Number of Cases.	Proportion.
Naturally, ...	1404	1404 in 1417
Artificially, ...	13	1 in 107
Mode of Artificial Delivery.		
Traction of presenting feet, ...	1	1 in 1417
Bringing down a foot (breech presentation), . .	1	1 in 1417
Turning, ...	6	1 in 236
Forceps, ...	3	1 in 472
Crotchet, ...	1	1 in 1417
Induction of premature labor, ...	1	1 in 1417

Turning was adopted in five cases for preternatural presentations, and in one case for the purpose of hastening delivery under an attack of apoplectic convulsions. The breech presentation, in which a foot was brought down, was a case in which the presenting part did not pass the brim, notwithstanding strong and powerful pains of upwards of a day's duration.

XVI. *The Complicated Labors.*—The following table contains a list of the chief complications observed in the 1417 labors, with the respective numbers and proportion of each:

Hemorrhage from partial placenta prævia in . .	3 cases,	or 1	in every	472 cases.
Hemorrhage before labor, in	12 "	1	"	118 "
Hemorrhage during labor, in	4 "	1	"	354 "
Hemorrhage between the expulsion of the child and of the placenta in	16 "	1	"	87 "

Hemorrhage post-partum, in	24	cases, or	1	in every	59	cases.
Hemorrhage (indefinitely stated), in	8	"	1	"	177	"
Total cases of hemorrhage,[1]	67	"	1	"	21	"
Convulsions during labor, in	4	"	1	"	354	"
Prolapsus of the entire uterus to the outlet, in	2	"	1	"	708	"
Rigidity of the os uteri, in	11	"	1	"	128	"
Carcinoma of the os uteri, in	1	"	1	"	1417	"
Prolapsus of the anterior lip of the uterus, in	6	"	1	"	236	"
Expulsion of the entire ovum (membranes unbroken), in	1	"	1	"	1417	"
Prolapsus of the cord, in	18	"	1	"	79	"
Cord once or twice around the child's neck, in	158	"	1	"	9	"
Cord three times around the child's neck, in	4	"	1	"	354	"
Cord four times around the child's neck, in	2	"	1	"	708	"
Cord encircling the neck and one hand, in	1	"	1	"	1417	"
Cord around the neck and arm, in	4	"	1	"	354	"
Cord around the neck and body, in	2	"	1	"	708	"
Cord around the shoulder, in	2	"	1	"	708	"
Cord around the body, in	2	"	1	"	708	"
Cord around the arm, in	1	"	1	"	1417	"
Cord around the leg, in	2	"	1	"	708	"
Cord snapped across before delivery (12 inches long), in	1	"	1	"	1417	"
Amaurosis and delirium before delivery, in	1	"	1	"	1417	"
Gangrenous slough from the interior of the rectum, in	2	"	1	"	708	"

XVII. *The Instrumental Deliveries.*—Instruments were had recourse to in very few cases. The following table exhibits the number and proportion of the deliveries in the 1417 labors:

Instrumental delivery, in	4	cases, or	1	in every	354	cases.
Viz., Delivery by the forceps, in	3	"	1	"	472	"
Delivery by the crotchet, in	1	"	1	"	1417	"

Of the four mothers, one died eight days after delivery from puerperal fever. She had been in labor upwards of two days, and was ultimately delivered of a dead child by the short forceps. Of the children extracted by the forceps, two were still-born. In both cases the head had descended into the pelvis before the instruments were applied. In one of these two cases the hospital record states, that, from the state of the cord, the child appeared to have been dead for some time before delivery. It was born after above two days' labor. In the second case the woman had been upwards of twenty-four hours in labor. Convulsions came on an hour and a half before delivery, and returned several times. The child was of very large size, weighing 10 lbs. 2½ oz.

Delivery by the crotchet was resorted to, in consequence of the head of the child being arrested at the brim of a distorted pelvis.

The proportion of cases in which instrumental delivery was had recourse to in the Edinburgh Hospital, was smaller than in most other obstetric institutions. The following table will perhaps illustrate this point more fully than any more lengthened remarks. The first column of figures shows the absolute number of labor cases reported by thè different practitioners named; the second column shows the proportion of instances in each of the returns in which the practitioner resorted to delivery by instruments; and the third and fourth columns respectively show the comparative proportion in which the two modes of instrumental delivery (the forceps and crotchet) were employed by each practitioner. It will be seen how frequently the forceps were employed by some in comparison with craniotomy; how frequently craniotomy was employed by others in comparison with the forceps; and how very different the proportion of cases supposed to require instrumental delivery at all, is in the practices of different medical schools and institutions.

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[1] In the Hospital books, "hemorrhage" is frequently noted when there was a more than usual loss of blood, although it may have been in comparatively small quantity.

Table showing the Proportion of Instrumental Deliveries, and of Deliveries by the Forceps and by Craniotomy, in different Obstetric Institutions.

Name of Reporter.	Total Number of Labors Reported.	Proportion of Instrumental Deliveries.	Proportion of Deliveries by Forceps.	Proportion of Deliveries by Craniotomy.
Siebold—Berlin,	2,093	1 in 7	1 in 7	1 in 2093
Busch—Berlin,	2,056	1 " 11	1 " 12	1 " 342
Carus—Dresden,	2,549	1 " 13	1 " 14	1 " 283
Naegele—Heidelberg,	1,711	1 " 31	1 " 31	1 " 1711
Bland—Westminster,	1,897	1 " 95	1 " 158	1 " 237
Beatty—Dublin,	1,182	1 " 98	1 " 131	1 " 394
Collins—Dublin,	16,654	1 " 115	1 " 617	1 " 141
Churchill—Dublin,	1,640	1 " 117	1 " 546	1 " 149
Lever—London,	4,666	1 " 137	1 " 518	1 " 186
Böer—Vienna,	9,589	1 " 199	1 " 274	1 " 737
Lachapelle—Paris,	22,243	1 " 252	1 " 293	1 " 1854
Ramsbotham—London,	48,682	1 " 322	1 " 553	1 " 773
Simpson—Edinburgh,	1,417	1 " 354	1 " 472	1 " 1417

XVIII. *The Maternal Deaths.*—Among the 1475 women delivered under the superintendence of the Institution, eleven deaths occurred, or 1 in 134.[1] Of these, seven occurred among the cases delivered in the hospital, and four among the out cases.

Four of these eleven patients were in their first labors; three in their second; and the remaining, severally in their third, sixth, ninth, and twelfth.

The cause of death was—

Puerperal fever and inflammation, in	7 cases.
Puerperal convulsions, in	2 "
Chest disease, in	1 "
Fatal sinking, after incision of a carcinomatous os uteri, in . .	1 "
Total, ...	11 deaths.

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[1] An erroneous statement of Dr. Simpson's statistics regarding the mortality to the mother, as read before the Medico-Chirurgical Society, was published in the Edinburgh Monthly Journal of Medical Science for June, 1847, p. 934. The attention of the profession was then drawn by Dr. Collins to the apparent discrepancy, by a letter published in the Obstetric Record, 1848, p. 146. An acknowledgment of the error on behalf of the Editors appears in the Edinburgh Monthly Journal of Medical Science for January, 1849, p. 494; the fault in accuracy having been entirely that of their own reporter, and not of Dr. Simpson.—(Ed.)

The delivery was effected by the natural efforts in nine of the cases; by the forceps in one of the cases dying of puerperal fever; by turning in one of the cases dying of convulsions—the body, on dissection, presenting a large clot of effused blood in the brain, and a collection of pus in one of the kidneys. In the case where the os uteri was incised for carcinoma, the mother had been in labor for two or three days, and the pulse had become very rapid, &c., before the operation was adopted. The child was subsequently born in the course of four or five pains. But the pulse never rallied after delivery, and she died on the third day.

I have merely further to add, that the convalescence of the hospital patients was often interrupted by febrile and inflammatory attacks. The house, No. 3 St. John Street, used as an hospital, was, in addition to a sunk kitchen flat, three stories in height, with two rooms in the first and second flats, and three in the third; and it was much too small for the accommodation of the patients, and of the resident house-surgeons, matron, &c. The rooms themselves were low-roofed, and very imperfectly ventilated. Various plans were attempted to improve the ventilation, but not with much success. Whenever the hospital became in any degree crowded, fevers (or weeds), with more or less abdominal tenderness, appeared among the patients; and frequently the supervention of such attacks during convalescence became so common, as to constitute the rule rather than the exception to it. Every one acquainted with hospital practice, whether obstetric, surgical, or medical, is well aware of the great liability among the patients to febrile and inflammatory attacks whenever the wards are overcrowded; and in no practice is this more visible than in midwifery. Indeed, I believe there are few or no circumstances which would contribute more to save surgical and obstetric patients from phlebitic and other analogous disorders, than a total change in the present system of hospital practice.

I have often stated and taught, that if our present medical, surgical, and obstetric hospitals were changed from being crowded palaces, with a layer of sick in each flat, into villages or cottages, with one, or at most two patients in each room, a great saving of human life would be effected. And if the village were constructed of iron (as is now sometimes done for other purposes), instead of brick or stone, it could be taken down and rebuilt every few years; a matter apparently of much moment in hospital hygiene. Besides, the value of the material would not greatly deteriorate from use; the principal outlay would be in the first cost of it. It could be erected in any vacant space or spaces of ground, within or around a city, that chanced to be unoccupied; and, in cases of epidemics, the accommodation could always be at once and readily increased.

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